Technocenosis:
An Assessment of Efforts to Identify the Sources of Derelict Trawl Nets in the Northwestern Hawaiian Islands

A DISSERTATION
SUBMITTED TO THE FACULTY OF THE GRADUATE SCHOOL
OF THE UNIVERSITY OF MINNESOTA
BY

Nicholas Max Handler

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

ADVISOR: DR. BRUCE P. BRAUN

FEBRUARY, 2011

Acknowledgements

This work would never have been possible without the help and support of a great many other people. Its completion is the result of their collective efforts as much as my own. In particular, I would like to thank my advisor, Dr. Bruce Braun. The unerring accuracy of his critiques and the general wisdom of his advice have never ceased to amaze me. Even beyond his abilities as a thinker, however, this dissertation owes everything to his inestimable patience, his willingness to trust in my instincts and abilities, and above all his boundless enthusiasm for scholarship and for life. I would also like to extend my heartfelt gratitude to Dr. Kurt Kipfmueller, with whom I have engaged in long and productive discussions not only about ecological clustering techniques, but about almost every other aspect of biogeography as well. I could never have borne the long days and nights in the Social Sciences Building without his constant energy and humor. I might say the same about Katie Pratt, Justin Jacobson, and Danielle Meinhardt, who have offered their constant support, advice, and friendship to me over the past eight years, and have proved beyond a shadow of doubt that graduate school is a team sport. Insofar as I have managed to retain my sanity throughout this process, it is thanks largely to Dan Brazil and Bronwyn Collins, who have become not only irreplaceable friends, but also mentors of indescribable value over the past several years. I quite honestly could not have done it without them. I would also like to thank my parents and my brothers. How they have managed to remain so patient and supporting during the long dissertation process is something that mystifies me each and every day. In particular, I will never be able to thank Zach and Vanessa enough. They have shared their adventures, their dinner table, and their home with me so openly and so many times that I will probably never find a way to properly repay them. Finally, and most of all, I would like to thank my wonderful fiancée Rebecca for her constant love, support, humor, and companionship. Words cannot express how much this dissertation—and its author—owes to her.

Table of Contents

List of Figures

List of Abbreviations

EMR – Enclosed Multifunctional Reticulation

MARPOL – International Convention for the Prevention of Pollution from Ships

NMFS – National Marine Fisheries Service

NOAA – National Oceanic and Atmospheric Administration

NWHI – Northwestern Hawaiian Islands

PEMR – Progressively Enclosed Multifunctional Reticulation

STCZ – Subtropical Convergence Zone

WWF – World Wildlife Fund

Chapter 1 . Into the Technical Wilderness

The following pages are meant to introduce the reader to the problem of derelict fishing gear in the Northwestern Hawaiian Islands (NWHI). The introduction is divided into four major sections. The first describes the isolation and ecology of the islands. The second explains the mechanisms by which derelict fishing gear accumulates in the region, as well as its negative ecological impacts there. The third section addresses potential solutions for the NWHI's uniquely recalcitrant derelict gear problem. The fourth highlights my own unique contribution to those solutions. In a typical introductory chapter, the first two sections would be radically condensed, thereby underscoring my own intended contributions to the literature. The chapter would instead consist almost entirely of the third and fourth sections. For those who wish to cut straight to that point, the first two sections (1.1 and 1.2) may indeed be skimmed or skipped over. They do not form the core of my argument. However, I have chosen to include them here for two main reasons.

The primary reason is simply this: the current literature on derelict fishing gear in the NWHI suffers from a distinct surfeit of synthesis. Although brief and perfunctory introductions to the problem are typically included in most studies, these remain for the most part superficial. The literature as a whole remains fragmented and highly focused on specific problems, methods, models, findings, etc. There is no overarching account of what makes the islands so unique, how derelict nets get there, how many of them get there, what they do once in the islands, and why we should care. The first two sections of this introduction are meant to rectify this splintering of the literature by synthesizing the current research into a more integrated, holistic, and readable account—and to do so without sacrificing too many of the essential details. There is little place for such an extended synthesis in today's professional journals, whose articles are expected to be both succinct and groundbreaking. Accordingly, I have decided to include it here.

There is a second reason for the inclusion of such a synthesis: I wish to emphasize the potentially broad scope of this dissertation's implications. This is something of which we might easily lose sight, for my own argument is naturally quite focused. It hinges on the analysis of a mere handful of details that, as we shall see, have not been adequately examined in the literature. As such, its analysis is, like that of any other study, honed in to one very specific aspect of the derelict net problem in the NWHI. Despite this, its implications are not restricted to that specific focal point. By including the first two sections of this introduction, I wish to highlight the continuities that exist between my own, somewhat theoretical, work and the larger arena of research and policymaking in the NWHI. My findings, in other words, should not be seen as occupying a hermetically sealed pocket within the academic literature. They are intimately tied up with our overall response to derelict fishing gear in the NWHI and the long-term ecological protection of the islands themselves.

1.1 A Wild and Protected Place

The Northwestern Hawaiian Islands may seem an unlikely setting for a study of technical objects. A barren string of volcanic plugs, sandy atolls, and solitary reefs stretching across two thousand kilometers of the central North Pacific Ocean, the NWHI lie just about as far from the centers of human civilization as one can get on Earth (Figure 1.1). Remote and windblown, these uninhabited islets make up most of the length of the Hawaiian archipelago, yet they have names that almost no one would recognize: Nihoa Island, Mokumanamana, French Frigate Shoals, Gardner Pinnacles, Maro Reef, Laysan Island, Lisianski Island, Pearl and Hermes Atoll, Midway Atoll, Kure Atoll. These anonymous specks of land are surrounded on all sides by vast expanses of unbroken ocean. From their midst one could sail 3,500 kilometers to the Aleutian Islands, 4,000 kilometers to the coast of California, 7,000 kilometers to Japan, or 11,000 kilometers to Antarctica without ever sighting another piece of land. The archipelago does not lie upon any major shipping routes, nor does

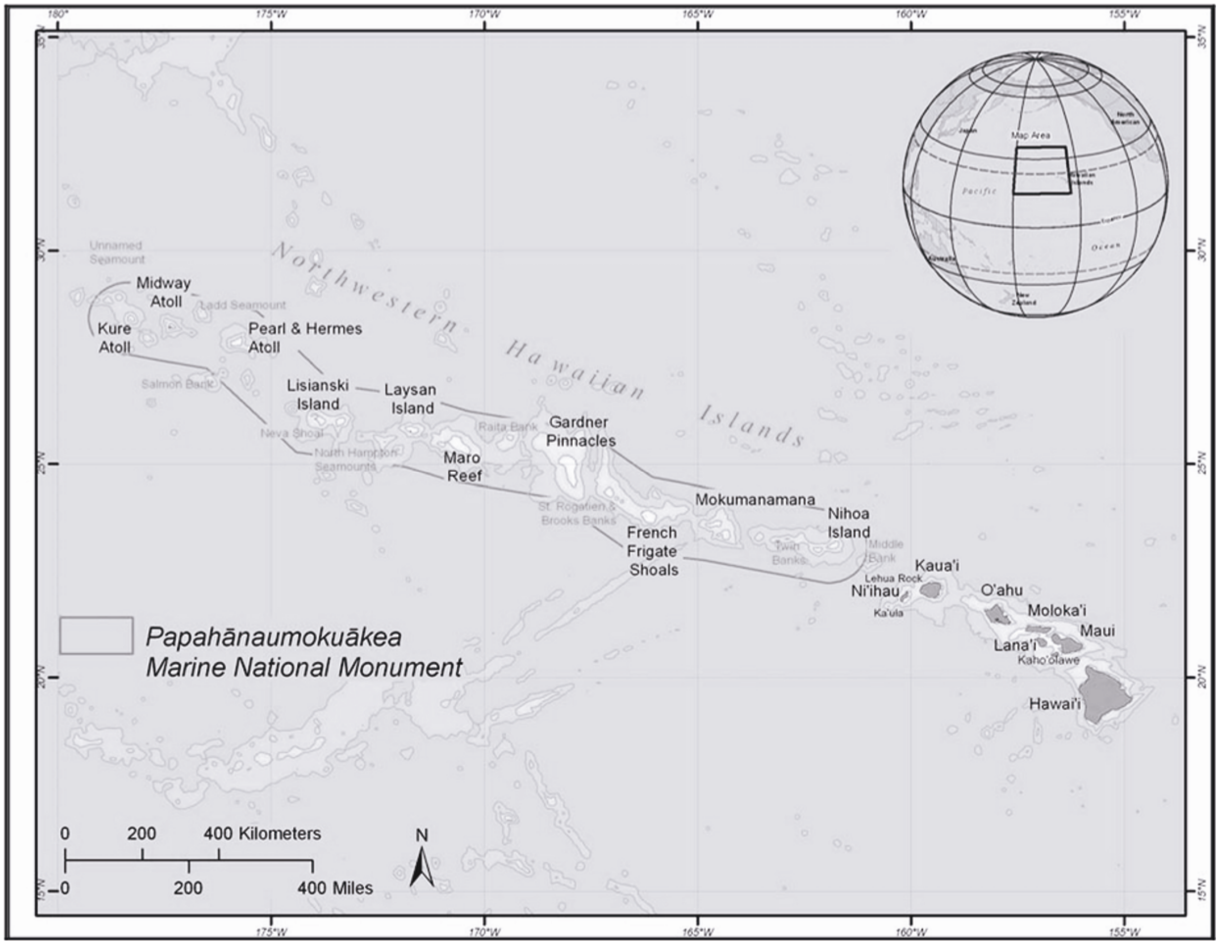


Figure 1.1 Map of the Hawaiian Archipelago. (Image from Papahānaumokuākea Marine National Monument, 2008.)

it host any major commercial fisheries.[1] Accordingly, it sees virtually no vessel traffic throughout the year.

If the immense surface of the North Pacific Ocean isolates the NWHI as a group, it also effectively quarantines each individual island from its nearest neighbors. The distance between successive islands in the chain ranges from 85 to 210 kilometers—not very far when compared to the thousands of kilometers that separate the NWHI from the surrounding continents, but certainly far enough to make any crossing between islands a serious undertaking. More to the point, upon making that crossing one might find that the neighboring island hardly seems to qualify as one at all. It may be a wholly submerged reef, whose ominous presence is betrayed only by the unplaceable roar of breaking surf. Or it may be an inaccessible pinnacle of volcanic basalt, lodged between the heaving sea surface and a wheeling cloud of seabirds. These distant points of land, arranged in a long thin line, form an archipelago in only the loosest sense. Surrounded on all sides by an unbroken horizon, each island in the NWHI is in many ways a self-contained, windswept world unto itself.

Even more than their isolation, it is perhaps the size of the islands that assigns them to obscurity. With scarcely 8 km^2 of dry land, the NWHI comprise less than three one-hundredths of one percent of the total terrestrial area of the Hawaiian Islands. This minuscule quantity of solid ground, spread over so vast a distance, is difficult for the human brain to visualize. To picture it, one might imagine breaking up a typical U.S. airport—say Boston's Logan International—into ten pieces. Discard one of those pieces (Logan is slightly larger than the NWHI by land area), and then distribute the remaining fragments along a line stretching from Boston to Miami. These are the islets of the

---

[1] A small and non-intensive commercial fishery for bottomfish such as snappers, groupers, and jacks has existed in the NWHI for roughly one century. The islands are split into two fishery zones: the Mau zone includes Mokumanamana and Nihoa Island, just west of the Main Hawaiian Islands, while the Ho'omalu zone covers the remaining islands. Only 17 permits are issued in the fishery each year, and recently only 9 vessels have participated in any given year. These are relatively small vessels (30-45 feet) that use mechanically assisted handlines. Still, as Heinemann et al. (2005) argue, this diminutive fleet may nonetheless be exerting negative pressures on the archipelago's bottomfish populations. For a complete history see Uchida & Uchiyama (1986).

NWHI. Like many tropical atolls, they are so tiny that they are scarcely islands at all. Indeed, one of them, Maro Reef, only emerges briefly from the ocean during low tide.

Nor can these diminutive bits of land be confused biologically with the lush Hawaiian Islands of popular imagination. Composed mostly of hardscrabble volcanic rock or cemented coral heads partially covered with sand, the NWHI are low, barren, and fully exposed to the sun and wind. Only a tenacious layer of grasses, ground-creepers, and scrub covers the islands' squat sand dunes. Unlike the shorelines of many Pacific isles, those of the NWHI are not shaded by gently swaying coconut palms.[2] A search for trees will yield only an occasional ironwood tree (*Casuarina equisetifolia*) languishing in the sand, sun-bleached, leafless, and dying (Starr & Martz, 1999; Starr et al., 2001). Terrestrial animals are for the most part unable to survive in this austere environment, even those which normally thrive on remote, uninhabited islands. For example, several rabbits—notorious colonizers of remote islands—were released on Lisianski Island in 1904. They seemed to do well for several years, but then, having eaten up the last of the island's scant vegetation, they all promptly starved to death (Clapp & Wirtz, 1975).

Such unforgiving conditions have helped to minimize human activity in the archipelago. Yet despite the difficulty of human existence here (or perhaps because of it), other organisms have thrived. This is especially true for marine life. Due in large part to their isolation, the NWHI's roughly 13,000 km^2 of coral reefs harbor some of the most pristine coral ecosystems in the world. Unlike many popular scuba diving destinations, the nearshore waters of the NWHI teem not only with vibrant forests of corals and clouds of small, brightly colored fish, but with the sorts of large

---

[2] Only four of the NWHI host any palm trees at all. There are a few planted coconut palms (*Cocos nucifera*) on Tern Island in French Frigate Shoals, and another clump of about 25 on Laysan Island. All of these are old trees that show no signs of successfully regenerating (Starr & Martz, 1999). On Kure Atoll attempts to plant coconut palms in the 1960s failed, and none have established themselves there despite the fact that coconuts wash up onto its shores with some regularity (Starr et al. 2001). Attempts to plant coconuts on Lisianski Island in the 1840s similarly came to nothing (Clapp & Wirtz 1975, p.24). Only on Midway Atoll, with its active U.S. naval base from 1940 until 1996, have coconut palms managed to survive and reproduce.

predators that are increasingly seen as the keystone species of healthy reef systems.[3] More than 7,000 marine species inhabit these waters, and nearly a quarter of those are endemic to Hawai'i, meaning they are found nowhere else on the planet (Anon. 2006). The rates of endemism become more pronounced as one heads further north and west, away from the Main Hawaiian Islands. By the time one reaches the four northernmost islands, well over 50% of the reef fish are endemic, making this one of the most unusual collections of fish species on the planet (DeMartini & Friedlander, 2004; Papahānaumokuākea Marine National Monument, 2008).

As I have already intimated, the extraordinary level of undersea biodiversity does not extend up onto the islands' terrestrial surfaces. Yet even the comparatively bleak island interiors host prodigious populations of particular organisms. Sheltering no native or introduced rodent populations, the thickly carpeted sand dunes provide ideal nesting habitat for millions of seabirds.[4] Indeed, early European and American explorers found it nearly impossible to walk on these islands without repeatedly plunging up to their knees in hidden seabird burrows (Clapp & Wirtz 1975). While the sheer numbers of many of these birds make it difficult to describe them as rare, they are certainly unique to the NWHI. Of the 19 avian species that breed in the islands, several build their nests almost nowhere else on Earth. In the winter, for example, tunnels dug beneath the dunes offer some of the Pacific Ocean's last safe nesting places for Bonin petrels (*Pterodroma hypoleuca*) and Tristram's storm-petrels (*Oceanodroma tristrami*), two species whose burrows have elsewhere been decimated by rodents. Roughly 98% of the world's black-footed albatross (*Phoebastria nigripes*) nest on these specks of land, as do over 99% of the world's Laysan albatross (*Phoebastria immutabilis*).

---

[3] Apex predators such as reef sharks, giant trevally, and amberjacks comprise about 54% of the fish biomass in the NWHI, compared to about 3% in the Main Hawaiian Islands (Friedlander & DeMartini, 2002).

[4] Historically, not all of the NWHI have been continuously rodent-free. The presence of Polynesian rats (*Rattus exulans*) on Kure Atoll was reported by explorers as early as 1870, and were not fully eradicated from the island until 1993 (NWHIMP 2008, p.21). Black rats (*Rattus rattus*), inadvertently introduced to Midway in 1943, were eradicated in 1997 (Ibid, pp.70, 177). Today, the only extant population of non-native mammals in the NWHI is a population of house mice (*Mus musculus*) on Midway's Sand Island. These are slated for elimination with chemical rodenticide (Ibid, p.208).

The list of unique birds could go on and on: the islands host three species of endangered passerine (the Laysan finch, *Telespiza cantans*; the Nihoa finch, *Telespiza ultima*, and the Nihoa millerbird, *Acrocephalus familiaris kingi*), a handful of endemic flightless birds, and the world's rarest species of duck (the Laysan duck, *Anas laysanensis*).

Of even greater importance, to many conservationists, is the fact that the islands' sandy beaches and sheltered lagoons serve as essential nursery sites for the threatened green sea turtle (*Chelonia mydas*) and the critically endangered Hawaiian monk seal (*Monachus schauinslandi*). Several other threatened or endangered marine species have also been well documented in the NWHI, even if they do not regularly nest in the islands' nearshore waters. Among these can be counted six endangered whale species, four threatened or endangered sea turtle species, and the endangered short-tailed albatross (*Phoebastria albatrus*).

The extraordinary ecological richness of the NWHI was officially recognized in 2006, when the entire island chain was designated a Marine National Monument—a status which ensures that, moving into the future, it will receive "the nation's highest form of marine environmental protection" (Anon. 2006). At just over 362,000 square kilometers, the new Papahānaumokuākea Marine National Monument is easily the largest protected area of any sort in the United States and, indeed, at the time of its establishment was the largest marine preserve on the planet.[5] The mandated preservation of this vast area is to be accomplished in the same manner as in other U.S. National Parks: by limiting visitation to one or two designated tourist sites while severely restricting all other forms of access to the park. Accordingly, the small commercial hand-line fishery based in the islands (see Note 1) is to be phased out by the year 2011. All oil, gas, and mineral exploration has of course been banned, along with the dumping of any sort of wastes. To decrease the likelihood of accidental fuel spills or the introduction of invasive species through flushed ballast water, any unauthorized

---

[5] Since 2006, two other marine preserves have been established which eclipse Papahānaumokuākea in size: In 2008, the Pacific island nation of Kiribati enlarged its Phoenix Islands Protected Area to 425,3000 km^2; In 2010, Britain established a marine reserve of 544,000 km^2 around the Chagos Islands in the Indian Ocean.

ship traffic through Monument waters has been prohibited. Finally, a six-tiered permit system has been put in place to ensure governmental control over who is allowed access to the islands and what, exactly, the permit-holder is allowed to do there.

The collective thrust of these regulations is clear: if isolation is what made the NWHI so unique in the first place, continued isolation is what will keep them that way. Wilderness in the islands will be achieved or maintained as it always is: by actively purging the land (or in this case the sea) of all traces of human presence (Cronon, 1996). Under the archipelago's management plan, natural entities (e.g., whale pods, thunderstorms, water masses, solitary albatross) are free to enter or leave the Monument as they see fit. In contrast, humans and their creations are, except under designated circumstances, to be kept away. However, there are some human things that the paper boundary of Papahānaumo-kuākea cannot possibly keep out. Chief among these unwelcome visitors are pieces of derelict fishing gear: commercial fishing nets that, lost or abandoned at sea, have made their way by the tens of thousands into the protected waters of the Monument (Figure 1.2). For all their purported wilderness value, the NWHI today are quite literally awash in abandoned fishing nets.

As we shall see, this derelict fishing gear is puzzling for a number of reasons. But the most fundamental conundrum is this: although more than 80% of the nets found in the NWHI are trawls or fragments of trawls, there are no trawl fisheries of any kind in or around the Hawai'ian Islands (*c.f.* several different figures given in the literature: 83.6% in Macfayden *et al.*, 2009; 86% in Timmers *et al.*, 2005; and 88% in Donohue, Boland *et al.*, 2000). Indeed, the nearest trawl fisheries lie thousands of kilometers away, in the relatively narrow band of territorial waters lining the coastal nations of the Pacific Rim. In order to arrive in the NWHI, derelict fishing gear must therefore make an extraordinary journey across vast distances of open ocean.

Figure 1.2 Derelict fishing gear. A freely drifting mass of derelict netting in the North Pacific Ocean. (Photo from Scripps Institute of Oceanography)

1.2 An Endless Stream of Visitors

The pilgrimage of so many derelict fishing nets to the NWHI, though remarkable, is not as improbable as it seems. In fact, it is readily explained by two factors: (1) the peculiar behavior of plastics at sea, and (2) the general circulation patterns of the North Pacific Ocean. We will consider each of these in turn. The nets used in contemporary trawl fisheries are constructed either from synthetic fibers that are inherently buoyant (e.g. polypropylene or polyethylene) or, if they are built from synthetic fibers that sink (such as polyamide, polyester, or newer Aramid-based fibers), are often attached to numerous small floats (Kostyunin, 1971; Fridman, 1986; Sainsbury, 1996). With a resultant overall density slightly less than that of the ocean water in which they are immersed, trawl fishing nets tend to float, but only just. This slim margin of buoyancy accounts for the ability of derelict nets to reach the NWHI, and to do so in remarkably good condition. Were trawl nets even slightly more dense, they would most likely sink to the bottom long before reaching the NWHI. Were they just slightly less dense they would likely arrive in a far weaker, more brittle state. This, at least, is the implication of Andrady's research (1990, 2000, 2003; also see Ye and Andrady, 1991) into oceanic plastic debris. As he notes, plastics are infamous for their slow degradation in terrestrial environments. However, with the exception of expanded polystyrene (a.k.a. styrofoam), they actually degrade far more slowly in the marine environment than they do on land.

Andrady explains that in any given environment the breakdown of plastics is facilitated by three things: high (and highly variable) temperatures, exposure to UV rays, and the mechanical stress of repetitive motions. In seawater, each of these three factors is minimized. Firstly, plastics at sea are kept at relatively cool and constant temperatures by the surrounding water. Furthermore, as they drift they act as a substrate for the growth of algae and other small marine organisms. This layer of biofouling protects them from direct sunlight. It also weighs them down, causing them to gradually sink below the ocean surface. As their position in the water column drops, so does the temperature of the surrounding water and the amount of sunlight to which they are exposed. In other words, it becomes continually cooler and darker. In addition, mechanical stress caused by the repetitive undulation of surface waves is greatly reduced as plastics become submerged. In this increasingly chilled, lightless, and motion-free environment, the rate at which marine plastics become brittle and lose their tensile strength slows drastically.

Of course, if this process were endlessly self-perpetuating, derelict fishing nets would probably never accumulate in the NWHI. Long before reaching the islands they would simply sink to the bottom of the Pacific Ocean and come to rest. That this does not occur can largely be attributed to the fact that biofouling organisms require sunlight to survive. As a derelict net sinks, the sunlight percolating down to it eventually becomes so limited that the biofouling organisms die and drop off, allowing it to resurface and begin the entire cycle again. Protected below the immediate sea surface from wild temperature swings, repetitive mechanical stresses, and UV radiation, derelict fishing gear has the potential to drift unseen for decades, traveling many thousands of miles, without appreciably deteriorating. Of course, being well preserved is one thing, and being preserved *en route* to a specific location is another. How is it that so many derelict nets end up converging in the NWHI, as opposed to dispersing throughout the North Pacific? The answer lies in the peculiar geography of large-scale ocean currents there.

Circulation patterns in the North Pacific are dominated by the spiraling currents of the North Pacific Subtropical Gyre, an immense vortex of seawater that rotates in a clockwise direction around the edges of the North Pacific (Figure 1.3). The gyre is actually comprised of four great boundary currents: the Kuroshio Current to the west, the North Pacific Current to the north, the California Current to the east, and the North Equatorial Current to the south. These broad currents have a universal tendency to curl toward the right (a phenomenon known as Ekman drift). Therefore, any debris transported around the gyre will gradually move towards its center: an area known as the North Pacific Subtropical Convergence Zone (STCZ), where various surface currents collide and intermingle. While convergence zones are not unusual in themselves (they occur in some form wherever surface currents come together), this one is unusually large. Stretching across nearly the entire Pacific north of the equator, the STCZ draws in so much water that, at its highest point, it rises a meter higher than its lower outer edges (McClain et al., 2002). Any flotsam or jetsam situated atop this enormous dome of seawater is, for all practical purposes, trapped there. Hemmed in on all sides

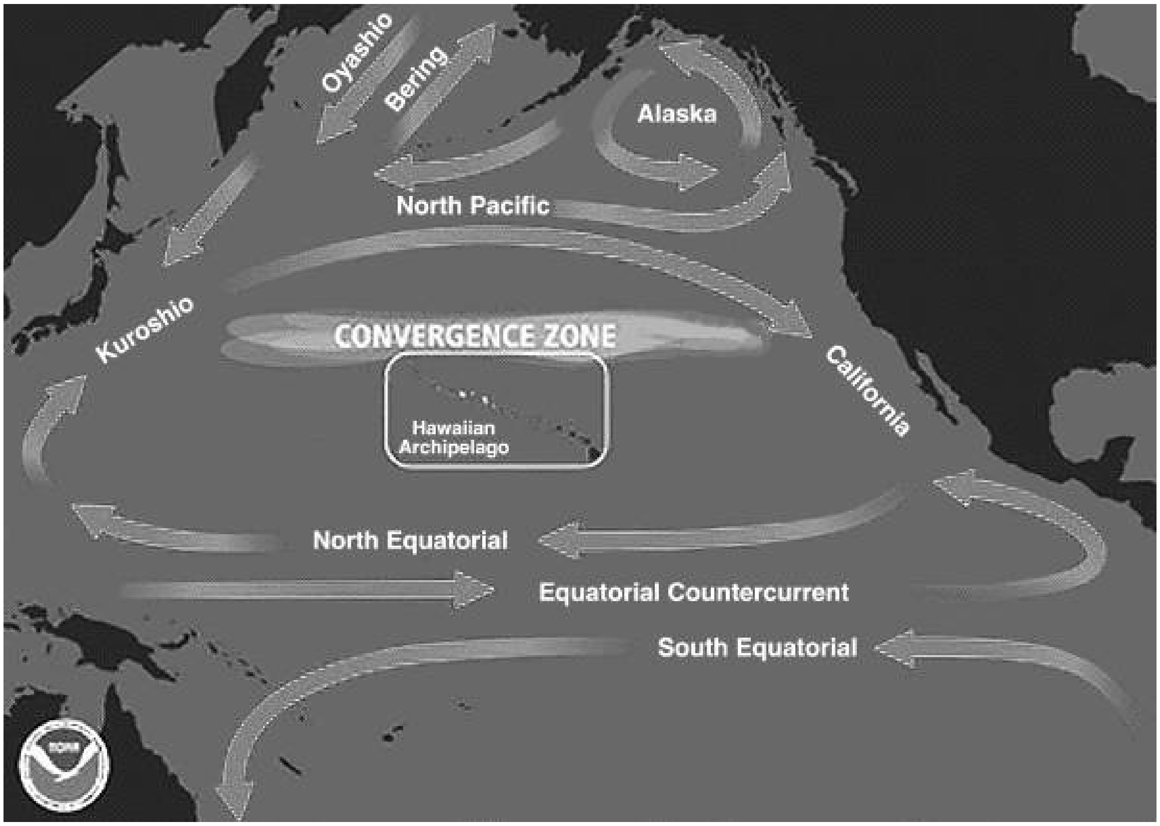


Figure 1.3 Map of the North Pacific Gyre, showing its constitutive boundary currents and the convergence zone lying within those boundaries. (Modified from a NOAA Marine Debris Program image.)

by inward flowing surface currents, such floating debris quite literally has nowhere else to go.

The circulation of the gyre around the North Pacific assures that a fishing net lost off the coast of Japan, another abandoned near Taiwan, and a third damaged and discarded off the coast of Oregon will all eventually end up in a single clump within the STCZ. Given enough time, they will all be concentrated under a persistent zone of high atmospheric pressure that lies several hundred kilometers north of the Hawai'ian Islands. This was famously predicted by Kubota (1994). His research, employing computerized models of oceanic drift patterns, found that theoretical debris items placed throughout the North Pacific would inevitably accumulate in a narrow band traversing the STCZ. Running east-northeast to west-southwest, this simulated swath of debris crossed directly through the NWHI in the vicinity of Laysan and Lisianski Islands. Kubota's prediction was later confirmed by a number of shipboard debris surveys, which showed unusually high concentrations of fishing nets and other debris just north of the NWHI (Matsumura and Nasu, 1997).

While it is impossible to say exactly how much derelict fishing gear exists in the waters of the STCZ, aerial surveys over the region suggest it is extremely prevalent. In 2005, the U.S. National Oceanic and Atmospheric Administration (NOAA) conducted three low-altitude survey flights over the STCZ in an effort to locate derelict nets. Within an effective survey area of about 9,720 km^2 (equivalent to a square of ocean measuring slightly less than 100 km per side), researchers documented 1,885 individual pieces of floating debris, of which 122 were derelict fishing nets (Pichel *et al.*, 2007). Among these were two very large masses of balled up netting with diameters of more than ten meters (*ibid*). Despite such surveys, the distribution of marine debris remains poorly understood. We know that it is spatially heterogeneous, but little else. This prevents us from using NOAA's survey results to accurately extrapolate the total number of derelict nets in the STCZ or the North Pacific Subtropical Gyre. However, given that the STCZ is roughly 10,000 kilometers long, and that the gyre itself occupies some 34 million square kilometers, even a conservative guess would put the number of derelict fishing nets in the ocean north of Hawai'i in the tens of thousands.

Many of these nets are essentially trapped within the STCZ, but this does not mean that they are immobile, or that they will remain permanently quarantined from any contact with land. Rather, it means only that their location depends on the location of the convergence zone. If the STCZ remains stationary, so will the derelict nets it contains. But the STCZ is highly mobile over the course of each year. During the summer months, it is typically located at about 34 degrees N, roughly 1,500 kilometers north of Hawaii (Pichel et al. 2007). However, in the winter months a persistent area of low pressure in the Gulf of Alaska, known as the Aleutian Low, causes the front to migrate southward by up to 1,000 kilometers. It typically reaches its southernmost point at around 28 degrees north, a parallel that intersects the NWHI just to the north of Pearl and Hermes Atoll. Some sources cite a latitudinal range for the STCZ of between 37 and 23 degrees N (Roden 1975), putting the southernmost point of the front's migration at Mokumanamana, quite close to the Main Hawaiian Islands. Wherever that point lies, it is clear that during El Niño years the STCZ and its dense concentrations of derelict fishing gear push even further south (Pichel et al, 2007; Morishige et al. 2007). Following behind the front may be numerous "fossil fronts," swaths of concentrated debris that no longer correspond to its current location (Pichel *et al.,* 2007; Roden, 1980).

When derelict nets do move from the open ocean across the reefs of the NWHI, they can accumulate there in staggering numbers. The islands have been likened to the teeth of an enormous comb, such is their propensity to intercept drifting debris from all across the North Pacific. Surveys conducted by the U.S. National Marine Fisheries Service (which is part of NOAA) between 1999 and 2001 provided a startling illustration of this tendency, finding 47 pieces of derelict fishing gear per km^2 in the nearshore waters of Pearl and Hermes Atoll, 63 pieces per km^2 at Lisianski Island, 94 pieces per km^2 at French Frigate Shoals (where the survey was conducted in 1996 and 1997), and an astonishing 165 pieces per km^2 at Kure Atoll, near the northwestern terminus of the archipelago (Boland, 1997; Laist & Liffman, 2000; Boland & Donohue, 2003). Such numbers are dramatic. However, it should be noted that not every reef in the NWHI, nor even every part of a given "problem" reef, is littered with such high densities of derelict fishing gear. Some reef areas seem to harbor scarcely any derelict nets at all (Donohue, Boland, *et al.*, 2000).

This variability is best explained by the presence or absence of what researchers call "net habitat". Studies in the NWHI indicate that certain areas are more apt to collect nets (especially large nets) than others. Net-prone areas typically share three characteristics: they are shallow, they have reef structures with high topographic relief, and they experience low wave energy. Islands with large, shallow lagoons reticulated by a large number of coral heads—what Dameron *et al.* (2007) refer to as "expansive low-energy environments"—will tend to collect more nets than those with only semi-protected lagoons, or few coral heads, or relatively deep water (Donohue *et al.*, 2001; Dameron *et al.*, 2007). The reasons for this have to do with the way in which derelict nets typically make contact with the islands of the NWHI. Pushed forward by the incessant surf, and balling up like enormous tumbleweeds as they do so, large masses of derelict netting are often able to wash up and over the islands' relatively flat barrier reefs (or backreefs) without becoming entangled. When they enter the calm waters of the lagoon, a pronounced lack of wave energy allows the masses of netting to settle back down to a lower position in the water column. If the lagoon is shallow, and if it contains a large number of obtrusive coral heads, such netting becomes easily entangled on the jagged lagoonal reef substrate. If it avoids such entanglement, it is likely to wash up on one of the lagoon's sandy beaches. Such beachside strandings are not uncommon themselves: in the period 1982-1986 alone, some 773 derelict nets were documented and removed from the beaches of the NWHI (Donohue, Brainard *et al.*, 2000).

The most telling figures, however, do not concern the number of nets littering the beaches and reefs of the NWHI at any one time. Rather, because new nets continually wash in and become trapped in the islands' low energy environments, it is the rate of this continual accumulation that is of greater concern. According to the most recent estimates, 52 metric tons of new netting (approximately 115,000 lbs.) accumulate in the islands every year (Dameron *et al.*, 2007). On individual reefs this translates to annual accumulation rates of up to 141 pieces of derelict gear per square kilometer (Boland & Donohue, 2003). Such figures quite clearly underscore the difficulty (indeed, the near impossibility) of keeping derelict fishing gear out of Papahānaumokuākea's reef ecosystems. For even if every reef and lagoon in the NWHI were somehow cleared instantaneously of derelict fishing gear, more fragments of netting would almost immediately begin washing in from the open sea. As long as there are abandoned nets adrift somewhere in the waters of the North Pacific, they will unfailingly make their way toward the NWHI.

This extraordinary influx of nets would be no more than an aesthetic problem if derelict fishing gear were innocuous. But innocuous it is not. Derelict nets have been shown to negatively impact the NWHI's reef ecosystem in at least three distinct ways (Timmers et al. 2005). Firstly, they pose a serious entanglement risk for marine organisms. Secondly, as they settle to the bottom, nets abrade living corals, break off entire coral heads, and scour the reef substrate itself. Thirdly, they provide a potential vector for invasive organisms to reach the NWHI. Of these three impacts, it is the first, the entanglement of marine organisms, that has most concerned conservationists and ecologists.

A high proportion of the entanglements that have been documented in the NWHI involve nets that are already hung up on reef outcrops (*e.g.*, Laist and Liffmann, 2000). However, this may be due to the ease of locating and studying such nets. Indeed, there is evidence that drifting nets also pose an acute risk of entanglement. Many pelagic organisms have evolved to forage around floating objects, which often mark the presence of nutrient-rich waters (Kiessling 2003, Constantino & Salmon 2003). A floating mass of netting may therefore serve to attract the attention of marine organisms, whose investigations can quickly lead to their entanglement (Laist, 1994; Kiessling, 2003; Macfayden *et al.*, 2009). The visibility (and therein the danger) of such nets is compounded by the tendency, in large-scale convergence zones, of floating debris items to clump together into large aggregations. Observers have reported that "rafts of assorted debris, including various plastics; ropes; fishing nets; and cargo-associated wastes such as dunnage, pallets, wires and plastic covers, drums and shipping containers, along with accumulated slicks of various oils, often extend for many kilometers" in such areas (Macfayden *et al.,* 2009). In the nearshore waters of the NWHI derelict nets seem to entangle each other with some frequency, aggregating into large floating mats of netting and other debris (Timmers et al. 2005).

Whether occurring on reef areas or in open water, entanglements in the NWHI have been especially well documented for Hawaiian monk seals. During the 1990s, the annual rate of lethal entanglement for monk seals in the islands ranged from 0.18% to 0.85% of their total population—a level of mortality that, while potentially inconsequential in a large population, probably cannot be absorbed indefinitely by the very small remaining number (roughly 1,200) of wild monk seals (Henderson, 1990, 2001). In the 1980s and 1990s, the Honolulu Laboratory of the U.S. National Marine Fisheries Service (NMFS) documented more than 200 monk seal entanglements in the NWHI, including a worrisome 25 cases in 1999 alone (Laist & Liffman, 2000). And while research in the NWHI has heretofore focused almost exclusively on monk seals, it is likely that entanglement also constitutes a grave problem for other organisms inhabiting the islands' nearshore waters. For instance, studies of derelict trawl netting in Australia's Gulf of Carpentaria have found high rates of entanglement among several sea turtle species, as well as frequent, anecdotally supported entanglements of fish, sharks, seabirds, and dolphins (Kiessling, 2003). There is no reason to believe that such entanglements are less widespread in the waters surrounding the NWHI.

1.3 Solving the Derelict Gear Problem

In a concerted attempt to mitigate the negative environmental impacts of derelict fishing gear, NOAA and the National Marine Fisheries Service (NMFS) have, since 1996, conducted annual operations to locate and remove derelict fishing gear from the reefs of the NWHI (Donohue, 2000; Donohue, Brainard *et al.*, 2000; Laist and Liffmann, 2000). As of 2009, their efforts, which employ teams of scuba divers working in a coordinated way with inflatable zodiac boats and a crane-equipped mother ship (Figure 1.4), had resulted in the removal of 671.45 metric tons (nearly 1.5 million pounds) of netting (NOAA Marine Debris Program, 2010). Before 2006, these efforts took an island-hopping approach, proceeding each year from one atoll to another and removing as much netting as possible. Since then, the program has switched over to a maintenance mode, meaning that derelict nets are removed only from those "problem areas" where accumulation and entanglement rates are highest.

This requisite shift to selective maintenance highlights one of the fundamental shortcomings of net removal efforts: their impacts are at best fleeting. Net removal campaigns in the NWHI can never be entirely successful, for the continual influx of new nets from the open sea is perpetually

Figure 1.4 Gear Retrieval. Scuba divers work to remove a derelict net from the seafloor. (Image from NOAA Photo Library.)

undoing the success of previous mitigation work. This is not to suggest that the removal of derelict nets has no positive impacts on reef ecosystems, for it most assuredly reduces the amount of ongoing mechanical damage to coral heads and diminishes the risk of entanglement posed to seals and other organisms. But net removal can never be more than a temporary fix, a curative measure or palliative that, while ameliorating the ultimate symptoms of net dereliction, does nothing to resolve its underlying causes. Indeed, even the curative value of net removal should not be overstated. For, by the time a given piece of derelict fishing gear is removed from the water (which in the NWHI occurs at intervals of one year or longer), it has in all likelihood already had ample opportunity to entangle marine organisms, damage coral reefs, and introduce invasive species to the region (Brown and

Macfadyen, 2007). In sum, as a purely curative measure, and perhaps a rather ineffective one at that, net removal can never constitute a viable long-term solution to the problem of derelict fishing gear.

A second drawback to net removal strategies such as NOAA's is their expense. Wiig (2005) has calculated that the retrieval of derelict fishing gear tends to cost between $65 and $25,000 per metric ton. However, the removal of derelict netting in the NWHI is an unusually difficult endeavor with an unusually high price tag. This is due to the vast distances involved, the impossibility (or inadvisability) of bringing large retrieval vessels into shallow reef areas, the fragile and protected nature of the benthic environment, and the basic physical difficulty of disentangling a large net from a substrate of jagged corals. Donohue, Brainard, *et al.* (2000) put the cost of NWHI net removal in 1999 at over $1 million. From this we can calculate that each ton of recovered netting required an expenditure of more than $40,000. Some of this expense can perhaps be attributed to NOAA's lack of any dedicated gear-retrieval vessel: in 2003 the Administration reportedly spent $10,000 per day simply to lease its "mother ship" vessels (Wiig, 2005). In a later paper, Donohue put the estimated cost-per-ton of NWHI net removal at $30,000—a slight decrease— but increased her estimate of the program's total annual cost to $3 million per year (Donohue, 2005). Unsurprisingly, this level of expenditure dwarfs the costs associated with other, less complicated annual gear retrieval programs. Brown *et al*. (2005) put the annual cost of Norway's (typically thorough) gillnet retrieval program at $260,000, the cost of a pilot program off of the Irish coast at $185,000, and the annual cost of Sweden's Baltic Sea net retrieval program at $70,000. Brown and Macfadyen (2007) have concluded that, given the high costs and uncertain benefits of such programs, net retrieval in most cases constitutes an unreasonable and unwarranted response to net dereliction.

We could doubtless point to other drawbacks of net retrieval in the NWHI. For instance, the requisite use of scuba divers to free nets from the coral substrate is logistically complex, highly dependent on weather conditions, and greatly affected by underwater visibility levels (Donohue, Brainard, *et al.*, 2000). However, the shortcomings already mentioned suffice to illustrate that curative measures leave a great deal to be desired, both for policy-makers and environmentalists. A more cost-effective and environmentally sound alternative would be to focus on preventive measures instead—*i.e.*, those that obstruct derelict nets from ever entering the water in the first place (Brown and Macfadyen, 2007; Matsuoka *et al.*, 2005). Prevention means shifting attention from net sinks to net sources, moving upstream and attending to fishing nets before they are lost, rather than afterward.

There is no single template for such preventive measures. They might conceivably take many forms, depending on how net dereliction actually occurs in specific instances (Macfadyen *et al.*, 2009). While it is probably uncommon for an entire trawl net to simply be discarded into the ocean, given the high cost (between $30,000 and $50,000) of such gear (*c.f.* Minton, 2000; Brainard *et al.*, 2000), smaller pieces of trawl netting are probably discarded fairly often. For instance, when a bottom trawl—or even a midwater trawl fished near the bottom—becomes snared on a rocky outcropping or submerged shipwreck, causing damage to the net that requires immediate shipboard repair, the ruined section of mesh is typically cut out, discarded, and replaced with new panels of netting (Minton, 2000). In this case, net dereliction involves a relatively small fragment of netting and likely can be attributed to inadequate waste disposal facilities onboard or at the vessel's home port (Topping *et al.*, 1997). On the other hand, if extreme weather conditions or another kind of vessel emergency arises during trawling, creating an imminent risk of capsizing, a vessel may be forced to simply jettison its fishing gear entirely (Minton, 2000). In this case, the skipper may well have chosen to fish in relatively unsafe conditions because of the intense competitive pressures created by specific fishery regulations or regulatory regimes. Carr and Harris (1997) provide support for such a view, suggesting that intensified economic pressures on fishermen have promoted greater levels of risk-taking with fishing gear, geographic expansion of fishing grounds, and a resultant increase in the numbers of gear conflicts with other fishing vessels.

The optimal solution in these two cases of net dereliction is clearly different. While in each case the loss of fishing gear might well have been prevented with appropriate policy or regulatory measures (the providence of better portside disposal facilities, or the crafting of regulations to reduce inter-vessel competitive pressures), it is obvious that no single solution could effectively be applied to the two separate cases. Such a blanket measure would likely prove wholly ineffective and unnecessarily burdensome in one case, therein constituting a considerable waste of effort and/or resources (Henderson and Steiner, 2000). This is a crucial point: For preventive measures to be effective, they must be targeted at specific problem fisheries, where net dereliction occurs in specific ways and can be countered with specific solutions.

The futility of the alternative approach (i.e. non-targeted prevention) has been clearly demonstrated by the failures of the existing global ban on the disposal of plastic fishing nets at sea: Annex V of MARPOL 73/78 (the International Convention for the Prevention of Pollution from Ships, 1973 as modified by the Protocol of 1978) is supposed to prevent the abandonment of fishing gear in the fisheries of all its signatories. In the United States, the first part of MARPOL (Annexes I and II) was implemented by the 1980 Act to Prevent Pollution from Ships. Annex V of the convention, titled 'Regulations for the Prevention of Pollution by Garbage from Ships', was implemented by the Marine Plastic Pollution Research and Control Act of 1987. Annex V (and its 1987 U.S. implementing law) explicitly prohibits the at-sea disposal of plastic fishing nets. Still, enforcement of the law has been hampered by—among other things—loopholes in the wording of the legislation, the relative ease with which violators can escape detection at sea, and the difficulty of proving jurisdiction within U.S. territorial waters (Carroll, 2000). For all of these reasons, the implementation of Annex V into U.S. law has had no apparent effect on the rate at which derelict fishing gear accumulates in the NWHI (Henderson, 2001). This failure makes it clear that non-targeted prevention is an insufficient policy response to the problem.

Targeting preventive measures, however, requires that the sources of derelict fishing gear be known. Where they are not, they must be identified. In such cases, the identification of derelict net sources becomes a critical bottleneck hindering the implementation of targeted preventative measures. The NWHI constitute just such a case, for the sources of derelict gear in the islands remain unclear. Indeed, Donohue (2005) has recognized this critical need for source identification in the islands, conceding that, "Given the damage inflicted by derelict gear on NWHI natural resources and the significant costs associated with mitigation of this pollution, confirmation of source fisheries and reduction of gear loss and discard is likely the only viable long-term strategy for the protection of the NWHI's unique resources" (p. 887). Source identification is therefore recognized as a necessary precursor to the implementation of any policy measure directed at specific problem fisheries (Henderson and Steiner, 2000).

Since 2006, the development of better source identification methods has been a compulsory task for NOAA. This obligation has its roots in the congressional Oceans Act of 2000, which created a U.S. Commission on Ocean Policy and tasked it with producing a comprehensive report on U.S. marine policy for the 21st century. The Ocean Commission's 2004 report declared that "The implementation of effective control measures [for marine debris] is currently hampered by a lack of consistent monitoring and identification of sources of debris" (U.S. Commission on Ocean Policy 2004, p. 268). It recommended that NOAA create a marine debris management program that would, among other things, work to identify the sources of marine debris and derelict fishing gear. In 2006 this suggestion was codified into law by the Marine Debris Research, Prevention, and Reduction Act, which declares identification and source determination of marine debris to be two of its primary goals and which, in Sections 3.1 and 3.2, obligates NOAA to pursue improved methods for identifying the sources of derelict fishing gear and other marine debris. It also obligates NOAA, in Section 6.1, to set up and maintain an information clearinghouse in order to assist any interested parties with source identification or other marine debris research. NOAA's progress on these fronts is to be detailed in an annual report that the Interagency Marine Debris Committee, co-chaired by NOAA, is required to make to Congress each year.

There are two general approaches that NOAA (or any other organization) might take toward the task of source identification: Firstly, nets could be identified through extrinsic identifiers such as tags or other markings; secondly, they could be identified through intrinsic identifiers, *i.e.*, unique characteristics of their physical design and construction. The first approach, based on individual gear marking, may initially seem like the more sensible option. After all, gear marking is a fairly common practice for many fishermen already. However, a number of obstacles converge to make the marking of trawl nets highly infeasible. Although it is true that fishermen in a great many fisheries are required to mark their gear, the gear in question is almost invariably passive gear such as gillnets, trammel nets, or traps. Passive gear is detached from the vessel during fishing and left on its own for a period of time before being recovered. There is therefore a great deal of incentive for fishermen to voluntarily mark their own passive gear: doing so facilitates the identification and recovery of their gear from the water, discourages the theft of the gear or its catch by other fishermen, and helps enforce legal limits placed on the amount of gear that each vessel may operate (National Research Council, 2008). Because passive gear is stationary relative to the bottom or the surrounding water, it can easily be marked with uniquely painted or tagged buoys, which will in any case not compromise its performance.

Trawls, on the other hand, are a form of active gear, meaning that they must be actively towed behind the fishing vessel in order to catch fish. Tagging such gear is unusual, for it remains attached to the vessel at all times, rather than being released and then subsequently located and recovered from the ocean. Active gear can only conceivably be separated from the vessel during dereliction. Accordingly, required trawl marking is restricted to fisheries in which derelict trawl netting has become a widespread and recognized problem. For example, prawn fishermen in Northern Australia are required to attach identification tags to each trawl's headline and footrope—

the heavy ropes lining the top and bottom of the trawl mouth. Yet this example also highlights the great difficulty of marking trawls in a useful manner, for the headline and footrope are almost never the parts of the trawl that are lost during dereliction. It is far more typical to lose fragments of mesh webbing from the main body of the trawl, while retaining its framing ropes. Accordingly, the mandated headline and footrope markings do very little to enable the identification of derelict gear (Kiessling, 2003). Circumventing this problem, ensuring that every piece of derelict trawl netting carries a marker, would require gear manufacturers to affix a very large number of tags at points all over the net. However, this "solution" would so greatly compromise the performance and durability of the trawl itself that it is considered a non-option (Henderson and Steiner, 2000).

There are other significant obstacles to trawl marking as well. However it might be achieved, tagging an entire trawl would be expensive for trawl manufacturers—an expense that would assuredly be passed on to trawl purchasers (i.e. fishermen) and, ultimately, to consumers. Effective marking would also require the investment, in money, time, and effort, of creating and continually maintaining a national gear registry database (*ibid*). This administrative requirement would be made especially difficult by the fact that trawl nets may be freely sold, traded, or loaned to numerous vessels during their lifetimes. Accordingly, the particular fisherman identified by a marker on a derelict net might no longer be its owner/operator, and indeed might not even be involved in fishing anymore (Minton, 2000). Finally, acceptance of and compliance with any mandatory marking requirement is unlikely among fishermen, given the general perception that such markings will make them legally liable for their lost gear (Henderson and Steiner, 2000). Indeed, the prospect of gear marking became a topic of fierce debate in the 1980s, with fishermen surmising that any such requirement would open the door for authorities to take punitive measures against anyone "guilty" of losing their gear (National Research Council, 2008). Reducing such skepticism and probable non-compliance in the industry would likely necessitate the inclusion of a "no fault" provision coupled with a requirement for the timely reporting of lost gear—compromises that would make evidence of net dereliction more forthcoming but less "actionable" (ibid).

Given these difficulties, considerably more effort has been focused on the use of intrinsic identifiers—*i.e.*, design characteristics—to ascertain the sources of derelict trawl nets in the NWHI. This second form of identification relies on a rather unique feature of fisheries: They are bounded geographic spaces defined by a host of functional parameters (e.g., target species, bycatch species, size of fishing vessel, length and precise dates of fishing season, type of fishing gear used, minimum allowable mesh size of that gear, etc.). The functional characteristics of a recovered trawl net can therefore be used as proxies for its geographic origin. The interpretation of such proxies simply requires a broad level of expertise on currently employed fishing equipment and methods in the relevant fisheries (Henderson and Steiner, 2000). This "intrinsic" method has, in the last decade, become something of a standard approach for identifying the sources of derelict trawl nets and targeting preventive measures at those sources.

Although there is no single NOAA document in which the logic of this approach is spelled out in its entirety, one does exist for the world's other primary hotspot for derelict trawl accumulation: the Arafura Sea and Gulf of Carpentaria in Northern Australia. The response to derelict trawls in that region is managed by Australia's National Oceans Office and Department of the Environment and Heritage, in collaboration with the World Wildlife Fund (WWF) Australia. Their primary report on the matter (Kiessling, 2003) includes a comprehensive explanation of why trawl net design identification is so crucial to preventing future gear loss. It is also quite clearly indebted to NOAA's research efforts in the NWHI: NOAA conferences, reports, and articles are heavily cited throughout the document. Therefore, its unusually clear statements on the necessity of source identification for derelict trawl nets can and should be taken as an expression of the logic informing NOAA's approach as well.

In the Australian report, Kiessling (2003) explains that reliable source identification is necessary in order to prevent the abandonment of trawl nets by fishermen. "In regard to penalties for poor [fishing] practice," he states,

> a basic requirement is to increase the capacity to connect items of debris to the activities of particular nations, sectors, companies, vessels and/or individuals operating in or near Australian waters. This should include … identification of gear types … and verification of the source of derelict fishing gear through collaboration with gear experts and surveillance operations... Improvements to permanently mark or otherwise reliably identify fishing gear would be a significant step towards mitigating the problem. (Kisessling 2003, p. 45)

The report also clarifies that the primary means of identifying fishing gear must be the "classification of the source/manufacturing origin of items" and that this classification should be achieved through the "identification of indicator items [with which] to categorise the source of debris" (Kiessling 2003, p. 36). The precise indicators to be identified, the report goes on to explain (p. 37), are contained in the actual design features of the nets:

> In order to develop solutions to marine debris, understanding of the source of problems is critical – from the manufacturer to the individual user. Derelict fishing nets provide valuable clues via their mesh size, colour, knot and fibre type, as to the origin of their manufacture and possible source.

In order to facilitate the interpretation such design "clues", WWF Australia has published and distributed a comprehensive field guide titled *The Net Kit: A Fishing Net Identification Guide to Northern Australia* (White *et al.*, 2004). The Net Kit assigns unique code numbers to 184 different types of derelict net recovered in Northern Australia, describes their respective design features, and provides a mechanism for the collection of data about each one's prevalence:

> Net information, such as type of net encountered, its identification code, dimensions and the presence or absence of stranded marine wildlife can be recorded on the data sheets in the back of the booklet and sent to WWF for entry into a central database.

> This database is maintained by WWF and provides valuable information to management agencies for the formation of policy and management actions in northern Australia and other parts of the world. (White *et al.* 2004, p. 2)

The particular reason to gather this "valuable information", it is explained, is so that "targeted solutions to this problem can be developed and implemented" (White *et al.* 2004, p. 2). NOAA's current source identification efforts are built upon the same body of logic as their Australian counterparts and—it should therefore come as no surprise—have pursued similar design-based methods.

Since 1984, NOAA has convened a total of four international conferences to address the issues of derelict fishing gear and marine plastic debris more generally. Each conference has resulted in a recommendation that gear description efforts be undertaken in order to facilitate the source identification of derelict nets in the North Pacific Ocean (Henderson and Steiner, 2000). The 1984 recommendation yielded immediate results in federal year 1985, when $48,000 in federal funding was directed toward a project to establish a reference collection of fishing nets used in the North Pacific. However, the project was discontinued in 1986 due to general funding shortages at the federal level. In its brief period of activity, it resulted in the assembly of a relatively small reference collection of nets at the Alaska Fisheries Science Center in Seattle, Washington. While parts of the collection remain there, it has seldom been utilized and has in any event never been cited in any reports or papers. (*ibid*). For a number of years after this initial attempt, no formal effort was made to establish another reference collection of nets.

This changed with NOAA's concerted efforts, beginning in the late 1990s, to locate and remove derelict fishing gear from the NWHI. During the trawl net retrieval process, representative 40 x 40 cm sections of netting were removed and analyzed by NOAA researchers. Specific parameters were recorded for each netting sample and that information was then entered into a database. In 1998-1999, every recovered net was analyzed in this way. However, as increasing amounts of derelict gear were recovered in subsequent years, analysis could only feasibly be conducted on some of the nets. From 2000-2002, only 25% of the recovered nets were sampled for analysis of their designs. In 2003 that proportion was reduced to 10%. Despite these reductions, NOAA had, by 2004, analyzed over 5,000 derelict nets. If the resulting database of net designs can be used to systematically identify derelict nets as they are recovered from the NWHI, then NOAA will quite clearly have met its obligations under the Marine Debris Act.

However, NOAA's net identification database has had disappointingly little success in positively identifying the sources of recovered nets. A published portion of the net identification database, consisting of information on 250 derelict nets, was able to identify a general kind of source fishery—*e.g.*, "shrimp trawl"—for only 18.8% of the derelict gear (Timmers *et al.*, 2005). Only 1.6% of the analyzed nets could be conclusively tied to a specific netting manufacturer (*ibid*). Researchers involved in these efforts admit that the major obstacle hindering NOAA's identification efforts is a pervasive lack of knowledge about the North Pacific fisheries and, in particular, the nets used in them (Donohue, personal communication). In other words, the possible list of sources for derelict fishing gear in the NWHI is itself unknown. Consequently, researchers are placed in the impossible situation of not knowing what (or where) it is that they are trying to trace nets back to.

This is not a situation that arises by chance or poor foresight. Rather, it is the result of three complicating factors that necessarily obfuscate the task of trawl net identification. We might refer to these as the problems of (1) fragmentation, (2) complexity, and (3) multiplicity. Presumably these are important factors whenever trawl nets become derelict. However, they have yet to be adequately addressed by the ghost net literature, which is skewed toward case studies that avoid them. Potentially quite common in the marine environment, yet seldom glimpsed in the literature, these three complicating factors clearly merit a closer examination than they have previously received.

Fragmentation refers to the fact that very little of the derelict fishing gear recovered in the NWHI consists of entire nets. Rather, what wash up in the islands are irregular fragments of netting torn away from larger nets. This highlights one of the great imbalances in the current literature on derelict fishing gear. By an overwhelming majority, case studies have focused on fisheries that utilize passive fishing gear such as gillnets or trammel nets. When such gear is lost or abandoned, it is typically done so in its entirety: a complete gillnet, or several of them strung together, will simply fail to be recovered by the fishing vessel. Because passive gear that has been abandoned is still fully intact, it can engage in *ghost fishing*, continuing, despite the lack of any human oversight, to ensnare fish exactly as it was designed to do. Most case studies of derelict fishing gear have focused squarely on the problem of how to quantify rates of ghost fishing mortality and/or net loss (e.g., Gilardi et al., 2010; Baeta et al., 2009; Santos et al., 2009; Campbell and Sumpton, 2009; Adey et al., 2008; Ayaz et al., 2006; Hareide et al., 2005; Matsuoka, 2005; Al-Masroori et al., 2004; Godøy et al., 2003; Humborstad et al., 2003; Sancho et al., 2003; Santos et al., 2003a; Tschernij and Larsson, 2003; Bullimore et al., 2001; Erzini et al., 1997; Kaiser et al., 1996). Accordingly, they have dealt almost exclusively with the wholly intact, passive nets (or traps) that engage in such unsupervised fishing.

By contrast, the derelict netting found in the NWHI tends to come from active fishing gear, which functions only when towed behind a moving fishing vessel. As I have already noted, because active gear is never detached from the vessel during fishing, it can never simply drift away, unseen, in one piece. Instead, it is typically lost when part of it encounters a sharp or unmovable obstacle (such as a rocky outcropping on the seafloor) and is torn away from the main body of the net. Such fragments of detached netting do not engage in ghost fishing, per se. They cannot continue fishing on their own, for they can only ensnare fish in their intended manner when they are dragged behind a fishing vessel. Perhaps for this reason, they have largely escaped the scrutiny of derelict fishing gear researchers, fixated as the latter are on the phenomenon of ghost fishing. However, as the preceding section clearly illustrates, derelict trawl fragments can pose their own grave threats to the marine ecosystems through which they drift, even if they do not technically engage in unsupervised fishing activity.

It is germane to point out that while passive nets might conceivably be easier to lose than active nets (since they do not need to be actively wrenched away from the fishing vessel), very little of the global fish catch is actually harvested with the former. Indeed, only about 10% of the world's annual commercial catch is harvested with gillnets (Watson et al., 2006). By comparison, roughly 20% of the global catch is taken by bottom trawls (presumably the easiest form of active gear to lose), and 70% is taken by the combination of all active gears: bottom trawls, midwater trawls, and seines (*ibid*). This is emphatically not to say that there are twice as many bottom trawls in active use as there are gillnets, nor that there is seven times more active gear than passive gear in the world's oceans. After all, active fishing gears were developed precisely because they tend to catch far greater quantities of fish per haul than passive gears. Given this inherent disparity in catch volumes, it is quite possible that there are, catch statistics notwithstanding, numerically more gillnets in the global oceans than trawls and seines. However, we may affirm that, at the very least, active gears comprise a quite significant percentage of the world's fishing nets. Moreover, their use has steadily increased since the 1950s, and has done so at a faster rate than the use of gillnets, whose proportion of the global catch has actually shrunk during that time (*ibid*). Drifting remnants of netting from active fishing gears therefore represent a potentially very widespread problem around the world, and one of growing importance. While ghost fishing by derelict gillnets is a highly unsettling and therefore attention-grabbing phenomenon, it may be a smaller problem than the profusion of trawl and seine netting fragments.

The almost unavoidable fragmentation of trawling gear upon dereliction makes identifying it a very difficult undertaking. Instead of studying a complete net, whose entire design is immediately laid bare to the eye, the researcher must make due with a shred of recovered netting, from which he or she is forced to somehow extrapolate the design of the larger, originating net itself. The missing portion of the recovered net—typically much larger than the recovered fragment—must be logically reconstructed from the scant amount of evidence contained in the fragment. In this sense, the identification of derelict trawl netting requires a prior kind of *forensic reconstruction*, in which the part is used to recover the logic and structure of the whole. There is no immediately obvious way of performing this task.

Still, fragmentation would not present such a formidable obstacle were it not for the great complexity of most active fishing gears. Unlike gillnets, which are typically composed of uniform sheets of netting strung vertically between a head rope and a footrope, active gears such as trawl nets normally have a large and highly variable number of components. Though all trawl nets are roughly cone-shaped, the exact curvature of that cone and the number of sheets of netting used to construct it can vary greatly (Figure 1.5). So too, can the mesh size employed in each constituent sheet of netting. Whereas the mesh size of a typical gillnet corresponds directly to the species it is intended to catch, and is often mandated explicitly by fishery regulations, in trawl nets it is only the mesh size in the narrowest end of the cone (known as the codend) that corresponds to the body size of the target species and is subject to regulation. The size of the mesh openings (or "lumens") used in the remainder of the net can and does vary enormously. So while a quick measurement of the mesh size in a derelict gillnet can reveal, with a high degree of certainty, the fishery for which that net was intended, the same cannot be said of trawl nets. Upon recovering a piece of derelict trawl netting, one can generally say neither what part of the trawl that fragment is from, nor what precise factor (such as the body size of the target species) accounts for its design. Although Fridman (1973, 1986) has repeatedly suggested that trawl designs reflect the requirements imposed by three general factors—fishing vessel, target species, and fishing grounds—his formulation remains rather vague and has in any event never been rigorously tested.

Fragmentation and complexity are problems that will bedevil any taxonomist of derelict trawl netting. The way in which trawl nets are built and then lost assures that this is so. They are unavoidably large, complicated objects from which comparatively small pieces tend to be lost. By contrast, the third complicating factor, multiplicity, is far more unique to the NWHI. In most areas of the world where ghost nets have been studied, the number of potential source fisheries is very low.

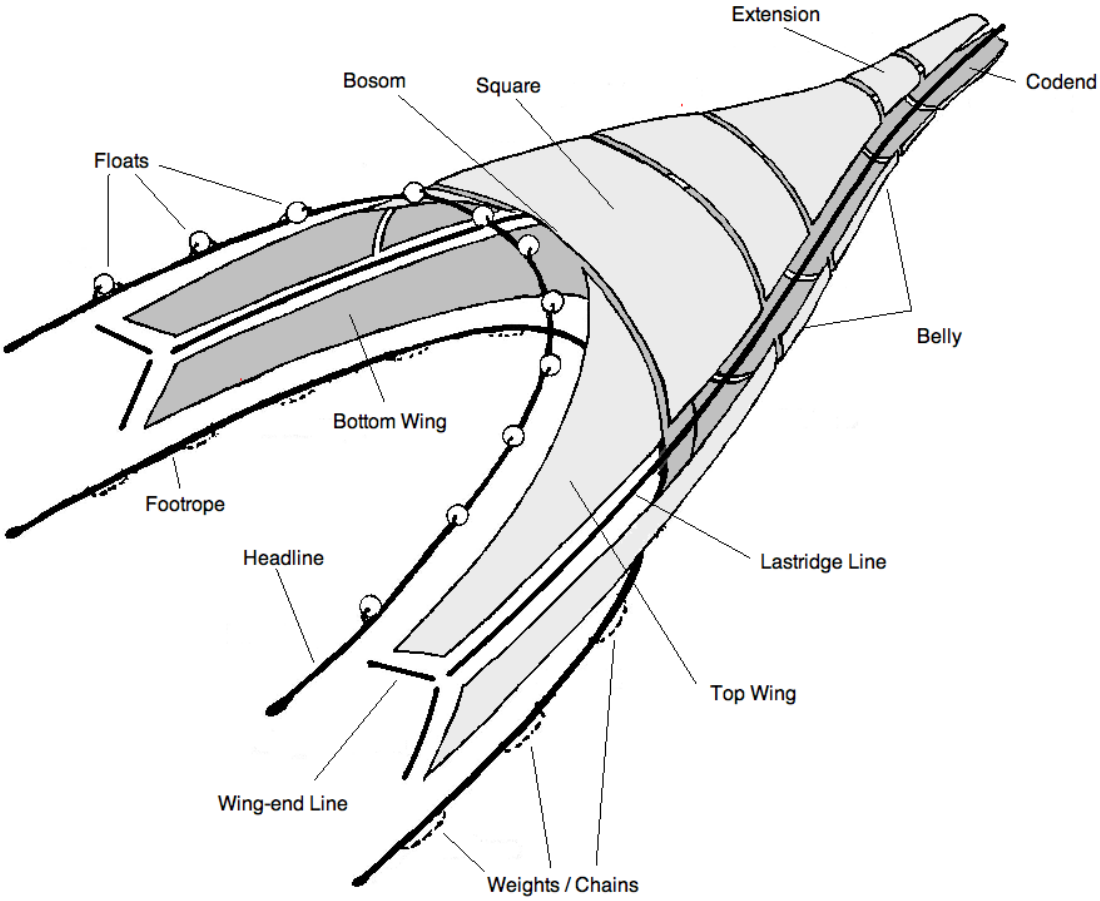


Figure 1.5 Exploded view of a basic two-seam trawl net. (Image modified from Hameed and Boopendranath, 2000.)

It is not especially difficult to determine which nation or which fishery is responsible for a derelict gillnet recovered off the northern Norwegian coastline (Langedal & Drivenes 2009), or in shallow nearshore waters off the Portuguese Algarve (Santos *et al.* 2003b), or even in deep, international waters along the continental shelf north of Ireland (Hareide *et al.* 2005). The ocean currents of such areas, and their position relative to any pertinent fisheries, make source identification of derelict nets a fairly straightforward task. Any derelict fishing gear discovered in them has a very limited number of potential sources. In such cases, a quick process of elimination may be enough to assign a derelict net to a source fishery, even if the researcher has little in-depth understanding of the nets in question.

By comparison, the North Pacific Gyre transports derelict nets to the NWHI from all across the North Pacific Ocean. The number of potential source fisheries for any piece of derelict gear recovered in the islands is exceedingly high. Instead of being asked to assign a derelict net to one of three or four possible source fisheries, the taxonomist must choose between dozens of them. To grasp the difficulty of this task, one need only imagine the difference between a standard multiple choice question, with three or four possible answers, and a multiple choice question with dozens of choices, some of them worded nearly identically to each other, others of them scarcely legible at all. The process of elimination will not work in such a scenario; there is no shortcut to identification. Choosing the correct answer simply requires a good working knowledge of all the possible choices. Yet scarcely anyone is even loosely familiar with all of the nets used around the North Pacific, let alone so well versed on their specific designs and constructions as to be considered an expert. In this sense, source identification in the NWHI requires the analysis of an essentially open system, in which the potential for novelty can never be discounted and even the number of possible answers is unknown. The only remotely similar example in the current ghost net literature involves the Gulf of Carpentaria in northern Australia, a region that, like the NWHI, is exposed to circular currents that transport derelict trawl nets into the Gulf from international waters in the Arafura Sea (Kiessling, 2003). However, even there the number of potential source nations is rather low, restricted mainly to an Australian prawn fishery, an Indonesian snapper fishery, and probable (though undocumented) activity by a few other Southeast Asian fishing fleets. By contrast, the NWHI dereliction problem, with its sheer multiplicity of potential source fisheries, is extraordinarily complex.

The study of trawl netting in the NWHI therefore represents a significant departure from previous and current studies on derelict fishing gear. It must contend with serious analytical obstacles—fragmentation, complexity, and multiplicity—that transform net identification from a potentially simple task into a complex process of forensic reconstruction. This process quite simply has no precursor in previous net dereliction studies. Indeed, there is some irony in the fact that most ghost fishing studies do not actually contain (or seem to require) much of an analysis of the derelict fishing nets themselves. The nets often appear as fairly self-evident objects whose identity is never even in question. It is generally immediately clear what species they were designed to catch and, therefore, what fisheries they come from. Neither their design nor its relation to their source fishery requires much analysis or explanation. By comparison, source identification in the NWHI is a relatively labyrinthine procedure. The ultimate goal—using the physical characteristics of an entire trawl net to deduce its source fishery—probably cannot be directly achieved through the cursory examination of a single small fragment of netting. An additional step is required, in which the recovered fragment is first employed as a piece of evidence with which to logically piece back together the remainder of the originating trawl. Only then can the trawl itself be identified and assigned to a particular fishery.

It is difficult to overstate the importance of this additional step. As I have tried to make clear, forensic reconstruction introduces an entirely novel set of problems and an unprecedented degree of complexity into the task of net identification. These additional quandaries are not somehow ancillary to the identification of trawls. They cannot simply be circumvented or set aside for later consideration. Quite the opposite: they occupy a crucial and unavoidable bottleneck in the chain of logic that underpins and legitimates all net identification efforts. Indeed, we can easily pinpoint their position in that chain of logic. In its condensed form, the logic of source identification looks like this: (1) If we wish to reduce the dereliction of trawl nets in the future, then we must implement appropriate *preventive measures*; (2) implementation of preventive measures requires that we correctly *target* them at the specific problem fisheries where dereliction frequently occurs; (3) targeting requires that we gather a basic amount of *geographic information* about the originating fishery of each recovered net; (4) basic geographic information can be substituted with *functional* *information* about a net's intended target species, since fisheries are geographic spaces defined almost entirely by their functional parameters; (5) and finally, each net's function can be inferred from the proxy measure of its *technical design*. The rationale of this chain seamlessly links prevention to policy action to targeting to geographic tracing to function to design. Indeed, if these connections are anything less than seamless, then the entire logical framework of targeted prevention threatens to come apart. Forensic reconstruction—using the design characteristics of a recovered fragment of netting, along with its presumed functional relationships to other parts of the trawl, to reconstruct the original design of the entire net—clearly occupies the final two segments in this chain. It revolves around the relationship between function and design.

The assumed transparency of this relationship is put to the test by the questions that forensic reconstruction inherently raises. For instance: Is it really possible to surmount the obstacles of fragmentation, complexity, and multiplicity and successfully read a thing's intended function through a part of its design? If trawl designs *are* functionally legible in this way, than what methods should we employ to read their functional signatures? Even more basically, what would such signatures even look like? What kinds of information can plausibly be extracted from a fragment of netting, and how reliable is that information? These questions are admittedly quite basic. However, they are also crucial. If trawl designs do not contain a legible functional signature, if the relationship between design and function is muddied or unclear, then efforts to identify trawl fragments based on their design characteristics are logically unjustified.

An assessment of trawl design and function should therefore be the starting point upon which all source identification efforts build. Despite this, no such assessment has been carried out. The following chapters attempt to rectify this situation. They constitute a basic but badly needed assessment of the relationship between trawl net function and trawl net design, and they accordingly serve as a basic evaluation of the logic informing NOAA's current trawl net identification efforts. We might say that the goal of this study is twofold: to assess the feasibility of using trawl net design features to identify trawl net sources, and to evaluate whether it therefore makes sense to dedicate additional resources to the development of NOAA's net identification database. This assessment is clearly a task for geographers: NOAA is, after all, grappling with the fundamentally geographic problem of where derelict nets in the NWHI are coming from. In attempting to solve this geographic conundrum, it has dedicated a considerable amount of effort to the pursuit of a solution that may or may not be advisable. In particular, NOAA's source identification efforts rely on a little-scrutinized geographic claim about technical objects, or at least about the technical objects in question: that trawl nets can be used as geographic proxies; that their technical designs can reliably convey geographic information about their origins. The chapters that constitute this dissertation, even as they are driven by highly pragmatic concerns about policy and geography, therefore must also delve into relatively esoteric questions about technical function and design. Their ultimate focus is on the policy challenge of preventing net dereliction and the geographic challenge of net traceability. Yet, we might say that trawl dereliction in the NWHI poses, to policy-makers, a peculiar set of geographic problems whose resolution requires an initial, in-depth analysis of trawls' technical function and design.

Something curious will occur when we set out to conduct this analysis. In attempting to resolve this initial set of problems we will be led to a second set whose existence was not especially obvious at the outset. Our pragmatic attempts to gauge the traceability of trawl nets will deliver us to the edge of an unexpected theoretical lacuna, an unheralded gap in the way we think about technical function itself. This rift is not separable from the practical study of trawl net traceability, and it in fact constitutes a formidable barrier to the completion of this study. In other words, failure to bridge this disjuncture will make it exceedingly difficult to say anything at all about the traceability of trawl nets. In order to build a foundation for the identification of derelict trawl nets, we will therefore need to venture into this theoretical *terra incognita*. In sum, while the analysis of trawl design is primarily a practically motivated and practically necessary task, it will also require some unanticipated theoretical work. The flipside to this, of course, is that it will also pay unanticipated theoretical dividends.

1.4 The Problem of Technical Assemblage

What is the unforeseen theoretical lacuna to whose very edge we are impelled by the assessment of trawl net traceability? As a first approximation, we might say it involves the problem of *technical assemblage*. I will describe this problem in greater detail below. To begin with, though, I should explain that it revolves around our everyday *mereology* of technical objects: in other words, our way of thinking about how a technical object's parts are related to each other and to the object as a whole. This is admittedly not something that most of us pause to consider very often. Technical mereology is not exactly a topic for dinner table conversations, nor something about which we tend to make conscious or deliberate decisions. Yet whether we recognize it our not, must of us tend to carry around a fairly specific, shared conception of how a technical object's component parts fit together to form a well-oiled functional whole. This popular mereological presupposition, which seems so natural and unobjectionable that it slips by quite unnoticed in most instances, becomes quite conspicuous when we really examine the forensic reconstruction of trawl nets. After all, those reconstructive efforts hinge precisely on the nature of the relationship between each recovered part of a trawl net and the missing whole from which it was torn. And unless that relationship takes a specific form, described by the mereological presupposition to which I have alluded, forensic reconstruction quite simply cannot proceed.

In order to explain this technical mereology, let us begin with the parts. At the heart of forensic reconstruction lies a relatively straightforward premise about the parts of any functional whole: it is an assumption that each component part of a complex functional object contains physical clues about the other parts to which it is materially or energetically connected. As long as these clues are recognized and correctly deciphered, then (the assumption goes) we can deduce from one part of the object the existence of other related parts. Even if those other parts remain unseen, their existence can be taken as logically necessary, given the clues at hand. It is this thin web of logical interconnections that makes forensic reconstruction possible. When a fragment of trawl netting is pulled from the water and inspected, its physical characteristics must be taken as evidence for the existence of a certain set of neighboring parts, each of which imply the existence of still other parts, and so on. Successful reconstructive efforts are able to proceed only through this sort of continual propagation, in which each part defines and is defined by a set of functionally necessary relations with other parts. Each part is, by design, inextricably bound up with the other parts to which it was connected. Each part makes sense only in relation to the others.

Indeed, forensic reconstruction is necessary precisely because of this inextricable web of interconnections. No single part, on its own, makes the object's overall function legible; none contains within itself the functional signature that would identify the entire complex object. Rather, it is only all of the parts *taken together* that carry out and indicate the object's overarching function. This means that the functional object cannot be broken down into discrete pieces that are functionally legible on their own. It resists functional decomposition. Instead, it constitutes a sort of organic functional unit, an indivisible and perfectly self-contained operational whole. Because each part reveals information only about its relations to other parts, and not about the overall object, we can say that it is entirely constituted by its relations to other parts within the seamless functional whole. In other words, the parts are defined by their *relations of interiority* within the object. This mereology, this peculiar notion of internal parthood and organic wholeness, has its basis in traditional notions of anatomical or corporeal organization (DeLanda 2006). It is based, in other words, on an extended organismic metaphor. It assumes that the complex functional object operates and sustains itself in a manner analogous to that of the living biological body.

This underlying biological comparison is seldom stated explicitly. Indeed, it is difficult to imagine anyone trying to claim, in this day and age, that most technical objects function in a manner akin to the living body. And yet, we still tend to implicitly view technical objects—and especially complex machines—as organic, functional wholes whose components are each defined by their logical place in the overall dance of cooperating internal parts. We still tend to view technical components as perfectly arranged, precision-crafted pieces whose every interaction has been meticulously planned out in advance. Components in this view do not have separate lives of their own. They do not have other external relations. Their role in the technical object's overall function does not take the form of a hobby, something that they *also* do, in addition to their other roles outside of the object. Nor do components tend to be easily substitutable, one for the other. Each has an incredibly specific role within the complex whir of moving gadgetry all around it, a role for which it has been painstakingly designed and constructed. So even in the absence of explicit organismic metaphors, we still define technical components by their constitutive relations of interiority. And we still think of the technical object as the enclosed functional whole within which such an interior can exist.

The organismic notion of technical objects, in the web-like perfection of its internal design and the resultant seamlessness of its overall function, also implies a marked infallibility of *technical mechanism*. In other words, within such an exquisitely organized and perfectly unified technical object, a single set of controlled inputs always leads to a single set of controlled outputs. A coffee grinder reliably pulverizes coffee beans into a powder of the desired consistency. A battery converts potential chemical energy into a given level of electrical current on demand. A carburetor uses a throttle and certain conditions of airflow to create an optimally combustible blend of fuel and air. In every case, the causes and effects of technical function are infallibly aligned into a precisely choreographed dance. Causation here is perfectly linear: the same material causes lead to the same material effects every time that an object is used. This causal linearity means that technical function, in addition to being wholly predetermined, is wondrously stable as well. A given technical object's function can be repeated *ad infinitum* with the same results always accruing from the same inputs. Its internal mechanism does not tremble or lurch, does not produce variable results. It simply performs its task, identically, each time it is asked to do so.

In the utter certainty of its outcome, its perfect causal linearity, this notion of technical mechanism effectively allows us to collapse form into function. After all, if a given technical object always performs its intended function quickly, cleanly, and correctly, it is somewhat redundant to refer both to the physical object itself and the technical results that are desired from it. To mention the object is already to refer to its successfully completed function. Accordingly, in everyday parlance the technical object can be used as a stand-in for its intended technical results. Rather than talking about the desired technical effects, we are freed to talk about the technical objects themselves, as though they were one and the same. With this rhetorical sleight of hand, technical function itself, the essential causal relationship within the technical object, vanishes entirely from our field of view. The object we are left with is for all practical purposes magical: even with no mechanism in sight, it simply makes its desired effects appear, on demand, out of thin air, as if by some sort of prestidigitation.

This willful folding together of technical form and function has found its most elegant expression in Bruno Latour's famous assertion that technical objects normally operate as *black boxes* (Latour 1993, 1999). According to this formulation, everyday technical objects are entirely opaque devices whose precise internal workings we can simply ignore, *as long as they are functioning correctly*. In other words, if the objects are producing their desired technical effects, then we have little motivation to question how those results are achieved. It is only when technical objects begin to malfunction, when they cease to perform their assigned work—or, what is the same thing, when their legitimacy is subjected to some kind of serious challenge—that their lids can be pried off and their internal machinations can be subjected to scrutiny and doubt. Absent such a "trial of strength," the internal mechanisms of technical objects are assumed to be pristine, perfect, and inviolable.

This is not to suggest that technical objects are always perfectly stable in the black box view, for things in Latour's world can and do break down. Indeed, they often produce spectacular social fireworks as they do so. Yet even in the midst of such pyrotechnics, there is a subtle way in which the sanctity of technical mechanism is preserved. For, the doubt and uncertainty that constitute a Latourian breakdown of the technical object do not characterize correctly functioning objects, but only *malfunctioning* ones. In Latour's formulation, if cause and effect become anything less than mechanistically assured, if apprehension enters the picture at all, then the object is already malfunctioning. It is already rocking violently within its casing. It must be opened up and examined precisely because it has already begun to make a horrible noise, has already begun to release tendrils of smoke from between its rivets. Almost by definition, then, technical function is the antithesis of uncertainty: the latter already means vulnerability, weakness, and malfunction. There is linear, mechanical causation, in the sense of specific causes leading to specific effects every time, and there is a total breakdown of technical function, but there is no in-between.

So deeply ingrained is this notion of a linear, mechanistic, and essentially infallible technical function, that it is difficult to imagine how one would even go about repudiating it—or indeed, why one would do so, given the accuracy with which it seems to describe the things around us. Technical objects *do* seem to operate with utter reliability. Their functional outcomes *are* wholly predictable and predetermined, unless the objects in question are broken. Indeed, these certainties of cause and effect are the very features that allow us to define them as technical in the first place. It would make little sense to speak of a technical object that successfully functions even while failing to properly elicit its intended technical effects from its technical causes. It is unclear whether such quasi-functional objects could even be legitimately described as technical. At best they might qualify as technical objects of exceedingly poor design and build. It is safe to assume that they would quickly be cast aside and replaced with better-designed (*i.e.*, more reliable) alternatives. In this sense it is clear that the interrelated technical notions of linear causation and organismic mereology are not necessarily wrong. Indeed, they seem to very accurately describe reality.

However, as Manuel DeLanda (2006) writes, there is an alternative way of looking at complex entities in the world, and one that is becoming increasingly difficult to ignore. As he notes, it is increasingly uncommon in the social sciences (including geography) to analyze complex objects as organic unities whose component parts fit perfectly together to form indivisible functional wholes. Organismic metaphors, in other words, have entered a period of steep decline. Instead, drawing on the philosophical work of Gilles Deleuze and Felix Guattari (e.g. Deleuze and Guattari 1987), a growing number of geographers and other social scientists have begun to treat complex entities as *assemblages* whose heterogeneous components may interact in important ways but are not reducible to those interactions.

Deleuze and Guattari's exact formulation of assemblages is, for a number of reasons, particularly difficult to codify. Firstly, it does not occur in a single block of text or even a single work. Rather, it is scattered, archipelago-like, across their voluminous writing. Secondly, it relies on a complicated set of heuristics that was developed by the authors over many years. Finally, it is subject to the highly performative nature of Deleuze and Guattari's writing, which tends to derail any straightforward attempts at textual analysis. Unsurprisingly, then, there has been little agreement among social scientists on what, precisely, constitutes an assemblage and how, exactly, the term should be gainfully employed. Indeed, in recent years the word "assemblage" has been widely taken up to describe almost any relational network that employs multiple logics of coherence and whose linkages exhibit some degree of emergent stability over time (e.g., Shaw et al., 2010; Lovell and Smith, 2010; Bickerstaff and Agyeman, 2009; McFarlane, 2009; Parker, 2009; Watson and Huntington, 2008; Barnes, 2008; Ong, 2006; Dant, 2004). Amid this profusion of fast and footloose references, DeLanda has done more than any other to painstakingly interpret, clarify, and distill Deleuze and Guattari's complex thoughts on the matter into a succinct and well organized *theory of assemblages*. For this reason—*i.e.*, for clarity's sake—it will behoove us to examine assemblages through the basic tenets of Delanda's theory, many of which stand in direct contrast to the aforementioned underpinnings of the organismic metaphor.

DeLanda's first point is that the component parts of assemblages, in contrast to those of an organic totality, are not defined entirely by their inclusion in a particular whole. Their existence cannot be entirely reduced to the relations of interiority that link them to the other components within a highly integrated functional object. While the parts of an assemblage do interact with each other in specific ways that cumulatively produce the characteristic effects of the overall assemblage, each part can just as well be detached and inserted into another assemblage, in which its interactions will necessarily be different. In this sense, the exact relations in which a component part is currently involved do not exhaust its relational possibilities. Its existence cannot be boiled down to a particular set of interior relations that are utterly dependent on its inclusion within a particular whole. Rather, each part is involved in *relations of exteriority* with the other parts. Each part is essentially autonomous from the others and from the whole, to whose design it is never fully subjugated. Each exceeds in some way the role it currently fulfills within a given object. Accordingly, the defining features of an assemblage cannot simply result from the accumulated *properties* of its component parts, for these properties are always infinite, unexpressed, and to some degree unknowable. Instead, an assemblage's defining features result from the *capacities* of its parts, as they are expressed in specific and limited interactions with other parts. In other words, just because a part is currently doing one thing does not mean that, under the correct circumstances, it could not be doing something else entirely.

The exteriority of relations affects the way in which assemblages are structured. In an organic totality, the functional whole is inconceivable without each and every one of its precision-crafted parts. If the whole is to retain its perfection, its remarkable functional unity, then each component simply cannot be other than it is. Each individual part is over-determined by its co-constitutive relations with the other surrounding parts; each is logically necessary in relation to the others. By contrast, the parts of an assemblage are highly replaceable by other, possibly quite different parts. The overall constellation of components in an assemblage is not determined by any kind of logical necessity. Rather, it is largely the result of the historical contingencies that brought specific parts into relation with each other (DeLanda refers to these as *contingently obligatory* relations). This means that the structure of the overall entity is not some stationary puzzle that can be pieced together through prolonged contemplation and logical deduction. If components can freely leave one assemblage and join another, if they assemble themselves in different ways according to the whims of historical happenstance, then structural patterns are never fixed in advance. They are never simply there, timeless, waiting to be deduced. They must *emerge* over time through repeated iterations of the assembly process. So assemblages are always the product of recurrent processes of assembly, in which the potential parts are exposed to certain formative pressures, constraints, opportunities, and so on. This leads DeLanda to one of his most striking conclusions: If assemblages are based on recurrent processes, if they are continually made and remade anew, they must emerge from *populations* of parts. Assemblages are structures that arise, in real time, as patterns in the ongoing interactions between populations.

Accordingly, an assemblage always remains at least partially open to flows of energy and matter from outside. It can never constitute a hermetically sealed entity or a fully enclosed environment. It emerges only through the continual addition of new matter and new energy from without, as well as the continual dissipation of partially expended matter and energy from within. This perpetual leakiness, this constitutive permeability, greatly expands what can be understood as a causal mechanism within the assemblage. For, as DeLanda has long sought to illustrate, linear causality is only a requisite feature of closed physical systems (DeLanda 1993). By contrast, so-called *dissipative* physical systems (*i.e.*, open systems) can also be characterized by non-linear causal mechanisms. Perhaps the most obvious examples of non-linear causation are thresholds, points at which a given physical system, subjected to a steadily increasing amount of stimulus, will abruptly undergo a major change of state. Thresholds show the existence, within physical systems, of certain points at which the same causal inputs will quite suddenly not lead to the same outputs: they indicate, in other words, that a single cause can have different effects. The reverse is also possible: in a dissipative system, a single effect might be achieved through any number of different causes. This is most notably the case when specific causes, rather than entirely bringing about their intended effects, only help to produce those effects. DeLanda refers to this form of partial causation as *catalysis*. He endeavors to show that mechanisms in any open system can operate catalytically, such that causes encourage but do not assure the production of their effects. What catalysis illustrates is that external processes can impinge on any given causal interaction, interfering in a manner that either prevents or consummates the intended causal relationship. Because such causation is only semi-determinate, any number of different catalysts might potentially lead to the exact same outcome. All of this has an additional, rather curious, corollary: If the relationship between cause and effect is not entirely predictable, and we are dealing with recurrent processes of interaction between populations of parts, then causation can only ever be statistically described. A given set of causes will produce the anticipated effects some of the time, or even most of the time, but certainly not every time. A mechanism's success or failure can therefore never be measured by a single iteration of the process in question, but only through numerous iterations and reiterations of that process. In an assemblage, causal outcomes are necessarily statistical in nature.

There is far more we could say about DeLanda's theory of assemblages, but these basic features suffice to underscore the radical differences that separate it from the more traditional view of complex entities as organic totalities. Accordingly, we can now return to the basic theoretical difficulty at the heart of trawl net reconstruction: the problem of technical assemblage. Put simply, it is unclear whether the above insights of assemblage theory can be brought to bear at all on technical objects. There is little evidence in the current literature that notions of emergence, contingency, exteriority, recurrence, population-thinking, and non-linear causation can be applied to the organization of technical matter and the performance of technical function. In this, technical objects remain oddly aloof from some of the latest advances in the social sciences. Complex social entities such as political organizations have been productively analyzed as assemblages (Legg, 2009). So have complex ecological entities such as ecosystems (Hinchliffe, 2008). Indeed, most assemblages described in the literature cut cleanly across the nature-society divide, and might best be defined as *socionatural* systems. In this, its steadfast refusal to differentiate in any fundamental way between social and natural components, assemblage theory goes a long way towards leveling the ontological playing field of nature-society studies. Indeed, it seems to constitute a universal template through which to understand all complex functional entities. All, that is, with the exception of technical objects.

I do not wish to suggest that technical objects are somehow absent from studies of assemblages. But invariably, assemblages emerge around pre-constituted technical objects rather than amid or through them. Accordingly, even within assemblages, technical matter retains the sort of functional infallibility and structural unity that is granted to it by the organismic metaphor. Technical objects in this view are opaque units that can be swept up into larger-scale processes of emergent assembly, but which are not themselves decomposable or structurally emergent. As in Latour's formulation of black boxes, their form becomes a kind of substitute or shorthand for their intended function; their function, in turn, is treated as mechanistic or causal in an unshakably linear sense. And it is technical function that is key here. For, while technical objects can be shown to emerge (or not emerge) from lengthy social campaigns of alliance and negotiation (*e.g.*, Latour 1996, Law 2002) or from lengthy ancestral lineages of similar objects (Simondon 2007), the technical function within those objects remains a kernel of predetermined causes and effects. Its mechanism is always linear and inviolable. Accordingly, the emergence associated with technical objects is something perennially external to them. It is always the emergence of something else, in which smoothly functioning technical objects are, at least in part, involved.

This subtle form of exceptionalism is plainly recognizable in a number of recent papers on bio- or nanotechnologies, which rather than treating technical matter as something irrevocably emergent and contingent, tend to view it as a vessel of unerring control and manipulation. Genetic technologies, in these accounts, are seen as a vector for capital relations or political domination to enter into the very processes of protein manufacture that constitute the living cell (*e.g.*, Rose, 2001;

Bridge, 2003; Prudham 2007). Nanotechnologies, whether in the form of an informational dust (Thrift 2005) or embedded into intelligent fabrics (Küchler 2008), become a way of transforming molecular matter itself into an omnipresent gatherer and transmitter of information—information that humans can presumably decipher and employ as they see fit. In bio- and nanotechnologies alike, the extension of technical *control* into microscopic or sub-microscopic realms is thought to threaten the sanctity of life or being itself. These accounts easily slip into a kind of pervasive dualism, in which technology is viewed as synonymous with control, while life and/or being are defined as that which cannot or should not be subjected to control. It is an opposition that glibly equates technical matter with predetermination and life/being with an unfettered openness to contingency and emergence. Indeed, it is on the basis of this widely accepted disjuncture that writers such as White and Wilbert (2009, p. 5) can deploy the term *technonature* as a jarring or "deliberately provocative" formulation. When technology is coterminous with control, as it remains in many accounts of bio- and nanotechnologies, then "technonature" can all too easily be read as an oxymoron.

Notably, even the handful of studies that run counter to this narrative of miniaturized manipulation and adeptly challenge the notion of technical control at the cellular and molecular scale (*e.g.*, Barry, 2005; Kearnes, 2006, 2007) do not quite manage to prize open the matter of technical assemblage. For they address the notion of control—or rather, the impossibility of control—at an atomic scale that is so reduced it cannot be broken down any further. Molecular technical objects may escape our ability to control them, but they are so extraordinarily miniscule that, for all practical purposes, they still constitute absolutely indivisible functional unities. Accordingly, in these accounts the defining properties of assemblage—emergence, contingency, recurrence, etc.—continue to arise *around* the technical objects in question rather than through or amid them. What is non-linearly assembled in these studies is not the structure of nanotechnologies, per se, their functional blueprints, but rather the unpredictable relations in which they, as already fully formed and essentially immutable objects, become involved. Furthermore, it is not even clear that the uncertainty of those relations has anything to do with the molecule's technical status. Rather, it is attributed more to the basic physical impossibility of controlling any kind of molecular interaction in "the nanoscale … world of Brownian motion and atomic uncertainty" (Kearnes 2006, p.58). So even while authors such as Andrew Barry and Matthew Kearnes adroitly utilize Deleuze's and Guattari's work to challenge enduring notions of technical control at the atomic scale, they do not address the problem of technical assemblage per se. Indeed, while critical of the narrative of miniaturized manipulation, they also rely on miniaturization (and its inherent material waywardness) as a sort of analytical entry point for Deleuzian insights. Their arguments could be extended to virtually any kind of molecular matter, but not to other kinds of technical matter.

Of course, there is no particular reason to insist that technical matter conform by the rules of assemblage theory. Humans painstakingly design technical objects to perform certain tasks, and to do so in a highly efficient and reliable manner. There is no implicit rationale dictating that they cannot be designed in accordance with an organismic mereology. Indeed, it is almost certainly advantageous to do so. After all, the mechanistic reliability, precision craftsmanship, and functional unity implied by the organismic view are the very qualities that we tend to look for most in technical objects. In this sense, the more organismic a technical object is, the more utility it tends to provide and the more highly it is regarded. There are good performance-based reasons, then, to think that technical objects are in fact more organismic than assemblage-like. Beyond these, one might also argue that the organismic view appeals rather strongly to our sense of realism. Its notions of a mechanistic technical function, borne out by specific webs of unique and indispensible components, seem plausible to the point of being obvious. It bears repeating that the (closely related) notion of black boxes describes very accurately our typical day-to-day encounters with technical objects—especially our tendency to ignore them when they are working correctly. All of this suggests that, when it comes to our mereology of technical objects, we too might do best to simply black box it. If it ain't broke, this suggestion goes, then don't fix it.

However, in assessing the forensic reconstruction of derelict trawl nets it will become clear that this suggestion is off the mark. Organismic metaphors do not always provide a more accurate account of technical design and function than does assemblage theory. A closer examination of trawl nets will show exactly the opposite: that assemblage theory can and indeed must be applied to some technical objects. This is an important theoretical advance, for it indicates that technical matter does not, in the end, constitute a sort of baffling exception to assemblage theory's otherwise universal (*i.e.*, socionatural) scope. It will also shed light on the highly unusual conditions that are necessary for the emergence of properly technical assemblages—showing, in other words, why technical assemblages have remained so notably absent from the literature.

Yet the advances sketched out in this study will not only be academic. The abandonment of organismic metaphors in favor of assemblage theory, insofar as it disrupts our basic mereological presuppositions about technical objects, will necessarily alter our conception of the relationship between a fragment of derelict trawl netting and its originating "parent" trawl net. This adjustment is what we might call ontological: it concerns the very way in which we conceive of a trawl's existence as a complex functional entity. But it is an ontological adjustment with epistemological implications. In other words, adjusting the way in which we conceive of trawl nets' existence (as assemblages, not organic totalities) will greatly affect what a piece of netting can teach us about its parent trawl, and how. We cannot study an assemblage in the same manner that we would study an organic totality, especially if what concerns us is the relationship between its parts and the whole. For this reason, the problem of technical assemblage must be taken as absolutely central to trawl net identification efforts, rather than merely as some sort of ancillary theoretical exercise.

The forensic reconstruction and subsequent identification of trawl nets is a difficult task, but one that would be much simpler were, in fact, the organismic metaphor an appropriate mereological lens. I begin Chapter 2 by showing that the challenges of forensic reconstruction can be—and indeed, have been—solved through recourse to organismic metaphors and the mereological presupposition of organic functional totalities. To illustrate this feasibility, I turn to the work of the renowned 19th century comparative anatomist Georges Cuvier. In the closing years of the 1700s and the opening decade of the 1800s, Cuvier successfully performed history's most celebrated series of forensic reconstructions, reassembling dozens of prehistoric animal skeletons, many of them previously unimagined, by laboriously piecing together tiny fragments of fossilized bone. Cuvier attributed his success in this endeavor to three guiding principles that he claimed to have discovered. I will review these principles, highlighting their specifically forensic aspects, before turning to the work of early 20th-century plant ecologist Frederic Clements.

Clements, in his taxonomic attempts to identify landscape-scale patterns of vegetation, relied on the same three basic principles as Cuvier. Crucially, however, Clements added a novel dynamic element to Cuvier's highly static formulation. I will show that this slightly enlivened, Clementsian version of Cuvier's principles characterizes the technical objects of Gilbert Simondon's mid- to late-20th century technical philosophy. Simondon's work is indispensible to this study for three reasons. Firstly, because his functional mereology so resembles Cuvier's that his work offers real hope for the functional reconstruction of technical objects; secondly, because Simondon's use of organismic metaphor and organic mereology highlights how deeply ingrained the they are in our general approach to technical objects; and thirdly, because Simondon's account is detailed enough that we can see just where it goes wrong and ceases to describe trawl nets—in other words, he shows us exactly where it is necessary to move from a theory of organic technical wholes to one of technical assemblages.

Once the specific forensic criteria within Simondon's theory have been identified and explained, Chapter 3 asks whether or not they pertain to trawl nets. More precisely, it asks whether trawl nets undergo technical progress in the manner that Simondon's theory requires. If they do, then it is reasonable to expect that eventually they will avail themselves to forensic reconstruction. However, an analysis of recent design advances in trawl nets will show not only that trawl nets mostly fail to comply with the Simondonian model of progress, but also that Simondon himself is unable to account for this failure. In other words, trawl nets appear to progress in a way that differs from all the other technical objects that he studied. A closer analysis of Simondon's work will suggest that perhaps the source of this difference is not merely how trawl nets undergo progress, but rather how they carry out their technical functions. This, it is suggested, is the crucial point at which Simondon's theory derails.

Chapter 4 evaluates this suggestion by examining how it is that trawl nets actually function. While Simondon's objects operate through a specific process, called mechanical transduction, that ensures the casual linearity of their technical functions, a close examination of trawl net function will show that the basic causal mechanisms on which trawling depends are fundamentally non-linear. As behavioral devices, trawl nets function by incorporating the perceptions and reactions of living animals into their operation. Through an exhaustive review of the trawl net literature, I will show that this behavioral basis of trawling violates in four different ways the Simondonian requirement for functional linearity. Accordingly, trawl nets cannot be expected to undergo progress in a way that makes their designs amenable to forensic reconstruction.

How, then, are we to explain the organization of trawl nets? Chapter 5 argues that trawl net design is characterized by a certain kind of structural paradox resulting from the specific way in which nets are assembled. An overview of the trawl engineering process shows that the act of selecting different netting components and combining them within a given trawl is underdetermined in a number of specific ways. As a result, trawl nets show such variability that general types can be recognized only through the statistical analysis of many nets. No single net, on its own, conveys sufficient information to warrant its typological inclusion in one class of nets or another.

The chapter then pivots to study the early plant ecologist Henry Gleason and his "individualistic dissent " from the proposed vegetation system (described in Chapter 2) of Frederic Clements. It is shown that Gleason's movement away from Clementsian vegetation units was characterized by the same primary features as our description of trawl net assembly. Gleason's development of modern ecological thinking was rooted in the same structural paradox as that which characterizes trawl net design; he showed that this paradox emerged in the assembly of ecological communities due to the same underdetermined selective pressures that guide trawl assembly; and he and his successor, Robert Whittaker, showed that these features necessitated a switch from organismic thinking to population thinking. Chapter 5 concludes with the astounding recognition that trawl nets and ecological communities should be understood as a single kind of object; that they are two physically different examples of a single type of dynamical system, an assemblage, in which populations of component parts are assembled in response to the same set of unusual formative pressures. Trawl nets, in other words, are inherently ecological objects.

Chapter 6 concludes the study by returning to the question of trawl net source identification in the NWHI. It argues that because trawl nets are ecologically organized, their study is limited by many of the same epistemological obstacles that limit the study of any ecological community. In particular, it shows that multivariate statistical methods make it impossible to identify or "source" individual trawl nets with any degree of certainty. The incorrect presumption that we can, in fact, do this stems from our traditionally organismic way of thinking about technical objects—a way of thinking that is clearly off base when it comes to trawls. So while there is potentially quite a lot that could be learned by analyzing the construction of trawls, the origin of lost netting fragments is probably not one of them. Finally, this concluding chapter examines some of the broader implications that this work has for the geographical study of technical objects, particularly in relation to the work of Bruno Latour, John Law, and Annemarie Mol on the relational topologies of technical objects.

Chapter 2 . What Makes Forensic Reconstruction Possible?

The traceability of derelict trawl nets depends on our ability not just to identify them, but to forensically reconstruct them from a single fragment of recovered netting. This task, as I detailed in the preceding chapter, is beset on all sides by difficulties. However, a look back at history shows that the specific challenges of forensic reconstruction are neither unprecedented nor insurmountable. In fact, at the turn of the 19th century an undertaking nearly identical to our own met with rather astonishing success. This was the much celebrated reassembly, by Parisian comparative anatomist Georges Cuvier, of fossilized bones from the Montmartre plaster quarries into complete skeletons.

Cuvier's reconstruction of prehistoric animals seems to offer a nearly perfect model for that of derelict trawl nets. For one thing, the impediments to his work bear an uncanny resemblance to those confronting the derelict trawl researcher: The recovered material that Cuvier had at his disposal was fragmented and incomplete; the originating skeletons were dauntingly complex structures; and the creatures he sought to reconstruct were so utterly unknown that they might plausibly look like anything. In spite of such difficulties, Cuvier managed to reconstruct and identify 78 species of animal from the Montmartre quarries, including 49 that were previously unknown and presumably extinct.

Such an extraordinary record of success would make Cuvier's methods of forensic reconstruction worthy of closer examination in their own right. However, there is an additional and equally compelling reason to study Cuvier's methods: after disappearing for some 150 years, the basic tenets of his *functional anatomy* (Coleman, 1964) would reemerge virtually unchanged in the technological philosophy of Gilbert Simondon. In other words, the fundamental properties of living bodies that, according to Cuvier, enabled him to reconstruct extinct animals from scattered fragments of bone, also characterize the technical objects of Simondon's philosophical work. This remarkable convergence of thought suggests that Simondon might provide an ideal theoretical foundation for the reconstruction of derelict trawl nets. If Cuvier's system allowed him to overcome the problems of fragmentation, complexity, and multiplicity in the reconstruction of biological objects, Simondon's uncanny reiteration of that system should, it seems, allow us to do the same for technical objects.

In this chapter I therefore detail the seemingly fortuitous reemergence of Cuvier's functional anatomy in Simondon's philosophy of technical objects. My argument will be that a clear and incontrovertible line can be drawn between Cuvier and Simondon. However, I will show that this trajectory of thought passes unavoidably through the work of celebrated early plant ecologist Frederic Clements as well. Clements, it will become clear, constitutes a crucial link between Cuvier and Simondon. For, in his model of vegetational climax succession, Clements not only restates the basic principles of Cuvier's functional anatomy but also makes an important modification to the original concept—and this slight alteration is a key feature of Simondon's work. Indeed, the Clementsian modification, although exceedingly minor in many respects, has probably served to mask the profound similarities between Cuvieran and Simondonian thought. Accordingly, it is only through Clements that the seemingly antagonistic work of the two Frenchmen begins to take on an unexpected unity. There is one further reason for diverting our route through Clements. In addition to providing a critical link between Cuvier and Simondon, his model of vegetation, insofar as it eventually proved untenable, also served as the impetus for an alternative notion of complex functional objects—a notion that spawned the birth of modern ecology and that will, for better or worse, prove indispensible in the study of trawl nets.

2.1 Cuvier, a man of principles

In the waning years of the 1700s, George Cuvier, a new lecturer at the *Jardin des Plantes* in Paris (later renamed the National Museum of Natural History) stunned the European academic community and began a meteoric rise to international celebrity by achieving something that no one had thought possible. A self-identified comparative anatomist, the young Cuvier was apparently not content to study the corporeal structures of living species. Rather, with a brash self-confidence that would prove typical of his career, he set out to reconstruct the fossilized remains of prehistoric animals that were daily emerging from the Parisian plaster quarries in Montmartre.

The difficulties of such an undertaking were manifold. Chief among them was the fragmentation of the fossil material: As Cuvier wrote in the preliminary discourse to his 1812 collection of papers, *Recherches sur les ossemens fossiles de quadrupèdes*, it was "uncommonly rare to find a fossil skeleton at all perfect; bones isolated and confusedly intermingled, most frequently broken and reduced to fragments; this is all with which our layers [of fossils] furnish us in this class, and is the sole resource of the naturalist" (Cuvier 1835, p. 60). The mere differentiation and sorting of these fragments would be a terribly onerous and uncertain task—one which Cuvier himself described, without any hint of his usual rhetorical flair, as "lengthy and laborious" (Cuvier 1980; Rudwick 1997, p. 63).[6] Furthermore, even were such a sorting accomplished, it remained unclear how the resultant piles of bone shards could possibly be reassembled into accurate skeletons: after all, skeletons were by their very nature complex and disarticulated structures, and no one had any real idea what the animals of prehistory looked like. Reconstruction would have to proceed with no template to serve as a guide.

Despite these deterrents, Cuvier forged ahead. His results, when they finally arrived, were nothing short of remarkable. From a collection of mineralized bone shards, themselves indistinguishable even to most trained anatomists, Cuvier pieced together an entire prehistoric fauna, whose ossified members ranged from the pedestrian to the bizarre. Most of the creatures

---

[6] This description is given by Cuvier in the 'Septiéme Mémoire' of his *Memoirs on Reconstruction of the Genera Palaeotherium and Anoplotherium*, a work which was ultimately included as the third volume of his (1812) *Recherches sur les Ossemens Fossiles du Quadrupèdes*. The latter has been reprinted in the original French by Arno Press (see Cuvier, 1980). I am using Rudwick's translation of this short phrase.

emerging from Cuvier's laboratory were immediately recognizable, if rather oddly out of place: Frenchmen were surprised to learn that their land had once supported exotic creatures such as tapirs, opossums, hyenas, mastodons, rhinoceros, and hippopotami. However, these relatively familiar skeletons were accompanied by other, stranger ones that bore little resemblance to any known animal. Astonishingly, even these were promptly identified by Cuvier though anatomical reference to their nearest living relatives. For example, he demonstrated that the pair of enormous fossil skeletons recently discovered in the New World, Megalonyx and Megatherium, were the remains of giant ground sloths, a creature so unlikely to the imagination that its possible existence had never even been fathomed.

Cuvier claimed that these reconstructive achievements could, like all of his discoveries, be attributed to his possession of certain basic principles of anatomy whose precision and consistency gave them the character of scientific laws. He seldom missed a chance to tout the power and precision of these principles. Taken together, Cuvier insisted, they necessarily led the anatomical researcher to the inescapable conclusion that

> The least prominence of the bone—the smallest apophysis—has a determined character relative to the class, the order, the genus, and even the species to which it belongs; so that whenever we have only the extremity of a well-preserved bone, we may, by scrutinizing it, and applying analogical skill and close comparison, determine all these things as certainly as if we had the whole animal. I have often in this way experimented on portions of known animals, before I entirely applied the test to fossils; but it has always had such infallible success, that I have no longer any doubt on the certainty of the results which it afforded. (Cuvier 1835, p. 65)

This was Cuvier's most famous and most audacious claim: that his anatomical laws enabled him to "assemble [entire animals] solely by thought", as long as he possessed a single well-preserved fragment of bone with which to begin his analysis (Cuvier, 1980; Rudwick 1997, p. 64). Though several historians of science have noted that Cuvier frequently confused his terminology, substituting the more dramatic term *reconstruction* for the more correct term *identification*, he in fact engaged in both activities (Coleman, 1964; Outram, 1986; Rudwick, 1997). Indeed, it is difficult to say which of his accomplishments was more impressive: the fact that he was able to piece together skeletons that had been shattered, strewn about, and intermixed underground, or the fact that he was able to assign those skeletons with a high degree of certainty to particular taxonomic families or genera. In any event, the tasks of reconstruction and identification were so closely related for Cuvier that he likely did not distinguish in any meaningful way between them.

Cuvier's anatomical laws consisted of just three basic principles, which he claimed to have discovered through empirical observation over many years of research. These were (1) the principle of the conditions of existence, (2) the principle of the correlation of parts, and (3) the principle of the subordination of characters. All three were so tightly intertwined that in practice they became more or less inseparable. Indeed, each refers to and depends upon the others to such a degree that they can (fittingly, as we shall see) only be properly understood as a single unified set of conditions. Cuvier himself, it should be noted, never distinguished between them in a rigorous manner, instead attributing his various accomplishments with apparent whimsy to any one or two of the principles at a time. This apparent interchangeability resulted not from any lack of rigor, per se, but rather from Cuvier's inextricably collective understanding of the three principles. We too shall consider these as a collective whole, but to do so we must first articulate the distinct contribution that each of the individual principles makes to the entirety.

2.1.1 The Principle of the Conditions of Existence

At once the most fundamental and the most frequently misunderstood of Cuvier's anatomical laws is the principle of the conditions of existence. Stated in its most basic form, it declares that, "nothing can exist without the re-union of those conditions which render its existence possible, the component parts of each being must be so arranged as to render possible the whole being, not only with regard to itself but to its surrounding relations." (Cuvier 1831, v. I, pp. 3-4). This has often been interpreted as an overtly teleological statement, since Cuvier seems to use the ultimate result of a process (i.e. the completed animal body) as the initial cause or guiding hand of that process. He is, in other words, guilty of circular reasoning: the reason that an animal exists in a certain form is because it has a certain form that makes its existence possible. It is in this sense that Cuvier has been labeled "a teleologist in the most literal manner" (Coleman 1964, p.43). This accusation seems valid at first glance. However, it deserves closer examination.

If the claim of teleological reasoning holds true, then Cuvier's system of comparative anatomy should explain animal form by resorting to some external plan. This is so because if the end result (a rationally organized animal body) is able to somehow guide the ongoing process (animal organization), then the result must be known in advance. There must accordingly exist some external planner who coordinates and guides the process. In other words, the conditions of existence will necessarily constitute a principle of exteriority. Indeed, as an initial approximation this proposition seems to hold true. Cuvier was deeply concerned with the living conditions and natural surroundings of each animal he studied, arguing that these were inextricably linked to its anatomical structure. No swimming vertebrate, for example, could have large ears, since these would impede its necessary locomotion through the water. Similarly, no predator could lack sharp claws with which to seize its prey, for it would then be unable to feed itself. These examples, and dozens more like it in

Cuvier's work, seem to support the claim that the conditions of existence were, for him, primarily external, and that the animal was designed in such a way as to fit into those preexisting conditions.

However, these arguments fundamentally misunderstand the meaning of the conditions of existence. In her authoritative study of Cuvier, Outram (1986) shows that allegations of teleological reasoning only seem to hold when Cuvier's thought is presented out of context. When Cuvier's entire body of work is considered, the arguments about his teleological reasoning quickly unravel. For as Outram convincingly argues, the principle of the conditions of existence can only be understood in relation to Cuvier's notion of *animal economy*. Perceived from this vantage point, what initially seemed to be a condition of exteriority is transformed into a condition of interiority instead.

Animal economy is a notion that stems from Cuvier's interest in the intelligibility of life itself. Like many of his contemporaries, he saw the inorganic world as a domain ruled by the ceaseless decay of matter and energy. Life, by contrast, was precisely that which was somehow able to "to resist, during a certain time, the laws which govern inanimate bodies, and even to act on all around them in a manner entirely contrary to those laws…" (Cuvier 1802, vol. I, p. 2). In other words, living things were distinguished by their ability to fend off the degrading actions of the inorganic world (i.e., death)—presumably through some ingenious though poorly understood system of counteractions.

What was striking to Cuvier was that this system of counteractions only characterized the unified animal body, and not its individual parts. No body part could survive for long if it were somehow detached from the whole:

> Th[e] general and common motion of all the parts forms so peculiarly the essence of life, that the parts which are separated from a living body soon die, because they possess no motion of their own, and only participate in the general motion produced by their union. Thus, according to the expression of Kant, the mode of existence of

> each part of inanimate bodies belongs to itself, but in living bodies it resides in the whole. (Cuvier 1802, vol. I, pp. 5-6)

A living animal was clearly something more than—but inseparable from—the sum of its parts. Indeed, this property is what made animals unique and more than mechanical. What seemed clear was that resistance to death (i.e., life) seemed to emerge from the reciprocal *interactions* of all the parts, for any part removed from those interactions would quickly fail. In other words, life required unity, but it was a peculiar sort of functional unity formed by numerous parts all acting mutually upon each other. Somehow, out of this highly varied assortment of pieces acting differently upon each other was forged the ultimate unity: life.

This, as Outram writes, is precisely the meaning of *animal economy*. It is the notion that a living being is "an interacting system whose parts work together as a condition for the maintenance of the whole in a certain form" (*ibid*, p.326). Each creature, of course, had its own specific parts and its own specific animal economy that tied those parts together to maintain life. Every species of animal therefore comprised a unique economical whole. As a comparative anatomist, Cuvier's goal was to study and define all such animal economies, documenting the numerous (though finite) ways in which life could be preserved in a body.

According to Cuvier, only one basic rule underlay the immense profusion of animal economies. He explained that, "Everything may exist which does not contain within itself the principle of contradiction" (Cuvier, n.d.; cited by Coleman 1964, p.189).[7] In the anatomical organization of body parts, "all combinations which are not contradictory are possible; in other words, everything which has a 'condition of existence,' whose parts cooperate in a common action, is possible" (*ibid*). Put another way, any organization of animal parts was permissible as long as those parts achieved unity through their interactions of mutual dependence and aid. No body could

---

[7] Here I use Coleman's (1964) translation of Cuvier's unpublished 'Essay on Zoological Analogies', which is held in the *Catalogue des manuscrits de fonds Cuvier* at the Bibliothèque de l'Institut de France in Paris.

survive long if it worked against itself, since this would produce functional division rather than functional unity.

Cuvier therefore professed that while a great variety of animal forms were possible, the number of possible forms was limited by the requirement of functional unity. Given that all of the parts of a body were involved in constant and mutual interactions, no one piece could be changed, substituted, or removed without affecting all of the other parts caught up in its functional web of relations. A single alteration to one of the body's internal components would break its overall functional unity. If that functional unity were to be preserved, the modification of a single part would necessarily trigger a whole cascade of changes in all the other parts. It is in this sense that Cuvier intended his principle of the conditions of existence:

> it is evident, that a suitable harmony between organs which act on one another, is a necessary condition of the existence of the being to which they belong; and that if any one of the functions were modified in a manner incompatible with the regulation of the others, that being could not exist. (Cuvier 1802, v. I, p. 48)

Nowhere does Cuvier mention the external imposition of anatomical requirements. In fact, his principle of the conditions of existence says nothing at all about an animal's existence in the outside world; the principle in no way arises from the detailed requirements of the animal's day-to-day life. Rather, it lays out the conditions of internal organization that allow the animal (any animal) to have an existence (any existence). As such, the principle of the conditions of existence is essentially a statement of observed fact. If the phenomenon of life is only possible through a body's "interlocking structure of functions", then the study of all life forms must take such structure as its starting point. Intent or purpose has nothing to do with it. As such, the principle is teleologically neutral.

When Cuvier's work is considered in light of his notion of animal economy, it is clear that the conditions of existence are not externally imposed conformations to a preexisting plan. Rather, what allows any animal to exist is its unification through the mutual interactions of its internal parts. The unique arrangement of these parts is what enables each animal to resist degradation and death, to hold itself together against all odds, to form a self-preserved unity in which life becomes possible. The principle of the conditions of existence refers to a sealing off from the outside world, the creation of a self-maintaining interiority that can resist the encroachment of the outside world with its inorganic laws of physical and chemical decay. Cuvier's first principle is one of interiority: a requirement of functional enclosure.

2.1.2 The Principle of the Correlation of Parts

Cuvier's other two principles follow directly from the principle of the conditions of existence, and have sometimes been called its corollaries (e.g., Coleman 1964, p.67). The first of these corollaries—Cuvier's second principle—is known as the principle of the correlation of parts. It is in many ways a translation of the conditions of existence into explicitly structural terms. If, as Cuvier maintained, every part within the living body interacts with the other parts through a dense web of functional interrelations, then every body part is necessarily tied to many others. Presumably, each body part helps to fulfill one main function, and its form will necessarily be shaped by that primary functional role. But it is also functionally related to numerous other parts, so its own form will to some degree reflect their forms as well. If the other parts undergo some kind of structural modification, those physical alterations will necessarily be reflected in its own form. Conversely, if the body part in question is modified in any way, its physical adjustment will propagate out through the web of parts to which it is functionally related.

This notion is, of course, implicit in the principle of the conditions of existence. But through the principle of the correlation of parts, Cuvier makes it explicit and accords it tremendous diagnostic power. It was the correlation of parts, he often touted, that had enabled him to overcome the bewildering fragmentation and complexity of the fossilized Montmartre skeletons. These difficulties might have threatened to derail his work, but "Fortunately, comparative anatomy possesses a principle which, properly developed, was capable of clearing up all embarrassment: it was that of the natural relation of forms in organized beings, by means of which each sort of creature may, by rigorous scrutiny, be known by each fragment of each of its parts" (Cuvier 1835, p. 61)

Cuvier never revealed how the principle was employed in practice, though he hinted at his methods in an early (1798) paper on the bones of the mysterious "Paris animal". When analyzing fossil bones, Cuvier wrote in this essay, it should be remembered that

> by the number and position of their articulating facets one can judge the number and direction of the bones that were attached to them. This is because the number, direction, and shape of the bones that compose each part of an animal's body are always in a necessary relation to all the other parts, in such a way that––up to a point––one can infer the whole from any one of them, and vice versa. (Cuvier 1798; cited in Rudwick 1997, p.36)[8]

One needed only to study the number and position of a bone's articulating facets to understand what else it was attached to, and how. This is about as specific as Cuvier ever got when it came to explaining how one could use the principle of the correlation of parts. In his *Essay on the Theory of the Earth* he lamented that there was not sufficient space to "enter into a more lengthened detail of this method" and promised to explain and illustrate all its laws in a "large work on Comparative Anatomy, which we propose to publish very soon" (Cuvier 1978, p.102). However, no such methodological work was ever published.

In place of such explanations, Cuvier often chose to illustrate the larger implications that his second principle held for the overall organization of one kind of animal body or another. For example, he pointed out that the body parts of any mammalian predator would have to be closely

---

[8] I have gratefully used Rudwick's translation of this manuscript.

adapted to the task of hunting down, seizing, and digesting other animals. Such a predator would require sharp grasping teeth. These would in turn require strong, solid roots, which could only be anchored in a massive jawbone. The jaw itself could only successfully grasp its prey if it was attached to large temporal muscles. These would require not only a deep hollow in the mandible where they could attach themselves, but also a set of cheekbones with sufficient arch to allow large temporal muscles to pass underneath. The joint between the upper and lower jaw would also require a specific arrangement capable of producing powerful physical leverage. Of course, the ability to grasp prey in one's jaws would be useless if not paired with the ability to carry that prey off. Therefore, sharp teeth also necessitated the existence of strong muscles to elevate the head. The attachment of those muscles required the base of the skull and the vertebrae in the neck to have a distinctive shape of their own. The act of seizing prey also required strong claws and unusual mobility in the toes, paws, and forearms. Such dexterity implied a specific arrangement of bones in the leg and shoulder. This cascading correlation of structures continued into the predator's torso, which must be unusually flexible, and its hind limbs, which must be built for speed, and even up into its orbital and nasal bones, which must be structured to provide acute senses of vision and smell. "In a word," Cuvier insisted,

> the formation of the tooth bespeaks the structure of the articulation of the jaw, that of the scapula, that of the claws, just as the equation of a curve involves all its properties; and in taking each property separately, as the basis of a particular equation, we should find again both the ordinary equation and all the other certain properties; so the claw, the scapula, the articulation of the jaw, the thigh bone, and all the other bones separately considered, require the certain tooth, or the tooth requires them reciprocally; and beginning with any one, he who possessed knowledge of the laws of organic economy, would detect the whole animal." (Cuvier 1835, p. 62)

A single sharp tooth or claw or uniquely shaped occipital condyle implied predation, and predation, in turn, implied all of the other anatomical traits.

This illustration, and others like it, might easily lead one to misinterpret Cuvier's second principle. For it seems from this example that the predatory function of the animal ties together and shapes a constellation of body parts that is unique to that function. Predation requires a bone shaped like *x* here, a tooth shaped like *y* there, and a claw shaped like *z* over there. Body parts are like the perfectly formed links in the chain of a specific function; better yet, they are like perfectly unique pieces of popcorn strung out on the impossibly thin thread of function. The parts do not make sense on their own. They are incomplete and useless by themselves. They become meaningful only when connected by the rigors and demands of active predation. Predation gives them the unity they otherwise lack.

The problem with this interpretation is that predation seems to hold a monopoly over the functionality of the jawbone, the condoyle, the cheekbone, the vertebrae, the occiput, the scapula, and so on. All of the parts that are involved achieve their unity through the predatory function. Of course, other parts scattered throughout the animal's body might achieve their unity through a different function of the animal—nesting, for example. If there are enough such functions, no part need be left out in the cold. However, as we have already seen, the conditions of existence do not allow specific bodily functions to provide any unity of their own. If each function unifies its various parts, and if conversely each part is explained by a single function, then the animal body becomes a collection of separate functional unities hopelessly divided from one another. As we have already seen, this situation is not possible: for Cuvier, a body divided against itself is a nonliving body.

The root of this mistake is an erroneous assumption that predation (or any other daily activity) constitutes one of the functions of the animal. This is to teeter back into teleology, confusing an animal's existence with its conditions of existence. The functions that make life possible are not the daily activities of the predatory way of life, but the internal bodily exchanges of fluid and energy that characterize the organs, tissues, and so forth. Circulation, respiration, regeneration, digestion, sensation, locomotion, and excretion: these, for Cuvier, are bodily functions. Predation is not. Accordingly, the correlation of structural traits that is afforded by predation is not the result of a single function. The correlation of tooth, jaw, condyle, orbit, and scapula still holds, but the chain of anatomical structures that they form is crossed by numerous functions. In the example I have already cited, the predator's sharp teeth are involved in digestion, its shoulder blades are involved in locomotion, and its nasal bones are involved in sensation.

The functional monopoly must be relaxed even further, however, for each individual part of the predator's body is also crossed by multiple functions. It may be true, for example, that the nasal bones are involved in sensation (smelling). But if the nasal bones help to support the muzzle and the teeth then they are also involved in digestion. And if the animal breathes through its nostrils as it prepares to pounce on its prey, than the nasal bones are involved in respiration as well. All of these functions jointly influence the form of the nasal bones. So instead of picturing each body part as a unique piece of popcorn threaded onto a single function, we might envision each part as a structure that is reticulated by the threads of multiple functions. The correlation of parts runs in many directions at once. This is why a single part, "separately considered, points out and marks all the others"—rather than only indicating and giving those parts involved in its specific bodily function (Cuvier 1835, p. 61). It is the reticulation of each part by multiple body functions that makes the principle of the correlation of parts so diagnostically powerful. Each function is spread out across numerous pieces, and each piece is involved in multiple functions.

2.1.3 The Principle of the Subordination of Characters

The principle of the correlation of parts solved many of Cuvier's problems—or so he claimed. But it also introduced an unexpected problem into the act of anatomical identification. If every body part were reticulated by numerous functions, which were subtly expressed in that part's protuberances, dimples, facets, and so on, it became unclear which of these many structural variations should be given the most importance in reconstructing and identifying the rest of the body. If a certain uniquely shaped bone yielded information on the animal's nervous system, circulatory system, respiratory system, and means of locomotion, which of these functions should be given precedence in determining what kind of animal was being reconstructed? A certain bone from organism X might have implications for locomotion that are very similar to those of organism Y. But the same bone from organism X might have implications for respiration that are quite unlike those of organism Y and are actually much more like those of organism Z. In this case, should the reconstruction and identification of organism X proceed based on its similarities to organism Y (locomotion), or its similarities to organism Z (respiration)? In less abstract terms, should a platypus skeleton be reconstructed according to the known properties of beavers, since it has four limbs and a flattened tail used for aquatic locomotion? Or should it be reconstructed according to the known properties of aquatic birds, since it has a bill to aid in digestion and a cloaca to aid in excretion and reproduction?

It was clear to Cuvier that forensic reconstruction could not proceed until this problem was properly solved. Replicating a maneuver that had been employed with great success by French botanist Antoine-Laurent de Jussieu in the late 1700s, Cuvier attempted to overcome this problem by articulating a natural hierarchy of anatomical features, a ranking of morphological characters based on their functional importance (Appel 1987, Outram 1986, Colman 1964). This was his principle of the subordination of characters: the notion that bodily functions could be clearly separated and ranked, such that there was one dominant (primary) function underlain by several subordinate (secondary or tertiary) functions of decreasing importance.

It is important to note that Cuvier did not wish to create an artificial system of identification based merely on external features of taxonomic convenience. Indeed, such a system of convenience was precisely what lay at the heart of Linnaean taxonomy, making Linnaean taxonomists the constant target of Cuvier's professional scorn. Rather, he sought wholly objective and scientific principles on which to base his subordination of characters. In his immense *Regne Animal* (1817), he explained that, to achieve a good classification

> we employ an assiduous comparison of beings, directed by the principle of the *subordination of characters*, which is itself derived from that of the conditions of existence. The parts of a being possessing a mutual adaptation, some traits of character exclude others, while on the contrary, there are others that require them. When, therefore, we perceive such or such traits in a being, we can calculate before hand those that co-exist in it, or those that are incompatible with them. The parts, the properties, or the traits of conformation, which have the greatest number of these relations of compatibility or of co-existence with others, or, in other words, that exercise the most marked influence upon the whole being, are called the *important characters, dominating characters;* the others are the *subordinate characters,* all varying in degree. (Cuvier 1831, v. I, pp. 5-6)

Using these methods, Cuvier hoped to determine which bodily functions were most important in determining the anatomical characters of the animal, and which were the least important. Although he often claimed success in this endeavor, his hierarchy was in fact highly unstable (Outram, 1986). In a 1795 work undertaken with Geoffrey Saint-Hilaire (*Nouvelle division des mammifères*), the circulatory and generative systems were listed as primary, touch or sensation as secondary, and teeth as tertiary. Two years later, in 1797's *Tableau élémentaire de l'histoire naturelle des animaux*, he changed his mind, insisting that nutrition and movement were in fact primary, while perception was secondary. By 1800, in his *Leçons d'anatomie comparée*, the order had been scrambled again: movement and feeling were primary in this iteration, while digestion, respiration, and excretion were secondary, and generation was tertiary. It was not until 1812, in the *Recherches sur les osseemens fossiles des quadrupèdes*, that he finalized his ranking system: the nervous system was to be primary, the locomotory, respiratory, and circulatory systems were secondary, and the sensory (touch-based) and digestive systems were tertiary, with the reproductive system as a sort of identifier of last resort.

Crucially, this anatomical principle enabled the researcher to make more or less automatic judgments about when morphological variability was important, and when it could be ignored. Any modifications in the nervous system, for example, were essential in determining the identity of the animal. Changes in the respiratory and circulatory systems were also important, but less so. Those in digestion or means of touch were even less paramount. The identity of the animal therefore lay at its core, in its most essential and most protected organs:

> In proportion, therefore, as we turn our attention from the principal organs to those which are less important, we discover increasing variations; and when we arrive at the surface of bodies where the nature of things requires that the parts least essential, and the injury of which is least dangerous, should be placed, the number of varieties becomes so considerable, that all the labours of naturalists have not yet been able to give us an account of them." (Cuvier 1802, v. I, pp. 58-59)

It is precisely for this reason that prehistoric mammals could be identified by their bones with more certainty than according to "characters for the most part derived from the hair, colour, and other marks, which disappear before the incrustation [or fossilization]" (Cuvier 1835, p. 60). Such superficial parts were naturally variable, for their variation had no real functional import. Their alteration would not affect the animal's requisite functional unity. So individual variations were not a problem for Cuvier, as long as they were only ornamental and not functional. Conversely, because such surface variations were only ornamental, they could for all practical purposes be ignored.

Random variation of functional parts, it is worth pointing out, was an entirely different matter. To combine random functional parts was to create monsters, and for Cuvier monsters simply could not exist. In monsters, the parts of different animals were united "in opposition to every law of nature." (Cuvier 1835, pp. 54-55). Accordingly, teratology, the study of monsters, was sheer fantasy. (It is precisely for this reason that Cuvier's archrival, Geoffrey Saint-Hilaire, would eventually resort to the study of deformed and misshapen humans—monsters—to argue most vehemently against Cuvier's anatomical philosophy. See Appel, 1987.) Because the mishmash of random functional parts was the antithesis of any living body, violating the foundational premise of animal economy, Cuvier assured his readers that there was no reason to dig for the skeletons of ancient Greece's sphinx, Pegasus, minotaur, or chimera; no reason to scour the earth for fossils of Persia's mantichore, griffin, or cartazonon; no reason to believe that ruins of Egypt might contain the desiccated remains of cynocephali or satyrs. According to Cuvier's three principles, such functional hodgepodges were anatomically quite impossible. For example, of the ancient Egyptian ruins, he wrote, "It is in some recess of one of these monuments that Agatharchides must have seen his carnivorous bull, whose mouth, cleft to his ears, spared no other animal; but surely naturalists will not assert that there can be such; for nature never united either cloven feet or horns with cutting teeth" (Cuvier 1835, pp. 55-56). Likewise, in his assessment of the unicorn Cuvier minced no words: "if this animal were ruminating and cleft-footed, it certainly had the frontal bone divided in two, and could not … have had a horn on the suture" (Cuvier 1835, p. 57). Monsters were impossible, since functional parts were not interchangeable.

2.1.4 Enclosed Multifunctional Reticulation

Cuvier's three anatomical principles have typically been subsumed under the collective term *functional anatomy* (e.g. Coleman, 1964). This is an appropriate and useful piece of shorthand, since the three principles taken together constitute a unique and singular approach to the organization of the living body. In the foregoing sections I have not attempted to give a complete account of this approach. Rather, my intention has been to highlight the specific characteristics of it that enabled Cuvier to carry out his spectacular forensic reconstructions. These features, as I have sought to illustrate, are (1) the internalization or enclosure of function, (2) the reticulation of each component by multiple functions, and (3) the clear separation of functions into a primary function and numerous supporting sub-functions. These three criteria are implicit within the principles of the conditions of existence, the correlation of parts, and the subordination of parts, but they are in no way coterminous with those principles. In order to distinguish the forensic criteria from the principles, I will refer to the former as a requirement of *enclosed multifunctional reticulation (EMR)*.

I have endeavored to show that EMR is a clear and necessary feature of Cuvier's reconstructive efforts.[9] Yet as a general set of criteria for forensic reconstruction, EMR is not restricted to the assembly of animal skeletons. In fact, its tenets hold true for the reconstruction of any complex functional object. In other words, any partial object can be reconstructed from its fragments as long as it is functionally enclosed, has multiple components that are crossed by multiple functions, and obeys a clear hierarchy of function and sub-functions. Indeed, this is precisely the logical structure employed by most puzzles. To illustrate the broader applicability of EMR, we might therefore think of a jigsaw puzzle or—even better—the popular number puzzle known as Sudoku.

A Sudoku puzzle (Figure 2.1) is a complex functional object. It is composed, typically, of a 9x9 grid of small squares. These, in turn, are grouped into smaller 3x3

8	**6**	3	9	**2**	5	7	4	1
4	1	2	**7**	8	6	3	**5**	**9**

---

[9] This is not to suggest that Cuvier somehow created EMR out of thin air. It is, of course, possible that other historical thinkers can be seen as Cuvier's intellectual antecedents; that other, earlier scholars also relied upon some version of EMR in their way of thinking about complex functional objects. But in any event, Cuvier was certainly the first to explicitly codify these three requirements and the first to employ them so successfully in the service of forensic reconstruction.

7	5	9	4	1	3	2	8	6
9	7	1	2	**6**	4	**8**	3	5
3	4	6	8	5	7	9	1	2
2	8	**5**	**3**	9	1	4	6	**7**
1	9	8	6	3	2	5	7	4
5	**2**	4	1	7	8	**6**	9	3
6	3	**7**	**5**	4	**9**	1	2	8

Figure 2.1 A Sudoku puzzle, successfully reconstructed from its known fragments (in bold).

sub-grids. The point of the puzzle, its primary function, is to fill its 81 squares with nine 1s, nine 2s, nine 3s, and so on up to nine 9s. But this overall function is achieved through the mutual interaction of several sub-functions. For example, every row must contain the digits 1 through 9. So too, every column must contain the digits 1 through 9. In other words, the completed puzzle will take the form of a "Latin square". But Sudoku adds a crucial sub-function: every 3x3 sub-grid must also contain the digits 1 through 9. Subjected to the simultaneous requirements of these multiple sub-functions, each square of the overall grid is functionally over-determined. For this reason, each Sudoku puzzle can be filled in with a unique combination of correct numbers once a few of the squares' values are known. It can be forensically reconstructed from its fragments. While it might be tempting to think that this property of reconstructability is somehow unique to the specific rules of Sudoku, this is simply not the case. In fact, the overall function of a Sudoku puzzle, the logical reassembly of a 9x9 Latin square, can be achieved using any number of different sub-functions. For instance, in a newer version of the game called KenKen the overall grid is subdivided into irregularly shaped "cages", each of which is marked by an arithmetic operation and a number. The number represents the necessary sum, difference, product, or quotient of the digits that will be enclosed within the cage. KenKen puzzles can be solved without any of their squares filled in advance: the only fragment needed to complete them is the arithmetic sign and arithmetic result of each cage.

Note that I am not simply saying that Cuvier's fossils were *like* Sudoku or KenKen puzzles. Analogy has nothing to do with it. Rather, I am pointing out that both Sudoku puzzles and Cuvier's skeletons rigorously fulfill the forensic requirements of EMR. It was therefore no mere flight of fancy for Cuvier to claim that his principles would permit the reconstruction of a fragmented skeleton. Whether or not he actually succeeded in doing so is a valid question (and the consensus is that, despite his claims, he probably did not—see Coleman 1964, Outram 1986), but in theory his system of reconstructive principles was sound. That Cuvier appears to have frequently 'cheated' by falling back on empirical observations of complete skeletons in order to guide his reconstructions is no different than a Sudoku player repeatedly peeking at the printed solution for clues: the shortcoming is not in the logic of the puzzle, per se, but in the ultimately finite capabilities of the player.

2.2 The super-organism of Frederic Clements

Cuvier's system of comparative anatomy quickly fell into oblivion after his death in 1832. There were probably two main reasons for this. On one hand, Cuvier's achievements were so inextricably linked to his own extraordinary capabilities, experience, and brilliance as an individual, that no other anatomist could possibly hope to fill his shoes. Put less charitably, it is likely that few anatomists carried on using Cuvier's anatomical principles because no one could figure out quite how—or whether—he had actually employed them. On the other hand, Cuvier's theories were based on the uniqueness, perfection, and stability of each organism's animal economy. Stridently opposed to any gradualistic notion of evolution, they fell squarely on the wrong side of the Darwinian revolution. Accordingly, the rapid and nearly universal acceptance of the theory of natural selection, in the years after 1858, doomed Cuvier's anatomical theories to oblivion. By the late 1800s, his byzantine system of perfectly stable anatomical types had been relegated to the fine print of the footnotes of contemporary biological thought.

However, the basic tenets of Cuvier's thought retained their seductive power. EMR was too simple and too powerful a notion to remain buried indefinitely. Indeed, one hundred years after the publication of Cuvier's completed anatomical system in *Le Régne Animal* (1817), the early American plant ecologist Frederic Clements resuscitated them with remarkable fidelity in his own discipline-altering masterwork, *Plant Succession* (1916).[10] That this reformulation of Cuvieran thought has escaped scholarly attention until now should not come as a great surprise. After all, Clements was concerned with a very different kind of complex functional object than was Cuvier. Like other early ecologists, he was captivated by the complex patterns of vegetation growth that seemed to characterize any given landscape. His goal was to account in some way for the structural variations of vegetation at different points in the landscape. Of course, the objects exhibiting such structure were not human-scale artifacts that, like Cuvier's fossil bones, could simply be picked up, turned over, and examined. Rather, Clements' object of study was a vast, regional or super-regional unit of vegetation called the climax formation. Due to its sheer size and complexity, his object could not be easily manipulated or appraised. Nor did it operate, in the fashion of Cuvier's animal bodies, through a handful of readily observable bodily functions (e.g. digestion, circulation, and respiration). Rather, vegetation was believed to fulfill a number of poorly understood ecological functions related to the continuous cycling of nutrients and energy in a given ecosystem. So Clements was relying on notions of structure and function that differed greatly from Cuvier's.

Furthermore, as I have already suggested, Clements made an adjustment of great importance to the original notion of EMR. This modification was so central to Clementsian thought that it has tended to overshadow the fact that it was only that: a modification. In the following section I will

---

[10] This is not to say that Clements, in developing his ideas, relied solely or even purposefully on Cuvier's work. On one hand, other strands of thought—especially biological and geological thought—certainly came together as well in the formulation of Clementsian ecology. On the other hand, it is not even clear that Clements was intimately acquainted with, let alone consciously indebted to, Cuvier's work. Whether Clements recognized it or not, however, it is clear from a present-day perspective that the basic tenets of Cuvier's EMR constitute a vital aspect of the Clementsian vegetation system.

first show how Clements reiterated the notion of EMR. I will then examine his own famous addition to that concept.

2.2.1 The Comparative Anatomy of Landscapes

Of all the researchers who could have revived Cuvier's notions of structure and function, it is perhaps somewhat counterintuitive that this distinction would ultimately fall on a plant ecologist rather than an anatomist or a zoologist. This line of succession is not as strange as it first appears, though, for Clements was every bit as much an anatomist as an ecologist. Indeed, he proclaims in the opening sentences of *Plant Succession* that

> The developmental study of vegetation necessarily rests upon the assumption that the unit or climax formation is an organic entity. As an organism the formation arises, grows, matures, and dies. Its response to the habitat is shown in processes or functions and in structures which are the record as well as the result of these functions. Furthermore, each climax formation is able to reproduce itself, repeating with essential fidelity the stages of its development. The life-history of a formation is a complex but definite process, comparable in its chief features with the life-history of an individual plant (Clements 1916, p.3).

So Clements *was* in fact studying the structures and functions of an organism (or as he sometimes called it, a super-organism). Vegetation was for him a single coherent entity, a super-organism called the climax formation. Landscape ecology, the comparative study of these different formations, was therefore profoundly anatomical. This basic organismal notion was so fundamental to everything else in *Plant Succession* that Clements wasted no opportunity to remind his readers of it. Indeed, while it functions as the volume's opening salvo, the above paragraph also serves as a kind of refrain or chorus for Clements, who repeats the entire passage, verbatim, at regular intervals throughout the remainder of the book.

Like Cuvier, Clements was intent on identifying complex organisms—in this case, vegetational landscape formations. Also like Cuvier, he saw the problem of identity as a matter of cataloguing each organism's unique web of structural and functional relations. It should therefore come as no surprise that he ended up recreating Cuvier's system of EMR as well—albeit in explicitly botanical or phytological terms. The Clementsian notion of a climax formation is in this respect nearly identical to Cuvier's notion of the animal: a fully enclosed body whose many parts are reticulated by multiple functions or sub-functions, and whose functions are ranked according to a clear hierarchy of importance. Let's briefly examine each of these aspects in turn.

The functional enclosure of the climax formation is implicitly stated in Clements' description of it as an organism. The necessary presupposition of his entire body of work is that vegetation exists as a "unit" or an "organic entity" whose overall effects are greater than the sum of its parts. However, this unity emerges on such a large spatial scale that it can often be difficult to perceive. Indeed, the integrity of which Clements speaks is most readily apparent not across space, per se, but in the temporal stability of old-growth forests and other climax formations. Clements writes that "the essential unity of a climax", is manifest in its "high degree of stability", which must be "reckoned in thousands or even millions of years" (Clements 1936, pp. 255-6). I will return to the Clementsian notion of enclosure in a moment, for he has much more to say on the subject. For now though, it is enough to state that the climax formation is a coherent and bounded whole that, like the living bodies of Cuvier's animal economy, is characterized primarily by its remarkable ability to resist degradation over time.

This overarching functional unity was not to be confused with any kind of structural uniformity. Indeed, Clements conceded that in most cases the vegetation comprising a formation seemed to form a "mosaic" of such intricacy that it could often "appear to be a veritable kaleidoscope" of plants (Clements 1936, p. 282). For this reason, most of his professional energy was expended not in a detailed formulation of the notion of the climax formation, but rather in a lifelong attempt to explain the formation's perplexing diversity by breaking it apart into its structurally distinct (if thoroughly intermingled) component parts.

The results of this process of reverse engineering are dauntingly complex and—the Clementsian system having long since fallen from grace—are rarely studied in their entirety anymore. Yet they are an important manifestation of the principles of EMR. Clements' system of interlocking parts can be summarized in the following way. Firstly, according to Clements, every climax formation could be structurally identified by a small number of dominant species that occurred throughout its entire extent. These he referred to as *perdominants*. The grassland climax of central North America, for instance, contained hundreds of species of grasses, sedges, and forbs, yet it derived its character as a distinct formation from the presence of a mere eight perdominants: *Stipa comata* (needle grass), *Agropyron smithii* (western wheatgrass), *Bouteloua gracilis* (blue grama), *Sporobolus cryptadius* (sand dropseed), *Koeleria cristata* (Junegrass), *Elymus sitanion* (squirelltail), *Poa scabrella* (pine bluegrass), and *Festuca ovina* (sheep fescue). Eight was in fact a relatively large number of dominants. Most climax formations had only two or three indicative species, as in the Maple-Beech, Oak-Hickory, or Fir-Birch-Spruce climax formations of the eastern North American forests.

Secondly, every formation could be spatially divided into a discrete number of *associations*. Each of these associations was dominated by at least one of the climax formation's perdominant species, but was additionally characterized by a handful of *eudominants*: relatively localized species that dominated the landscape only within that particular association. The grassland formation cited above could, for example, be divided into six associations: true prairie, mixed prairie, coastal prairie, desert plains, Palouse prairie, and California prairie. Of these, the true prairie association was uniquely characterized by three additional eudominants: *Stipa spartea* (porcupine grass), *Sporobolus* *asper* (tall dropseed), and *Sporobolus heterolepis* (prairie dropseed). The mixed prairie association was characterized by a single additional eudominant, *Buchloe dactyloides* (buffalo grass). The desert plains association was characterized by its own eudominants: *Bouteloua eriopoda* (black grama), *B. rothrockii* (Rothrock's grama), *B. radicosa* (purple grama), and *Aristada californica* (California threeawn). All of these served as regional co-dominants alongside the more universal perdominant species.

Thirdly, wherever one of these eudominant or predominant species occurred singly, without any of the others, it formed its own local *consociation* within the regional association. Each association were typically studded with these smaller consociations of relatively uniform appearance. For instance, within the mixed prairie association, the tops of ridges were often dominated entirely by patches of *Stipa comata*, while the swales were dominated entirely by *Agropyrum smithii.* By contrast, in those locations where such consociations were absent (*i.e.* where multiple eudominants and perdominants co-existed alongside each other), the association was spatially divisible into distinct *faciations*. Faciations were distinguished by their different relative abundances of each eudominant species. In other words, within a grama-dropseed-buffalo grass association (which I have invented solely for the purposes of illustration) one faciation might be dominated primarily by buffalo grass and secondarily by dropseed and grama species, while another might be dominated primarily by grama species and secondarily by buffalo grass and dropseed. Clements wrote that the Great Plains could be sub-divided into six faciations from north to south: *Stipa-Bouteloua*, *Bouteloua-Carex*, *Stipa-Agropyrum-Buchloe*, *Bouteloua-Buchloe*, *Hilaria-Stipa-Bouteloua*, and *Agropyum-Bouteloua*.

Finally, the precise ratio of the different eudominants within a faciation showed a degree of variability at very local scales. Blue grama might be twice as prevalent as its co-dominant buffalo grass on one hillside, but only marginally more prevalent on the neighboring hillside. To account for such small-scale discrepancies, the faciation could be divided into numerous *lociations*. For all practical purposes, these were the most basic components of the landscape, its fundamental building blocks. The formation could be conceived as a vast, interlocking collection of lociations: a mosaic, as Clements wrote. Of course, for the purposes of description these pieces could be combined into faciations or (if dominated by a single species) consociations; the latter could in turn be combined into associations; and associations themselves were the component parts of the formation. Because of this nested and interlocking structure, each lociation contained information not only about itself, but about its faciation, its association, and ultimately its formation. The lociation, if properly understood, was therefore potentially diagnostic of all of these.

Within this great structural matrix of vegetation, what tied one lociation to another, allowing them to be aggregated into coherent faciations, associations, and formations? According to Clements, each was connected to the others by overlapping sets of functional constraints that were imposed by the habitat. In other words, lociations were functionally connected by the various environmental factors that determined the ultimate vegetational structure of each one. These common factors included temperature, precipitation, soil type, soil saturation, slope, aspect, and altitude. Every lociation's structure was partially determined by all of these. Each was therefore crossed or reticulated by numerous functional requirements.

In order to make sense of this bewildering array of edaphic, topographic, and climatic factors, Clements did exactly what Cuvier had done before him: he separated them and ranked them into a distinct hierarchy of functional importance. According to his ranking, the primary structure of the formation (its dominant species) was determined entirely by climate, and especially precipitation. This was the primary functional constraint on vegetation and its great landscape-scale unifier: "the essential unity of a climax," Clements insisted, "is to be sought in its dominant species, since these … denote in themselves a definite relation to climate" (Clements 1936, p.255). Regional gradations in that climate also accounted for the division of the formation into distinct associations. The distinct associations of the grassland climax, for instance, resulted mainly from the varying precipitation levels across the Great Plains. Of secondary functional importance, in Clements' model, were the properties of the soil. These could exert a certain degree of influence upon the structure of the association, although to a lesser extent than the regional sub-climate. More to the point, edaphic differences such as soil water content were often directly responsible for the appearance of specific consociations. Moving down the Clementsian hierarchy, temperature, which is typically far more localized than precipitation, was of tertiary importance. Temperatures were the main factor responsible for the appearance of different prairie faciations. Finally, topographic factors such as altitude, slope, and exposure could modify both the climatic and edaphic factors to some degree. Topographic differences were expressed at the smallest geographic scale, in the nuanced vegetational structure of the lociations themselves.

In sum, the climax formation was a self-enclosed functional unity in which each of the component parts was mutually interconnected to the others by numerous sub-functions of decreasing importance. Conceived in this way, each type of formation, each super-organism, could theoretically be identified from even the most localized components of its vegetation. Although such formations lacked, by their very nature, the structural and functional clarity of Cuvier's animal bodies, they were clearly intended to fulfill the same forensic criteria of enclosed multifunctional reticulation. I should clarify: Clements did not explicitly corral his work within some set of requirements that was isomorphic with EMR. But he most certainly recognized that his notion of the climax formation held a diagnostic or forensic potential far exceeding that of any of his rivals' phytological systems. Hence, his unwavering and ultimately quixotic allegiance to the climax model, even as counterfactual evidence began to pile up against it; hence, too, the unblinking rapidity with which most other landscape ecologists accepted his model in the early decades of the 1900s. With its promise of nearly unbridled forensic power, vegetational EMR was a fairly enchanting notion for Clements and his peers alike, a sort of analytical Holy Grail whose most ardent believers could not and would not be deterred, even by the hard facts of reality.

All of this is not to say that Clements' system of identification was entirely identical to Cuvier's. Indeed, compared to Cuvier's functional anatomy, Clements' version of EMR was not especially pretty: it actually comes off as rather clunky and ill-fitting. In some ways this ungainliness was probably not to be helped. Vegetational structure by its very nature was complex and disarticulated, and the ecological functions it performed were difficult to understand in anything more than a rudimentary manner. So try as one might, there probably did not exist any particularly elegant way to turn a continuously variable landscape into a crisply defined super-organism. The awkwardness of such a maneuver was all but assured in advance. So if we concede that the Clementsian version of EMR, in its enduring deficit of visual clarity and its chronic want for functional precision, appears to constitute a rather clear step down from Cuvier's, this was perhaps unavoidable.

Of course, it is also true that the shortcomings of the climax model are rather clearer in hindsight than they were in the early 1900s. We should bear in mind that its central conceit—the notion that a forest or a prairie might exist as a single organism—while it may seem laughable today, was still very much an open question in Clements' day. It was far-fetched only insofar as all revolutionary ideas are, suddenly stretching and recalibrating our pre-existing notions of what is possible. Still, not all of the climax model's imperfections were invisible at the time of its formulation. Even if we suspend our present-day disbelief and accept the climax model on its own, early-twentieth-century terms, it is still hobbled by one major shortcoming—one that was as abundantly clear then as it is now. While the model presupposes that the formation is an organic entity defined by stability over "thousands or even millions of years," such areas of persistent stability were almost never witnessed in natural vegetation. On the contrary, most landscapes seemed to be characterized by constant change. Over time, tallgrass prairies became scrub woodlands. Birch forests were gradually overshadowed by spruce trees. Wildfires turned pine forests into denuded plains. Abandoned farm fields became majestic groves of white pine. Vegetation seemed to be constantly ebbing and flowing across the land.

This dynamism posed a grave problem for Clements. If the formation was not stable, if other plants could encroach upon it at will and interrupt its existing functional relations, then it could not be described as a functionally enclosed unit. And without functional enclosure the forensic advantages conferred by EMR quickly dissipated. The problem of enclosure, with its criterion of absolute and pre-given stability, was therefore the Achilles heel of the Clementsian model. The most crucial presupposition of his entire system, it could seemingly not be reconciled with the observed ubiquity of vegetation change. Yet it is precisely here that the Clementsian version of EMR most clearly supersedes Cuvier's. In an intellectual turn of absolute brilliance, Clements transformed this glaring weakness into his model's greatest strength and its most lasting contribution to ecological science: the incorporation of vegetation *dynamics* under the rubric of autogenic succession.

2.2.2 Progressively Enclosed Multifunctional Reticulation

Clements argued that the vegetation within any climatic region diverged from the climax formation only insofar as it had been *disturbed* by some event that partially or completely denuded the land: wildfires, insects, glaciers, agriculture, and so on. Following such a disturbance, the vegetation would slowly recover until it once again achieved its proper climax state. This process of recovery was neither random nor undifferentiated. Rather, each formation was characterized by a unique recovery process, during which the vegetation would pass through a number of discrete and predictable stages. Each stage would, after an initial period of stability, give way to a new stage of larger and more highly developed vegetation. This developmental pathway was known as the formation's *sere*. It is in this seral sense that Clements could claim that vegetation had a definite life-history, arising, growing, maturing, and dying just like any other organism.

Crucially, the process of succession was autogenic. In other words, it was driven by the vegetation's own internal dynamics, rather than external forces. Autogenic succession began when a disturbed area was colonized by a handful of plant species that could survive in the relatively harsh conditions of a recently denuded landscape. Such a landscape was typically *xeric*, characterized by intense sunlight, low ambient humidity, high exposure to desiccating winds, dry soils, wide diurnal temperature swings, and so on. This environment naturally exerted a kind of selective pressure on the vegetation: Clements referred to this selective effect as the *action* of the environment on the vegetation. Because of this unceasing action, the initial colonizer species would become more and more populous, dominating the landscape ever more completely over time.

Yet during this process of demographic saturation the colonizers would inevitably begin to modify the very environmental conditions that had allowed them to become established in the first place. Their leaves or petals would cast shade on the surface of the earth, their roots would trap moisture within the soil, their stems would block the moisture-sucking wind, and so on. In other words, although vegetation became established due to the action of the environment, that same vegetation also exerted a *reaction* back on its habitat. The effect of this reaction was always delayed for a certain period of time, since it was cumulative. It became noticeable only once the landscape was densely saturated or blanketed by the species in question:

> The reaction of a community is usually more than the sum of the reactions of the component species and individuals. It … becomes recognizable through the combined action of the group. In most cases the action of the group accumulates or emphasizes an effect which would otherwise be insignificant or temporary. A community of trees casts less shade than the same number of isolated individuals, but the shade is constant and continuous, and hence controlling. (Clements 1916, p.79)

The same could be shown for other reactions as well. As Clements pointed out, "The leaf-litter is again only the total of the fallen leaves of all the individuals, but its formation is completely dependent upon the community" (*ibid*). Every tree dropped its leaves onto the ground, but only a large group of trees could create a permanent layer of habitat-altering leaf litter. This emergent character of the reaction, its dependence on increasing vegetation density over time, is what accounts for the stagewise nature of Clementsian succession.

In the endless dance of action and reaction it is clear that, for Clements, every colonizing species constituted both the solution to a problem and a new (impending if not yet actualized) problem of its own. Every new species fulfilled a productive role in the short run but was self-limiting in the long run. The purely functional explanation of this pattern was to be found in the gap between action and reaction—the subtle disjuncture between the vegetation's causes and its effects. For example, the cause enabling the first colonizers to flourish, following a severe disturbance, was the presence of an acutely xeric habitat; the effect of those colonizers, however, was to produce a wetter, cooler, more *mesic* environment. As time went by, the existing vegetation therefore seemed increasingly to work against itself, to be characterized less by functional unity than by functional discord. Its causes and effects were not, as became obvious, neatly tied up in a way that would allow it to be self-sustaining. Rather, there were loose causal ends strewn about, unfilled opportunities for structural and functional improvement. Because this vegetation's causality was not enclosed, species that were better adapted to the newly mesic conditions could invade the system and take root. In this way, a new stage was ushered in.

With each new stage of vegetation, the emergent disjuncture between environmental causes and environmental effects diminished in magnitude. In other words, each stage was slightly better adapted to the conditions that it was destined to produce. Each made slightly better use of the many environmental factors at its disposal. As a result, each stage tended to last somewhat longer than the one that came before: more functionally enclosed, it was inherently more self-sustaining as well. Indeed, Clements pointed out that the stage directly antecedent to the climax (which he called the sub-climax) often lasted for so long that it could be mistaken for the climax. But even this penultimate stage's nearly imperceptible margin between action and reaction would eventually add up and produce a structural upheaval. Only in the climax stage were the effects of the vegetation also its precise causes. Only with the climax stage did causality become entirely internalized. With no loose environmental ends hanging about, there were no functional points of entry for new species to enter the climax formation: it was perfectly stable over the long run.

Crucially, succession does not fundamentally alter the foregoing analysis of the climax formation, with its constituent array of associations, consociations, faciations, and lociations. For, when left to its own devices, Clementsian succession will always reproduce the climax formation with utter precision. Indeed, autogenic succession actually strengthens the climax model by assuring that it cannot be invalidated, even by directly contradictory evidence. Viewed through the lens of autogenic succession, any observed discrepancies from the climax model can be presumed to be temporary and therefore insignificant. As long as disturbances are kept at bay (which, of course, they seldom are) any landscape's vegetation will automatically wind up in its preordained state of enclosed multifunctional reticulation. Enclosure is therefore still presupposed in the climax succession model; the difference is simply that it is guaranteed to *emerge* through a natural progression of stages, in which any existing functional contradictions are collapsed and rectified through successive structural overhauls. This system is not identical to EMR, but it very nearly is. We can refer to it as *progressively enclosed multifunctional reticulation (PEMR)*. PEMR is simply a dynamic form of EMR. More precisely, EMR serves as the horizon toward which PEMR unerringly moves. Although PEMR is dynamic, it is therefore highly structured as well. It may be a system of perpetual emergence, but what emerges is convergence toward a kind of structural norm. The dynamism of PEMR is restricted to its inexorable movement toward a highly organized kind of stasis.

2.3 Simondon's successional technical objects

For some thirty-five years the Clementsian model reigned supreme among ecologists. By 1950, however, it had been almost universally rejected. As the number of observed deviations from "normal" succession increased, Clements' system became overburdened with intricate explanations for each exception—and with the equally intricate terminology required to describe them. In the end there was simply too much contradictory evidence to ignore: ecology underwent a sort of paradigm shift away from the model of climax succession (and toward another model, which we shall consider in due course). Thereafter, EMR, in its Clementsian variant of PEMR, quickly receded from the forefront of scientific thought—just as it had following Cuvier's death a hundred years earlier.

At this point we may appear to have strayed rather far from the problem of derelict trawl nets. We have discussed a failed system of anatomy from the early 19th century and a failed system of landscape ecology from the early 20th century, but we have not gotten appreciably closer to the identification of trawl netting in the Northwest Hawaiian Islands. However the foregoing analysis of Cuvier and Clements has not been a simple detour. As we shall see, the notion of PEMR did not disappear for good after the ecological movement away from Clementsian climax succession. Rather, it was resurrected yet again by French philosopher Gilbert Simondon in 1958, this time to explain the functional organization of technical (rather than anatomical or vegetational) objects. Simondon's *On the Mode of Existence of Technical Objects* (hereafter abbreviated to *The Mode of Existence*) combined the strongest aspects of both Cuvier's and Clements' versions of EMR.[11] Like Cuvier, Simondon dealt with easily recognizable objects that operated through a limited number of clear functions. Like Clements, he incorporated a notion of progressive dynamism into the heart of his theory. What resulted was a fairly radical system of perpetual emergence and indetermination that nonetheless offered all the qualities of forensic certainty that we have already discussed. Unlikely though it may seem, Simondon's system of thought—a system that rejects all transcendence in favor of immanence, a system that replaces even the ontological notion of *being* with an ontogenetic one of *becoming*—is therefore very closely linked to the notions of transcendent and god-given stability embodied in

---

[11] As I have already noted for both Cuvier and Clements, Simondon quite obviously drew on numerous strands of philosophical and historical thought in order to formulate his theory of technical objects. My contention, therefore, is neither that his thinking is simply reducible to theirs, nor that he was necessarily aware of the striking continuities that connect his work to their earlier systems of forensic reconstruction. Yet, these provisos do not invalidate the surprising observation that such continuities of thought do, in fact, exist.

Cuvier's types. In the following sections I will detail the ways in which Simondonian technical thought constitutes a (surprisingly) clear reiteration of Cuvier and Clements.

2.3.1 Concrete and Abstract

If succession—the dynamic achievement of unity—served as a sort of technical fix for Clements, rescuing the stability of the climax formation from the ubiquity of vegetation change, it serves as the central point for Simondon. He states in the opening pages of *Mode of Existence* that, "The technical object is a unit of becoming" (Simondon 2007, p. 42) and explains that, "The technical being evolves by convergence and by adaptation to itself; it is unified internally according to a *principle of internal resonance*" (*ibid* – my italics).[12] Nowhere does Simondon explicitly define this principle. Yet he articulates it quite clearly in his assertion that, in the modern technical object, "each important piece is so connected with the rest by reciprocal exchanges of energy that it cannot be other than it is" (p. 43). As an example, he describes the internal organization of a modern engine:

> The shape of the cylinder, the shape and dimensions of the valves, the shape of the piston are all part of the same system in which a multitude of reciprocal causalities exist. To the shape of these elements there corresponds a compression ratio which itself requires a determined degree of spark advance; the shape of the cylinder-head, the metal of which it is made, produce, in relation to all the other elements of the cycle, a certain temperature in the spark plug electrodes; this temperature in turn affects the characteristics of the ignition and, thence, the entire cycle. (*ibid*)

---

[12] No English translation of *The Mode of Existence* has ever been commercially published. My analysis therefore relies on the Spanish translation published by Prometeo Libros in 2007. For the sake of brevity and readability, my analysis of this text cites only the page numbers of relevant quotations.

If this intricate description of "internal resonance" sounds suspiciously like Cuvier's own convoluted explanation of the correspondence of parts, in which the precise shape and position of a predator's jawbone was shown to determine a whole cascading chain of other physical features according to a "multitude of causalities," this is not by sheer coincidence. Indeed, as we shall see, within his principle of internal resonance Simondon encapsulates all three of Cuvier's principles along with the Clementsian modification of them.

Oddly, considering that they constitute an operative principle of the entire work, the words "internal resonance" do not actually appear again in *The Mode of Existence*. They do not have to, however, for Simondon immediately gives us an alternate set of terms for the principle: "It could be said that the contemporary engine is a concrete engine and that the old engine was an abstract engine" (*ibid*). In other words, a concrete object is one that is internally resonant, while an abstract object lacks internal resonance. The becoming of the technical object, its becoming resonant, is a process of increasing concretization. "The technical object exists, then, as a specific type that is achieved at the end of a convergent series. This series goes from the abstract mode to the concrete mode: it tends toward a state in which the technical being would be a system that is entirely coherent with itself, entirely unified" (p. 45). The abstract object and the concrete object stand like bookends at either end of Simondonian technical progress. In order to proceed, it will therefore be productive to examine the differences between them.

The first distinction that should be pointed out concerns the precise structure and role of the technical object's internal components. Simondon contrasts the modern, internally resonant engine (whose description we have already read) with an abstract, "primitive" engine that has undergone minimal development. In the operation of the latter, he writes,

> each element intervenes at a certain moment in the cycle, and is thereby supposed

> to have no effect on the other elements; the parts of the engine are like individuals working each in turn without ever knowing each other. … The early engine is a logical assembly of elements defined by their complete and singular function. Each element can best complete its own function if it is like a perfectly finished instrument, completely oriented towards the accomplishment of that function. (p. 43)

So in the abstract object, each component is designed to perform only its own unique sub-function within the totality of the object's overall functioning. There is no sharing of each part's load, no distribution of functions across multiple parts. This strict division of functions among independent structures makes a certain amount of logical or analytical sense, and it will probably get the intended job done. But it is unlikely to perform the desired operation with a great deal of efficiency or effectiveness. Indeed, this kind of technical quarantine, in which each sub-function and its associated set of structures is entirely sequestered from all of the others, gives rise to certain functional shortcomings that only become apparent in the overall operation of the object.

On one hand, because the abstract object's functional subsystems are working more or less independently of each other, their actions remain uncoordinated and may even be detrimental to each other. Accordingly there must "appear particular structures which we could call … defense structures" (pp. 43-44). Defense structures are components of the technical object that have a purely negative function. Their sole purpose is to ameliorate the negative effects that one subsystem of the functional object exercises on the others. Defense structures do their job by brokering a sort of compromise: they lessen the offensiveness of each conflicting part by decreasing its functional intensity and, therein, its effectiveness. So the abstract technical object is, like an early successional stage of vegetation, characterized by internal functional discord and a tendency toward self-destruction. But a technical object, Simondon insists, "should not be self-destructive; it should maintain itself in stable operation for as long a time as possible" (p. 48). The abstract object gains stability only insofar as its functions—dampened as they are by defense structures—are performed sub-optimally.

On the other hand, even where such self-destructive tendencies are not pronounced, the abstract object is by its very nature a fragile achievement. At any time it is prone to total breakdown, since "the relative isolation of each system constituting a functional sub-system threatens, in the case of its failure, the conservation of the other systems" (p. 47). No matter how toughly it is built, the abstract technical object is an exceedingly delicate creation. It is built like a house of cards whose stability can never be fully counted on. Damage to a single one of its components will trigger the failure of an entire functional sub-system, making it impossible for the object to achieve its overall function.

The concrete technical object, by contrast, has none of these shortcomings, for within it "each of the parts performs a variety of roles" (*ibid*). Its components are not isolated from each other in the single-minded pursuit of their own functional goals. Rather, each plays a part in several functional sub-systems, cooperating in a way that assures the object will not be cloven by functional divergence and discord. The structure of a concrete object is therefore "not a compromise, but a concomitance and convergence;" its design is focused on "the convergence of functions into a structural unity rather than [on] the search for a compromise between conflicting requirements" (p. 44). In other words, its stability does not require the addition of efficiency-sapping defense structures to mollify the effects of internal conflict. Furthermore, because of this "concretization and functional over-determination," no single one of its components can control the fate of a functional sub-system (p. 37). The concrete object may in fact function adequately even when one of its components malfunctions, since that part's functional roles are shared across many other different parts. In this sense the concrete object is far more robust than the abstract one. Indeed, Simondon describes concretization as "the formation of stable types" through the reticulation of each component part by multiple functions (p. 46). In this sense it is no different than Cuvier's correspondence of parts.

2.3.2 Functional Hierarchy

If every component in Simondon's concretized object is crisscrossed by multiple lines of function, not all of those functions are created equal. There is, he insists, always one primary technical function to which all of the other functional subsystems must be subordinated. At first blush this may come off as rather obvious. It seems almost unnecessary to state that every technical object has a main purpose or function for which it was designed. But Simondon's functional hierarchy is not quite as simple as this. For as we shall see, a thing's primary function is not at all the same as its purpose. Indeed, according to Simondon, the latter constitutes something of a distraction from the former. The primacy of an object's functions, he insists, must be determined by their degree of interiority—the extent to which their technical specifications remain untarnished by external, non-technical considerations. Therefore, when determining an object's technical identity, any functional subsystem whose design has been contaminated by extrinsic requirements must be cast aside. For this reason, much of the first part of *Mode of Existence* reads like a sort of extended *Reinheitsgebot*, a decree of technical purity listing out the many imposter features that cannot be taken as a primary determinant when identifying the technical object.

Simondon's first move in this regard is to reject the notion that an object's technical identity has any fundamental connection to its intended purpose or job. Different "species" of technical object, he writes, "are easy to identify in a summary manner, for practical use, insofar as one agrees to understand the technical object in terms of the practical goal it is intended to meet; but this specificity is illusory, for no fixed structure corresponds to a defined use" (p. 41). To call something an "engine" is in other words not a satisfactory form of identification, since the label has no ultimate connection to the engine's particular mode of technical functioning. As Simondon is at pains to show,

> The same result can be obtained from very different functionings and structures: a steam engine, a gasoline engine, a turbine, and an engine powered by springs or weights are all equally engines; yet, there is more analogy between a spring engine and a bow or cross-bow than between the former and a steam engine; a clock with weights has an engine analogous to a lathe, while an electric clock is analogous to a doorbell or buzzer. (*ibid*)

A certain kind of technical functioning might be employed in any number of objects with vastly different uses; so, too, a given object might conceivably be used for any number of different purposes. The primary function of a technical object can accordingly not be understood in terms of its purpose or the utilitarian end to which it is put. Instead, all objects, regardless of the use to which they are put, are designed and built "according to their profound intention, according to their technical essence" (p. 61). This essence remains stable even as the physical object undergoes successive transformations around it.

So deeply buried is the technical essence within an object's structure that it can even exclude some of the object's more superficial parts. The first components to be jettisoned in this way are any "decorative details and superficial accessories" that are added to the exterior of the object (p. 46). For example, the purely aesthetic additions made to many automobiles—we might think of spinning rims, chrome highlights, outlandish spoilers, etc.—are non-essential to the automobile's functioning. Because their form does not impact the overall function of the automobile, they can be made to measure according to the highly variable whims of the individual consumer. Such superficial details have very little connection to the car's technical essence.

Cuvier, of course, made very similar pronouncements about ornamentation and fur coloration in animals: though highly variable, such traits were functionally unimportant and could therefore be ignored. Simondon takes this condemnation even further. "The more a car must comply with the critical demands of its user, the more its essential aspects are encumbered by an external bondage. The body becomes weighed-down by accessories, its shapes no longer correspond to that of a streamlined structure. The *made-to-measure* feature is not only inessential, but works against the essence of the technical being, like a dead weight imposed from outside" (*ibid*). In other words, ornamentation is not merely irrelevant to technical function, but actually detrimental to it. Adornments do not even pretend to have a functional value: they are the antithesis of technical progress.

The more clearly functional components of the technical object do not necessarily fare much better, however. For example, the essence of the technical object cannot be found in functional components that are specialized to help it adapt to a specific set of external surroundings. Such adaptations reveal less about the object's own mode of technical functioning than they do about its intended environment. Where an object's "functional over-adaptation" becomes so drastic that it resembles parasitism—for instance, when a glider must latch onto an airplane in order to become and remain airborne—the structure of the first object is overwhelmingly determined by the functionality of its host. This "hypertelic" form of specialization causes the object "to adapt poorly to changes, even slight ones, in the conditions of its operation or its manufacture" (p. 71). It therefore has little to do with true technical progress, which as we have already seen is characterized by increasing stability and functional robustness. Hypertelic adaptations may be functional, in a sense, but they have little to do with an object's *essential* functioning.

Having whittled away the purely ornamental and hypertelic (i.e. extroverted) aspects of the technical object, Simondon shifts his attention to its more introverted functional subsystems. Even here there is room to distinguish some design features as less important than others, for not all functional subsystems are designed according to purely technical considerations. Some design modifications, he points out, are instead driven by the manufacturer's desire to maintain consumer interest in a product. The fundamental causes of such design alterations are commercial or economic rather than technical. At best, they are ancillary to the proper technical functioning of the overall object. At worst, they greatly complicate its functioning, introducing vulnerable new points of potential failure into its operation.

Simondon is particularly dismissive, in this regard, of the design changes ushered in by automobile manufacturers each year. Just because a car undergoes structural and functional modifications every year, he insists, does not mean that it is improving. If the so-called improvements are not technically essential to the object, then they do not constitute progress. Simondon uses this anti-economic criterion to dismiss even a number of features that most of us consider to be clear advantages of new cars over old ones. For instance, he writes that power steering does not constitute a *technically essential* improvement over manual steering. Nor do electrical ignition systems improve in any *technically essential* way upon manual ignition systems. In fact, so much of the development of the modern automobile is technically inessential that Simondon ultimately writes off any possibility of real automotive improvement: "The automobile, a technical object so charged with psychic and social implications, is not suitable for technical progress: automotive advances originate in neighboring areas, such as aviation, shipping, and transport trucks" (p. 48). Automotive design changes simply have no real technical value. They are driven less by the car's internal technical requirements than by economic factors such as "the taste for luxury, the desire for novelty that is so evident among users, and commercial propaganda" (*ibid*). As a result, the automobile does not show the typical concretizing trend toward functional and structural simplification, but rather "certain tendencies toward complication" (*ibid*).

I should point out that the addition of new, "complicating" components to the technical object is not forbidden outright. Sometimes, of course, the addition of new structures is indeed called for within an object. Yet, for Simondon, an increase in the number of parts is permissible only insofar as it allows counterproductive secondary effects of the object's operation, which were previously vague and non-localized (i.e., hard to pin down), to be clearly defined, channeled into a specific set of structures, and thereby incorporated into the object's overall function. So although concretization typically demands that an object's functions be spread out across the existing constellation of components, this particular kind of structural differentiation "proceeds in the same direction as the condensation of multiple functions in the same structure, because the differentiation of structures at the heart of the system of reciprocal causalities permits the suppression (by integrating them into the functioning) of secondary effects that were previously obstacles" (p. 55). In other words, additional components are warranted only if they help to subordinate minor functions to the primary one: the object's technical essence.

2.3.3 Succession Without Climax

This litany of functional subordinations, though varied, is based on a single basic tenet: True technical progress can never be driven by external requirements. Even though technical progress is carried out by humans—a crucial point to which I will dedicate more thought in the following chapter—its impetus must emerge from the technical object's own internal dynamics. But how might such a system of fully internalized progress actually work? How can functional improvement occur in a way that does not emerge primarily from external impositions? The answer, as formulated by Simondon, is an uncanny reiteration of the Clementsian system of autogenic succession.

As it was for Clements, progress for Simondon is embodied in a sort of articulated *sere*. "The specific evolution of technical objects does not happen in an absolutely continuous manner," he writes, "nor in an absolutely discontinuous manner either: it involves threshholds that are defined by the fact that they usher in successive systems of coherence" (p. 48). The stagewise nature of this transformation already, of course, suggests a certain affinity with the model of climax succession. Yet even more to the point, it is the *mechanism* of this punctuated metamorphosis that marks Simondon's technical evolution as unmistakably Clementsian.

Over time, Simondon explains, the technical object at any given stage of progress becomes increasingly saturated with minor improvements. These have an additive, almost auxiliary quality to the object's fundamental design. They result in the slow proliferation of helpful new sub-structures functions within the object, but they do not advance its overall degree of concretization. Accordingly, Simondon concludes that "The course of minor improvements is one of detours, useful in certain cases of practical use, but they do not cause the technical object to evolve in the slightest" (p. 61). Rather their overall effect is to "conceal the true schematic essence of each technical object beneath a pile of complex palliatives" (*ibid*). In this sense, minor improvements are not unlike the defense structures of abstract objects, or the "false renovation(s) which commerce requires in order to pass off a recent object as superior to older ones" (*ibid*). They are not merely superimposed by fashion, for they are intended to improve the functionality of the object, and yet they do nothing to resolve the object's real underlying imperfection.

As these palliative pile up, however, the technical object's real source of imperfection cannot help but become increasingly obvious. By accentuating this underlying functional conflict, the accumulation of minor improvements creates a situation of increasing tension and potential energy within the object. As Simondon writes, its increasingly dense web of internal relationships

> leads the object to discover obstacles within its own functioning, based on certain limitations in the conditions of its use: the incompatibilities that arise during the progressive saturation of a system of sub-systems are the result of certain limitations, and clearing away these limitations constitutes progress; but by its very nature, such a clearing of limitations cannot be accomplished except by a leap, by a modification of the internal distribution of functions, by a rearrangement of their system; what was an obstacle should be transformed into a means of achievement. (p. 49)

Just as in climax succession, each stage of the technical object is a self-limiting system over time. Its fundamental organizational flaws can only emerge over time, through the continual saturation of the system by minor improvements. Detours or palliatives though they may be, minor improvements are therefore a wholly necessary and positive aspect of technical evolution, for they transform a formerly vague and perhaps even unnoticed functional conflict at the heart of the object into something that is accentuated, well defined, and therefore correctable. Indeed, the technical object achieves its next stage by restructuring itself in a way that incorporates this accentuated shortcoming into the positive functioning of the object.

Just as in climax succession, the movement from one stage to the next is driven by an impetus toward increasing functional enclosure. Again, as minor improvements saturate the system, they draw attention to some basic functional discord lurking within the object. They highlight a rift, previously unnoticed, where unanticipated *effects* of the object's operation diverge from its necessary initial conditions or *causes*. Progress is an inventive restructuration that seals this emergent rift or—what amounts to the same thing—more fully encloses the functional system of causes and effects, such that the effects of the technical operation do not undermine its causes. As Simondon writes, "The successive *precisions and closures* applied to this system transform into stable functions the obstacles that arise on their own during its functioning" (p. 52—my italics). Enclosure emerges over time, just as in climax succession, and it emerges because abstract objects, like pre-climax stages of vegetation, are characterized by their "divergence of functional aims" (p. 45). Concretization, like succession, "is defined by the progressive reduction of this margin between functions" (*ibid*). All of this makes it clear that Simondon's notion of progress is less akin to evolution (the descriptor he repeatedly uses) than it is to succession.

As it passes through repeated stages of concretization, becoming more and more functionally enclosed, the formerly abstract object begins increasingly to resemble something that can be described as a technical *individual*: a unified and fully coherent entity capable of self-regulation and functional autonomy. Simondon writes that this “individualization of technical beings is the condition for technical progress” (p. 77), defining it as the object’s ability to forge a unique, internal environment for itself. His argument is that the technical object, if it is to maintain or increase its functional autonomy, must avoid hypertelic adaptation to its external environment. It must extricate itself from the web of extrinsic requirements imposed by its surroundings. This necessary decoupling can be accomplished only by internalizing those environmental requirements, such that the object is no longer adapting to its external surroundings but to its own internal conditions: “It could be said that [any] concretizing invention brings into existence a technogeographic environment … which is a necessary condition of functioning for the technical object” (pp. 76-7). Simondon refers to this internal environment of individualization as the object’s *associated milieu*.

Only the existence of an associated milieu can ensure the total enclosure of causality within the object. As Simondon writes, individualization is only “possible because of the recurrence of causality … which the technical being creates around itself” (pp. 77-8). The associated milieu is precisely this shell of fully recurrent causality. On one hand, the milieu maintains the environmental conditions that allow the technical object to correctly function—it is a cause of the functional object. On the other hand, the milieu is itself only created and maintained as an effect of the object’s functioning—it is an effect of the functional object. Each element, object and milieu, is fully the cause and the effect of the other. In this sense, the technical individual, according to Simondon, resembles nothing so much as a natural object:

> The concrete technical object … approximates the mode of existence of natural objects. It tends toward internal coherence, and toward a closure of the system of causes and effects that operate in a circular fashion within its boundaries; and further, it incorporates part of the natural world that intervenes as a condition of its functioning and, thus, itself forms part of the system of causes and effects. (p. 67)

It is clear here that by “natural objects” Simondon is referring to biological organisms rather than, say, geological objects such as glacial erratics. Indeed, this point is emphatically underscored by the omnipresent use of organismal analogy in *Mode of Existence*. Technical objects are said to “evolve” through “phylogenetic sequences” (p. 42). They contain technical elements, each of which “can be compared to an organ in a living body” (p. 86). Their associated milieu serves as the “blood, lymph, and conjunctive tissues”, that those technical “organs” require all around them (pp. 80-1). Innovations spread in a manner “just like seeds” and, once planted, can grow into entirely new technical individuals. And so on. The horizon toward which the Simondonian system of technical progress moves, through concretization and individualization, is not merely a natural object, but the biological organism. This is its point of projected convergence.

It is worth recalling that the notion of an associated milieu—like so much else in Simondon’s work—is clearly anticipated by the mechanisms of Clementsian succession. Clements emphasized the *reaction* of plants back on their habitat, defining the climax formation precisely as that stage of vegetation that was fully adapted to its own (typically *mesic*) internal environmental conditions. Of course, this is precisely the kind of self-adaptation that Simondon is referring to when he writes that, “the technical being evolves by convergence and adaptation to itself” (p. 42). It staves off hypertelic over-adaptation by adapting to its internal “technogeographic” environment. Both systems—Simondon’s and Clements’—achieve their progressive functional enclosure through self-adaptation to a unique internal environment. Both, as we have seen, approach that goal through a series of punctuated developmental stages, in which autogenic succession is driven by the cumulative saturation of a self-limiting system. Both are comprised of components that are reticulated by multiple, strictly subordinated functions. And both develop into something very like a biological organism.

The major difference between the ecological succession of Clements and the technical succession of Simondon is that the former has a pre-defined endpoint: the climax formation, a fully matured super-organism. By contrast, Simondon's technical objects never quite become fully naturalized organisms:

> Now, it can be said only that technical objects tend toward concretization, while natural objects, such as living beings, are concrete from the beginning. It is not necessary to confuse a tendency toward concretization with a status of entirely concrete existence. Every technical object possesses to some degree aspects of residual abstraction; one cannot go so far as to speak about technical objects as if they were natural objects. (pp. 69-70).

Indeed, Simondon emphasizes that, in distinction to other dynamic systems that are driven by simple adaptation, his notion of technical progress never achieves stable equilibrium. In its "successive stages of individuating structuration" it simply goes "from metastable state to metastable state by means of successive structural inventions," without ever reaching a perfectly stable climax (p. 173). Unlike the terminally static version of succession formulated by Clements, Simondon's version "manifests on the contrary a power of evolution that continues to grow from stage to stage, discovering more and more new forms capable of making it evolve instead of stabilizing it and making it tend toward continually reduced fluctuations" (*ibid*). Although total concretization (i.e. naturalization) is the horizon toward which Simondon's technical objects inexorably move, they can never quite reach that horizon, for it is infinitely receding. His model of succession has everything but the finality of climax.

2.4 Conclusion

However original Simondon's work may be within the discipline of philosophy, and however novel it may appear to those of us in the social sciences, it should be clear from the preceding analysis that his system of emergent individuation—at least as it is laid out in *Mode of Existence*—is hardly unprecedented. Indeed, Simondon's process of technical genesis is but a slightly more dynamic version of Clementsian plant succession, which itself was only a slightly more dynamic version of Cuvier's 18th-century anatomical principles of absolute typological fixity.

This continuity is important precisely because the powerful forensic qualities of Cuvier's principles, which I have collectively referred to as *enclosed multifunctional reticulation*, remain invariant throughout Clements' and Simondon's (topically quite disparate) reiterations of Cuvier. Indeed, what is perhaps most notable about the increasing dynamism of each iteration is that, even as the bodies or objects they describe become increasingly malleable and difficult to pin down, the basic conditions of EMR on which they rely do not change. Cuvier took it for granted that his objects already existed in this state of functional and structural perfection. Clements assumed that his objects would invariably achieve such perfection if simply left to their own devices. Simondon argued that his objects tended by their very nature to develop right up to the verge of this perfection, even if they could never quite reach it (we will examine this final insuperable margin in the next chapter). In the first case, EMR is a transcendent property of anatomical objects. In the second, EMR is an emergent but clearly bounded property. In the third, the emergence of EMR occurs but is interminable. But in all three cases EMR constitutes the defining set of requirements through which we are to understand the stability of complex functional entities.

As the degree of dynamism increases across each of these three systems, they necessarily become more complex. Yet, their underlying logical structure does not change. An object can still, in each of them, be reconstructed from its parts. It is merely that the amount of work required to do so increases. For instance, Cuvier could rather easily place each anatomical object into a perfectly stable catalogue of possible animal forms. Clements established a similar compendium of possible climax formations, but he was additionally required to document the multiple developmental stages (and deviations) through which any maturing climax might pass. By contrast, Simondon could not establish any sort of definitive catalogue of objects, since technical innovation and invention assured a constant supply of unanticipated designs. However, even this greatly amplified variability of forms was not infinite, for internal resonance, like animal economy, was by its very nature restricted to a small number of unique forms. The convergence of an object toward internal resonance, wrote Simondon, "gives the technical object its specificity because, at any given time, an indefinite plurality of functional systems is not possible" (p. 45). Internal resonance is a mode of specificity and delimitation; we simply cannot know what new forms of internal resonance might emerge in the future. The internal logic of the catalogue still holds true here, but the work of cataloguing can itself never be finished.

Simondon's technical philosophy therefore offers compelling evidence that trawl nets might, in fact, be forensically reconstructed from their fragments. It suggests that the difficulties encountered in previous efforts to identify derelict trawl nets are primarily epistemological in nature. In other words, if Simondon's model of technical organization and technical progress pertain to trawl nets, then there is nothing inherently wrong with NOAA's net identification database. The latter's current lack of success can presumably be reversed with more thorough data collection, greater levels of practical experience, more powerful analytical tools, increased computing power, more finely tuned models, and so on. Such improvements to the net identification database would obviously not be cheap: they would require a great increase in our (presumably the taxpayers') financial commitment to the database. Yet, those rising short-term expenditures would presumably prove to be a wise investment in the long run. By transforming the net identification database into a useful forensic tool, they will allow policymakers or regulators to implement precisely targeted measures designed to prevent future trawl net derelictions.

However, I will argue in the following chapters that neither increased optimism nor increased expenditures are in fact merited when it comes to NOAA's database. The lack of reliable knowledge about trawl net designs is not only epistemological in nature. It is not a matter of better record keeping or more processing power. Instead, I will show that the difficulties of trawl net identification must properly be understood as ontological: tied to the very way in which trawl nets exist and function as technical objects. Simondon's model, as we shall see, only holds true under certain specific conditions of technical function. While the overwhelming majority of technical objects meet these basic conditions, trawl nets, intriguingly, do not. Accordingly, we will have to reassess the way in which trawls function and what, therefore, we can know about them.

Chapter 3 . The Puzzling Progress of Trawl Nets

In the last chapter I argued that there is striking isomorphism between Gilbert Simondon's philosophy of technical evolution and earlier, topically quite distinct, systems of forensic reconstruction based on the principles of enclosed multifunctional reticulation (EMR). In particular, I sought to illustrate that Simondon's work recapitulates the progressive version of EMR pioneered by Frederic Clements in his theory of autogenic plant succession. However, there is quite obviously a key difference between Clements' version of progressively enclosed multifunctional reticulation (PEMR) and Simondon's: While plant succession occurs on its own (indeed, Clements often argued that it is wholly incompatible with human activity) Simondon's system of technical concretization depends on the constant oversight and intervention of humans. Put simply, technical objects cannot regenerate and modify themselves without a great deal of human help. The question of whether trawl nets concretize is therefore a question of whether humans are able to help them do so.

In this chapter I will analyze the role of humans in technical concretization, highlighting, in particular, what is required of any technical object in order for the concretizing form of progressive human interventions to occur. My argument will be that Simondonian concretization is hardly a foregone conclusion; that it is not an automatic and self-perpetuating process that simply occurs, by default, to all technical objects in equal measure. Rather, a careful reading of Simondon suggests that an object's ability to undergo concretization is closely tied to the particular manner in which it carries out its intended functions. More specifically, an object becomes more or less apt for concretization depending on the manner in which flows of energy and information are exchanged during its operative functioning.[13] As we shall see, technical progress, to Simondon, has a specific meaning in relation to those flows of energy and information, modifying them in very particular ways. In order to understand what makes concretization possible—to clarify what impels technical

---

[13] I use the term 'operative functioning' to designate the collection of active, internal processes by which a technical object produces its intended technical result. The term is admittedly somewhat ungainly. However, it is far less ambiguous than similar terms such as 'function', which can be taken to refer to an object's intended purpose, and 'operation', which can be taken to refer to the way in which a human wields, employs, or uses it.

objects toward ever greater levels of internal resonance and forensic reconstructability—we must therefore examine in greater detail Simondon's notion of technical function.

This chapter is divided into two main sections. The first section will more closely examine Simondon's notion of concretizing progress, as it is explained in *The Mode of Existence*. In so doing, it will necessarily explain how Simondonian technical objects function, for his notion of technical progress is tightly fused to his notion of technical function. Indeed, an analysis of Simondon's text will make it quite apparent that his technical objects can undergo concretization only insofar as they function through a particular structure or set of structures that he refers to as a *mechanical transducer*. I will highlight the precise conditions of energy and information flow that characterize this model of mechanical transduction, and show how it is related to the notion of concretization. This initial section of the chapter will establish a sufficiently firm footing from which to ask whether or not trawl nets actually undergo concretizing progress—and whether, accordingly, they can be expected to avail themselves to forensic reconstruction.

This is precisely the question that we shall address in the chapter's second section. The answer, as we shall see, is somewhat surprising. For, technical progress in trawl nets is oddly bivalent. Quite pedestrian in some aspects, it is downright puzzling in others. The overall trajectory of trawl net progress is confounding not only because it largely departs from the Simondonian model, but because it does so in a manner that was apparently not anticipated by Simondon himself. In other words, not one of Simondon's explanations for why an object might fail to concretize convincingly accounts for that failure in trawl nets. I will argue that this draws us toward an inescapable conclusion: perhaps, if trawl nets largely fail to concretize, it is because they do not function according Simondon's model of mechanical transduction.

3.1 How things work: Technical function as a transductive operation

To begin we might simply ask how it is, according to Simondon, that a technical object functions. How does it carry out its intended task? Simondon writes that at the core of any technical object's operative functioning there takes place a unique process of *transduction*. Transduction occurs when two disparate and seemingly irreconcilable energetic domains are brought into a sort of productive proximity with one another. The profound difference or incongruity between the domains constitutes a form of potential energy, and that potential energy becomes actualized through the workings of the technical object, producing a novel result. In the simplest sense, then, transduction is the conversion of one form of energy into another, potential energy into actualized energy. But it is also more than that, for its reconciliation of anomaly or discord occurs not through the purely negative action of compromise, in which the potential of both domains is mutually diminished, but rather through the collaborative and positive creation of something that is greater than the mere sum of its parts.

A stapler, for example, brings together a thin strip of metal and the power of a hinged crushing lever. However, the resultant transduction does not simply mangle the staple into a twisted wreck. This would be a wholly compromising result, negatively affecting both domains (albeit imperceptibly in the case of the lever) without leading to the positive creation of any new capacities. Rather than performing such a blind disfiguration, the stapler brings the metal strip and the lever into a very specific interaction, resulting in the positive creation of something entirely new: an unobtrusive and nearly failsafe clasp for stacks of paper.

The most crucial aspect of the lever-staple interaction—what makes it a technical operation rather than just a brute physical collision, like a slab of rock falling off a cliff—is the degree of precision with which the two domains are articulated and put into contact. Indeed, the stapler functions only because it sets up the impending transduction in an exceedingly specific manner. Stapling depends on a large number of marvelously precise arrangements: the variations of strength and flexibility along the curved length of the staple; the firmness with which it is horizontally positioned in the magazine by its neighboring staples, as well as by the follow block, follow rod, and follow spring; the ease with which the staple can be vertically sheared from the glued row of staples; the width and rigidity of the plunger blade that performs that separation; the sharpness of the staple's prongs; the hardness, smoothness, and curve of the anvil onto which those prongs are pressed; the curve and temper of the leaf spring that quickly returns the upper assembly to its original position when the lever's downward pressure is diminished; and so on. Stapling is possible only because of the exactitude with which all of these elements set up and prepare the transductive operation in advance.

In his well known critique of the *hylomorphic model*—a view of the world in which matter and form are strictly divided, and in which technical operations are seen as the mindless molding of brute (i.e. objective) physical matter into mentally pre-established (i.e. subjective) forms—Simondon described every technical operation as the ultimate result of two lengthy chains of prior elaboration that come fleetingly but productively together. While it is not my intent to expound upon his critique of hylomorphism, it is worth bearing this pincer-like model of technical function in mind. The operative functioning of a Simondonian technical object involves extended chains of highly detailed preparation, such that the final technical event occurs in as narrow and precise a way as possible.

Simondon, finding no preexisting word that adequately describes this qualitative degree of transductive precision, is compelled to invent one of his own: *technicity*.   An object's technicity increases as its specific transduction becomes ever more precisely and infallibly arranged. In this sense, technicity is the qualitative "degree of perfection" of the transductive interaction, or the "degree of concretization of an object" (Simondon 2007, p. 92). Hearkening back to the concepts elaborated in Chapter 2, we could say that an object's technicity designates the degree to which it is internally resonant during its operative functioning, such that its every feature contributes in a positive and focused way toward the completion of the transductive procedure.

The unity of purpose displayed by an object with a high degree of technicity, its transductive exactitude, has a corollary that is perhaps rather obvious: because it is so precise, the transduction performed by such an object is also very consistent. A poorly crafted stapler, used repeatedly, will produce a series of staples that are mashed into any number of conceivable forms. Indeed, some of them may take on a crushed shape even before being fully ejecting from the magazine, thereby jamming up the entire device. Such a stapler's potential energies are actualized imprecisely and therefore unpredictably. It functions in a manner that, although nominally planned in advance, is not unlike rockfall from a cliff: while it certainly converts potential energy into actualized energy, it does so in such a brute manner that its precise results can never be repeated or guaranteed a second time. By contrast, a high quality stapler will produce a virtually identical paper-clasp every time it functions. For this reason, Simondon emphasizes that "technicities can be conceived of as stable conduits reflecting the characteristics of the [technical] elements rather than as simple qualities. They are forces in the fullest sense of the word; that is to say, they are capacities for producing or undergoing an effect in a fixed manner" (Simondon 2007, p. 94). An object of high technicity, producing the same effects from the same causes every time it is used, employs and enforces a strict linearity of causation.

There is also another, somewhat more counterintuitive, corollary to this high degree of effectual fixity: because the "conduits" of transduction are so precise and so predictable in an object of advanced technicity, the wielder of the object can use it to perform a relatively wide range of purposeful actions. In other words, its operation can undergo very small and specific modifications on demand. The stability and acuity of its transduction means that its effects can be easily modulated to produce a slightly different result. For instance, a blade of Damascus steel can cut far more nuanced patterns into a block of wood than a cheaper metal blade or a jagged stone. A well-designed stapler can be used equally well to staple two sheets of paper together or fifty, as long as an appropriate amount of force is used to depress its head. A good soccer boot can be used to confidently send the ball on a slow, straight ground pass or on a fast, curving aerial shot—or indeed along any number of desired trajectories. What all of these examples demonstrate is that an object of high technicity is notable for its responsiveness: it is receptive to information transmitted by the user—i.e. changing intensities of input—and it effectively transmits information back to the user in the form of feedback (whether in visual, haptic, auditory, or any other form). A highly developed technical object, even one as simple as an athletic shoe, creates a clear two-way flow of information between the user and the object. The higher an object's degree of technicity, the more it will foster this critical flow of information between the object and its user.

3.2. Transductive perfection: The mutual segregation of energy & information

For Simondon, technical progress can in this sense be measured as the ever-increasing ability of the human and the object to communicate with one another; their capacity to mutually and unambiguously exchange information during the technical operation. This exchange, Simondon insists, is something that must be fostered and improved in a specific manner. Namely, it requires that the flow of information in the object be cleanly separated from its ordinary (*i.e.*, operative) flows of energy. It is worth pausing for a moment in order to examine what this separation of energy and information actually entails, both in relatively small and simple technical objects as well as in larger and more complex ones.

All of the examples I have cited so far—knives, staplers, footwear—involve rather simple and uncomplicated technical objects. According to Simondon, what is most distinctive about such objects is not their small size or lack of complexity, per se, but rather the unique way in which they channel information and energy. Simple handheld *tools* such as these, which Simondon also refers to as artisanal or craft objects (Simondon, 2009), require the human being to act as both the source of energy and the source of information for the technical operation (Simondon 2007, p. 132). When using a maul to split firewood, for instance, the human wielder of the maul must use his muscles to provide the tool's operative energy. But he also uses the resultant vibrations transmitted up the maul's handle as information about its operation, thereby enabling him to adjust the precise trajectory and force of his next swing. In this sense, the maul is both a tool (an extension of the worker's body) and an instrument (an extension of the worker's perceptual apparatus), though Simondon would argue it is primarily a tool, since it only serves its information-transmitting role when it is used for an energy-transmitting purpose (Simondon 2007, p.133). (Note that Simondon uses the example of a hammer, rather than a splitting maul.)

When a simple tool undergoes technical progress, its energetic and informational pathways become ever more clearly defined in relation to each other. In other words, progress occurs when the maul becomes physically more precise and, for that very reason, more responsive in the sense of being able to receive and transmit information with its user. The potential for information exchange may certainly exist in the operation of a dull-bladed, poorly weighted, and short-handled maul, but the unintelligibility of that information is assured by the sloppiness of the maul's energetic pathways. A tiny vibration remains meaningless to the operator of a poorly crafted maul, since that particular quivering is most likely inseparable from the myriad other random vibrations in the maul's handle. By contrast, within a maul of superior design and construction, energy and information are less intermingled and more easily distinguishable. Any tiny vibration takes on great significance to the operator of a well crafted maul, since it is clearly distinct from that maul's normal, predictable, and precise flows of energy.

Crucially, as the artisanal object or tool undergoes progress—as the maul becomes more precise and more responsive—it also becomes physically less difficult to use. Its increasing level of technicity, its greater transductive precision, is clearly felt in the newfound ease with which its human user is able to perform his or her customary technical actions (Simondon 2007, p. 132). The technical operation of swinging the maul becomes easier and more exact; it becomes more successful in general, and that increased level of technical success is unambiguously *felt* in the body of the individual user. No one needs to tell the wood splitter when a certain maul works better than another; nor must the wood splitter cogitate at length over the relative efficacy of the two mauls.

When one works better than the other, he or she will simply know it. The detection of progress is, in a sense, something that occurs automatically.

In principle, Simondonian progress occurs no differently for vastly larger and more complex "industrial objects" (Simondon, 2009). What distinguishes such objects from artisanal objects is not their mode of progress, per se, but rather the fact that human beings do not act as their dual source of information and energy. For in contrast to tools, which employ human muscle power, Simondon writes that industrial objects outsource their immense energetic demands. Bypassing the individual worker and his relatively puny bodily motions, the industrial object instead harnesses the vast reservoirs of energy that are available in natural physical systems. Technical operations of the industrial sort draw their energy from the thunderous descent of water through a hydroelectric power station, or the explosive pressures generated through the production of steam in an enclosed boiler. With the technical operation no longer effected through his corporeal skill, the industrial worker often perceives himself to be abandoned by progress, rendered obsolete by it rather than being made more efficient and physically capable. Technical progress in the industrial era therefore became something with overwhelmingly negative connotations for the individual worker (Simondon 2007, p. 133), since increasing levels of technical success were no longer *felt* with any clarity in his or her body.

The positive flipside to this sense of personal alienation, Simondon declares, is that "when he borrows energy from a natural source, the human being discovers an infinite reserve, and comes to possess considerable power" (Simondon 2009, p. 21). Indeed, the technicities of industrial objects often arise directly from the colossal scale at which they are able to utilize energy, a fact that explains their tendency toward physical gigantism. Their technical effects are made possible only by massive influxes of energy and correspondingly massive structures capable of transmitting that energy without annihilating themselves. In the industrial object, to reduce or impede this titanic energy flow is inherently to reduce or impede its technical precision and effectiveness as well—in other words, to reduce its technicity. In this sense, energy impediment of any kind nullifies or reverses the technical progress of industrial objects.

The most likely source of such energy impediment is the transmission of information within the industrial object, for the following reason: The industrial object is so removed from the human body that its operation must be modulated by the continual input of electronic or mechanical signals instead. If these informational signals utilize the same pathways as the object's operative flows of energy, then their transmission will necessarily sap or obstruct some of that energy, causing a decline in the object's effectiveness. In those informational moments, the technical object, rather than being highly concretized and internally resonant, will be working against itself. It will be characterized by internal strife, functional discord, and a lack of concretization. Simondon writes that for this reason, "There exists in this lack of differentiation between the … energetic channel and the … information channel, a serious obstacle that greatly diminishes the effectiveness of the regulation and the degree of individualization of the technical being" (Simondon 2007, p. 146). Progress in industrial objects requires the ever more effective separation of information and action into separate channels, such that the former will never obstruct or impinge upon the latter. In this sense, technical progress is no different whether it is artisanal—think back to the vibrations in the handle of the splitting maul—or industrial. Increasing an object's degree of transductive perfection involves, in every case, the enhanced separation of its energy and information flows into distinct channels.

What is strikingly different about progress in industrial objects, as compared to progress in tools, is the manner in which it is recognized, its detection. Since the individual human no longer plays any important energetic role in such an object's operation, and is instead reduced to a purely informational role, the task of the industrial worker becomes essentially one of oversight, optimizing the object's operative functioning, modulating it through the continual input of commands and exchange of information. "The individual," Simondon writes of the industrial era, "is converted into the mere spectator of the results of the functioning of machines" (Simondon 2007, p. 134). With this switch from physical labor to attentive observation, any changes in the technical object's efficacy and efficiency, changes previously registered in the body of the worker, are instead converted into abstract matters for technicians or engineers to measure and judge. The increasing success of the technical operation becomes something that is not experienced in one's own nerve endings, but is rather "thought of in an abstract form, intellectually, in a doctrinal manner" (*ibid*). Accordingly, for technical objects at an energetic scale above that of the human body, "it is no longer artisans but mathematicians who think progress" (*ibid*). As Simondon avers, "progress is apprehended as a movement that is detectable by its results, and not in itself, [not] in the ensemble of operations constituting it, [nor] in the elements that realize it…" (Simondon 2007, pp. 134-5). Without its immediate registry in the nerves and muscles of the human body, progress becomes something ill-defined, a pattern of results that must be detected through the highly trained observation of the technical operation. In sum, the ongoing improvement of the technical object—i.e., progress, concretization—hinges on the ability of human observers to somehow detect miniscule but meaningful changes in its results.

But how does this detection actually occur? How can the relative success of the technical operation be registered, and an ineffective object thereby be targeted for improvement, once that object is no longer operated as a physical extension of the human body? And what, for that matter, is actually detected? What is it that guides the arrow of progress for technical objects of an industrial scale? How is it, in sum, that an industrial object is able to communicate information about its relative efficacy or inefficacy with its human users? Answering this set of questions requires that we reach to the very core of Simondon's notion of technical function.

3.3. The mechanical transducer: A fleetingly closeable gap

As I have already noted, for Simondon, an object's technicity is a measure of its transductive precision, its consequent degree of adjustability or responsiveness, and its closely related ability to separate information from energy. At the center of all of these traits—and therefore at the center of the notion of technicity itself—is an indispensible technical feature that Simondon initially refers to as the *transducer* and then, later in *The Mode of Existence*, more specifically calls the *mechanical transducer*. He explains that this is

> an adjustable resistance interposed between a potential energy and the place of actualization of that energy: [a] resistance [that] is adjustable by a piece of information external to the potential energy and the actual energy. … [A] perfect transducer … is truly the mediator between both domains, but it is neither a domain of energy accumulation nor a domain of actualization: it is the margin of indetermination between both domains, that which leads potential energy to its actualization. Information intervenes in the course of this passage from the potential to the actual; information is a condition of actualization. (Simondon, 2007: pp. 159-160)

Simondon's explanation is quite abstract, and intentionally so, given that he will ultimately generalize the notion of transduction from technical objects to a broad range of other, non-technical objects. For clarification purposes, however, the notion of a mechanical transducer can quite easily be applied to certain sets of components within everyday technical objects.

In a simple gasoline engine, for example, the carburetor acts as a mechanical transducer. The carburetor is a device that facilitates internal combustion by producing an optimal blend of gasoline and air within the engine. It is what combines tiny droplets of gasoline with moving air to produce the combustible mist that is so productively ignited during internal combustion. The carburetor clearly serves as a piece of resistance in the operative functioning of that engine, since its primary effect is to strictly regulate the admission of gasoline and air, preventing each from flowing freely into the combustion chamber. Yet the carburetor is also an *adjustable* piece of resistance. Its actions can be modulated through the manipulation of a throttle, a valve whose adjustment permits greater or lesser amounts of air to enter the engine (thereby altering the amount of fuel that is drawn into the engine by that moving air).

Within the internal combustion engine as a whole, we can see that the carburetor is delicately interposed between the potential energies of the engine and the place where those energies are actualized. Potential energy exists primarily between the highly volatile aerosolized fuel-air mixture, and the enclosed combustion chamber, with its sparkplug, its attached pistons, and so on. Both the fuel and the assorted engine parts have been painstakingly crafted, through long and complex chains of technical elaboration, in such a way that the potential energy between them is maximized. The only thing holding them apart from each other is the carburetor. When the human user of the engine opens the carburetor's throttle, typically through the use of an accelerator pedal, the desired fuel-air mixture flows into the combustion chamber, where it is ignited by the sparkplug, producing an explosion which drives the piston outward, and so on. The energy that was once only potential is now actualized and the engine begins to function. Crucially, in order for this functioning to occur, it is mandatory that a human being—any human being—tell the engine, through the carburetor, how much air and fuel to admit. A human being must provide this piece of missing information for the engine, and does so by depressing the accelerator pedal a certain amount. Without the input of that critical piece of information, the engine cannot complete its operative functioning, cannot convert its potential energies into actualized energy.

According to Simondon, all technical objects incorporate something like a carburetor into their operative functioning. They all contain a mechanical transducer: a part or collection of parts that can accept information from a humans user in order to regulate or modulate the conversion of the device's potential energy into actualized energy. Although Simondon does not explicitly say so, the text of *The Mode of Existence* makes it clear that the transducer performs this crucial regulatory work through a sort of dual segregation or separation in the operative functioning of the object (see Figure 3.1).

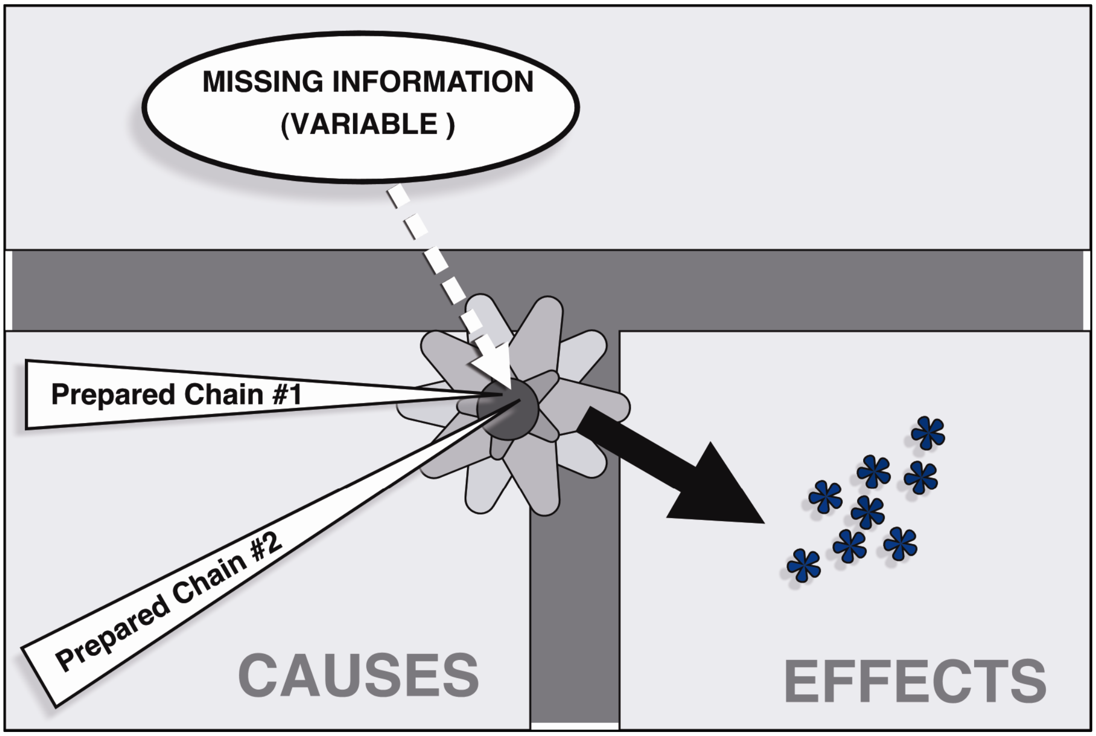


Figure 3.1 Simondon's model of mechanical transduction. The transducer (dark circle at center) effects both a *horizontal* separation of causes from effects and a *vertical* separation of information from the object's flows of energy.

On one hand, the transducer is the critical piece of resistance that to some degree links together (and therefore also inherently separates) the two disparate domains involved in the object's transductive interaction. It constitutes the exquisitely forged point of connection between the two transductive domains, the infinitesimal space or opening between the twin pincers of technical elaboration. It therefore defines and stabilizes the relation between the object's technical causes (embodied in the twin chains of elaboration) and its technical effects. It is a bottleneck through which all operative functioning must pass, the cipher that allows the steady conversion of correctly calibrated inputs into predictably repeatable outputs. In this sense, the transducer can be envisioned as a sort of gatekeeper whose role is to ensure that the technical operation will move in a controlled and orderly manner from its causes to its effects. It is responsible, in other words, for enforcing the object's predetermined mode of linear causation. We might, if only for the sake of labeling it, call this assurance of causal linearity the *horizontal* separation that is performed by the transducer.

On the other hand, the transducer also carries out an important *vertical* separation. By this I mean that it isolates the energetic flows of the technical object, its ongoing productive actions, from the informational feedback that is crucial if it is to attain optimal performance. As we have already seen, Simondon's technical object can only function in an internally resonant way when it utilizes separate channels for energy and information. However, it is clear that this separation cannot be absolute, since a piece of information submitted by the operator, if it is to have any effect at all on the object's operation, must at some point intercept and interject itself into the technical object's play of operative energies. Otherwise the technical object would be a monad, isolated entirely within itself, nonadjustable, unresponsive, automated in the worst sense of the word. It would be very nearly unusable. There is, then, a certain sense of paradoxical requirement in Simondonian technical function: information must not interrupt an object's operative functioning, but it must nevertheless be able to modulate that functioning on command. It must intervene without interfering.

Stuck between this Scylla and Charybdis, Simondon settles for a sort of delicate compromise: if the point of intervention cannot be entirely eliminated, it can at least be rendered more or less unobtrusive by its minimization to a single staccato instant in the object's functioning. Simondon refers to this flickering moment of permissible intersection as the object's *margin of indetermination*. Every technical object, he writes, must have an operative margin of indetermination. No object can function without ongoing flows of information, and yet, because the inherently underdetermined nature of information (who can ever say what news a message will bring?) threatens the object with precisely the sort of transductive imprecision that progress is meant to eradicate, the moment of indecision embodied by information input must be confined to the briefest of moments and most localized of positions in the object's functioning. In this way, its functioning will remain as nearly seamless as possible; its causes and effects will retain their intended linearity and predictability. It is to maintain this functional linearity that information, in a Simondonian technical object, interjects only in discrete bits, tiny and fleeting signals that are minutely localized in a marginal but critical phase of the object's functioning. This margin of indetermination is the vertical separation that is achieved by the mechanical transducer.

It should be emphasized that Simondon's transducer is not exactly a *thing*. It is not a well-defined object or structure that one might, for instance, simply pick up off the shelf and purchase. Rather, as Simondon suggests, transduction is a "notion [that] can be generalized" away from any single physical manifestation such that it encompasses many "different species of transducers," all of which exercise "a regulatory function" over their technical object through the maintenance of "a certain margin of localized indetermination in their functioning" (Simondon 2007, p. 160). No matter what physical form or collection of forms is taken by the transducer in question, its key attribute is in fact its formlessness: the hollow that it embodies, the margin of indetermination with which it effectively separates technical actions from technical information and technical causes from technical effects. Simondon's transducer should therefore not be confused with the specific electronic device—perhaps familiar to the reader—that forms part of a trawling vessel's echosounding system, and is used to help in the underwater location of fish and the monitoring of the trawl mouth during the tow. Simondon's is a technical "component" of an entirely different order.

3.4 The intelligibility of technical success: How objects emit information

Both of these separations, horizontal and vertical, are necessary if the engineer or technician is to fruitfully and mutually exchange information with the object. On one hand, it should be quite clear that the mechanical transducer's vertical separation is what allows the human user to transmit bits of information to the technical object from outside. The transducer delivers that information into the machine's margin of indetermination in the form of brief, clearly defined signals. These signals, in turn, elicit what is typically a pre-conditioned response from the object. A technical object, Simondon reminds us, is only able to receive signaled information because it "carries with it that which can be named the system of decision schemes; before making the machine function, it is necessary to program it" (Simondon 2007, pp. 158-9). So when humans use the transducer to give commands to the object, it responds in a prearranged manner. Changing the command signals that it receives makes it shift from one predetermined outcome to another that is equally predetermined. This is how humans communicate to technical objects.

This notion, of humans transmitting discrete signals to a machine, and of the machine responding to those commands in a predictable way, may not seem especially novel. Indeed, Simondon only makes this claim by way of explicit agreement with the cybernetic theory of information already pioneered by Norbert Wiener. Yet Simondon also makes an important denunciation of and addition to Wiener's theory. Where the cyberneticists get it wrong, Simondon insists, is in their failure to recognize that technical objects, through their very act of functioning, also emit information back to their users. While the former notion—of a localized margin of indetermination into which human signals are transmitted—"is not absent in the work of cybernetic authors," Simondon writes, "what is lacking in [their] study is the notion of reversibility of the reception and emission of information. If a machine presents a functioning that has critical phases [of indetermination] ... it can emit information as well as receive it" (Simondon 2007, p. 158). And in this previously overlooked emission of information, the transducer plays an equally pivotal, if less obvious role.

If technical objects are able to clearly transmit information back to their operators it is due to the stabilizing *horizontal separation* performed by their mechanical transducers. That is to say: the transducer, insofar as it regulates and maintains the object's linear regime of causes and effects, encourages or allows the object to function in accordance with the parameters laid out in its original blueprint or model. In its role as functional gatekeeper, the transducer assures that the object will function in a manner wholly consistent with the way its functioning was imagined during the act of invention. It is in this sense that Simondon can describe the technical object as a "bundle of actions and reactions where the game is predicted and calculable" (Simondon 2007, p. 236), since it "effects a determined operation [and] realizes a certain functioning according to a determined model" (Simondon 2007, p. 262). The technical object has a normal mode of functioning, a standard rhythm of operation, a stable baseline of anticipated actions and reactions. Therefore, as it functions, the object will quickly betray any operational variations away from that baseline. Any anomalies in its functioning, any inefficiencies or shortcomings or contradictions, will quickly become apparent to a human observer who is familiar with its design. "[T]he functioning machine," as Simondon writes, "suffers or produces a certain number of variations around the fundamental rhythms of its functioning, such as these result from its defined forms. These variations are meaningful, and they are meaningful in relation to the archetype of functioning, which is that of thought in the process of invention." (Simondon 2007, p. 155). So a technical object's operative functioning can itself take on the value of information, but only insofar as it is observed to deviate from its diagrammatic baseline over time. Conversely, the object's operational fluctuations cannot be assigned any firm meaning in the absence of that baseline.

It is only through the stabilizing action of the transducer, its assurances of functional linearity, that observed alterations in the object's technical output can be not only observed, but assumed to have a definite and identifiable set of causes in the technical inputs. The transducer is therefore what allows the object's deviations to present, for the engineer or technician, a kind of problem demanding resolution.

3.5 The Living and the Technical

Simondon stresses that this kind of *problematizing* information is simply not available to technical objects themselves, for they can neither pose novel problems to themselves nor invent novel solutions for them. They are stuck eternally in the present moment, acting in a purely immediate way when confronted with information. Technical objects have no means of reassembling and integrating scattered bits of information in a meaningful way, as in human memory; nor do they have any means of conditioning their actual state of affairs through recourse to a virtual future, as in human invention. Equally bereft of past and future, even very complex or data-driven technical objects are condemned to function in a highly predetermined and mechanical (i.e., linear) way. Notwithstanding their ability to undergo some degree of modulation during the course of their operative functioning, they still function through "a stereotype of successive gestures according to a predetermined conditioning" (Simondon 2007, p. 142). For this reason, the continual self-monitoring, self-regulation, and self-improvement of a technical object

> cannot be completed by the machine alone, even if it is perfectly automated. The type of memory and the type of perception that are suitable [for such a task]… require integration, the transformation of *a posteriori* into *a priori*, which only the living realizes within it. There exists something living in a technical ensemble, and the integrating function of life can only be assured by human beings… (Simondon 2007, p. 143)

This, Simondon claims, is why technical progress has emphatically not left humans behind, despite their obsolescence as a source of energy for most technical objects. Even the most highly concretized industrial objects require human information processing, human memory, and human imagination in order to function and concretize over the long term:

> The human individual appears, then, as that which must convert into information the forms deposited in machines; the operation of machines does not give birth to a piece of information, but is only a gathering and modification of forms; the functioning of a machine has no meaning, cannot give rise to true information signals for another machine; a living being is needed as a mediator to interpret a functioning in terms of information, and to reconvert it... (Simondon 2007, p. 154)

The power to integrate observations into information, using memory and imagination, is an ability that machines simply do not have. Information for them always takes the form of a command, a simple and present set of conditions, rather than a complex and shifting array of options. Human observation, human integration and interpretation of the variations produced by the functioning object, are what enable technical progress to occur. The latter is made possible through the ongoing and active comparison of the object's actual results with the virtual results that were inked out in its original blueprint. The human act of invention (which is itself transductive) harnesses the tension or the potential energy that exists across this gap, actualizing that potential in the form of a new, problem-solving creation. The development of highly concretized, individualized, and internally resonant technical objects is therefore inextricably linked to the inimitable cognitive capabilities of living beings.

Yet this necessary linkage between living things and technical matter is also effectively a separation. For if living beings, and humans in particular, are crucial to Simondon's notion of technical progress, they are also relegated to a very specific position within the ongoing technical operation: set apart, detached, able to transmit information into the transducer but clearly relegated to one side of it, whence they can observe the functioning technical object in its entirety. The living and the technical certainly interact through this constant exchange of information but they do not exactly intermingle. The living are kept out of the technical operation itself, which as we have already seen is regulated exclusively by an instantaneous, discrete, and signal-based form of information. This separation of living and technical matter, and the creative tension that it enables, is a hallmark of Simondonian technical function and progress.

3.6 Progress and Functional Over-determination

How, then, does concretizing technical progress occur? According to Simondon, it is a result not only of one kind of information exchange (e.g. the input of signals) or another (e.g. the observation of results), but of the mutual and ongoing communication between technical objects and humans. Progress, in other words, requires both the submission of information to technical objects by humans, and the emission of information back to those humans by the objects.   Indeed, in the examples of concretization given in *The Mode of Existence*, the meaningful variations in an object's operative functioning (emitted information) tend to become apparent precisely through the user's attempts to modulate its performance (submitted information). Adjustments in the intensity of a technical operation are what reveal an unforeseen threshold beyond which the object ceases to operate according to its initial design or model.

For instance, Simondon explains that the self-limiting quality of the diode became apparent only when its cathode temperature was elevated to such a degree that it unexpectedly ran out of available electrons, ceding all of them to the anode (Simondon 2007, pp. 64-6). In other words, the conflict between the diode's expected mode of functioning and its actual functioning emerged only because its users operated it at such high cathode temperatures. The diode's shortcomings (emitted information) were brought into stark relief through the controlled modulation of its operative functioning (submitted information). It was therefore the diode's dual role as information receiver *and* information emitter that allowed for the subsequent invention of the triode. This example, and others like it in *The Mode of Existence*, illustrate that Simondonian technical progress is driven not by observation alone, the output of information, but also by the careful modulation of the technical operation's causes, the input of information. Progress requires a signal-based form of operative functioning, centered on the regulatory capabilities of a mechanical transducer. Only once this regime of energy and information exchange is in place can human technicians fruitfully observe the functioning object as they modulate its operation—putting it, so to speak, through its paces. Where the technical results diverge from the expected ones (i.e. where the technical causes do not lead to the predicted technical results), the amount of deviance can be subjected to measurement and calculation.

Why does it matter, though, if a technician observes that a technical object produces unexpected results under certain conditions? Of what importance is it that an object produces a slightly different effect than the predicted one? In Simondon's philosophy, such results are troubling precisely because they reveal the existence of some unforeseen source of transductive imprecision within the object. They show that something in its operative functioning is behaving like a subtle rockfall, producing results that are blurred, inconsistent, and wasteful. Technical progress occurs when this mysterious source of technical imprecision is located and articulated more clearly, such that it can be productively channeled back into the functioning of the overall object. Once it is identified, the unsanctioned indetermination spilling out into the object's operation must be corralled back into the narrow and carefully controlled confines of the margin of indetermination. Simondonian progress can be envisioned as a never-ending campaign to round up and remove all points of possible variance from a given object's operative functioning, relocating them into a single specific instant that is wholly subject to human modulation. It is the continual "resettlement" of the technical object's functional vagaries into a kind of ghetto within its mechanical transducer, a confined space or enclosure within which technical imprecisions will be better controlled and less likely to run amok. Progress is the perpetual concentration of indeterminacies within the margin of indetermination.

The most failsafe way of removing all forms of indetermination to the specified margin is to functionally *over-determine* all of the object's other components. A newfound source of imprecision will best be removed from the midst of the technical operation when it is collectively shunted aside by all of the object's parts working together; when, in essence, all of the object's components are equally forewarned against it. When the components become functionally over-determined in relation to each other, when they achieve a high degree of internal resonance, then there will remain no available space for that imprecision to inhabit. Concretizing progress is just this: a perpetual restructuring of the object that leaves unforeseen indeterminations nowhere to hide and no way to surprise. Concretization moves toward the wholesale commitment, by all of the object's components, to relegate all sources of imprecision to the margin of indetermination. As this functional over-determination occurs, the object's operative transduction becomes ever more precise and ever more consistent. Its technicity increases. Concretization is the ongoing perfection of the margin of indetermination, its ever-increasing power or potency.

In sum, we can state that concretization is possible only for technical objects whose operative functioning is modulated by a mechanical transducer. Mechanical transduction, with its strictly enforced causal linearity, its requisite separation of information and energy, and its symbiotic division of living beings and technical objects, is the particular mode of functioning that makes it possible for the technical object to undergo concretizing progress. Objects avail themselves to concretization—or, rather, to successive concretizing interventions by humans—because they function in this specific manner. Simondon seems to have taken it for granted that all technical objects function though the process of mechanical transduction. He therefore attributed the failure of certain technical objects to concretize not to their mode of functioning, which was not open to negotiation, but to a number of other possible reasons that we shall examine shortly. However, his omission in this regard should not be mistaken for proscription. His failure to identify other modes of operative functioning does not exactly mean that such alternative modes cannot exist. Indeed, it is quite conceivable that one could adhere to the Simondonian model of the technical object while also expanding upon or diverging from his mechanically tranductive notion of technical function. In the remainder of this chapter, I will argue that the trajectory of progress in trawl nets pushes us toward just such a novel conclusion. For when we pose the question of whether or not trawl nets concretize, there is no simple answer. Trawl nets do concretize, but they also do not. And where they do not concretize, their failure to do so is not explained by anything in *The Mode of Existence*. As we shall see, this leaves their mode of functioning as the primary variable that might explain their peculiar mode of progress.

3.7 Concretization in trawl nets: a familiar but minor form of progress

Let us turn now to the question of whether trawl nets undergo concretization. If they do, then it is reasonable to suspect that they can be forensically reconstructed through the tenets of PEMR. If they do not, then it is incumbent on us to explain why they do not and to identify how they are instead structured. Progress in trawl net design has been described as advancing along four fronts (Winger et al., 2006). Specifically, it involves the development of increasingly sophisticated methods for achieving one or more of the following four goals: (1) increased fuel efficiency—i.e. the reduction of hydrodynamic drag; (2) improved size selectivity—i.e. the ability to catch only those individual fish that are larger than the minimum landing size; (3) compliance with increasingly stringent bycatch regulations—i.e. a reduction in the amount of the catch that consists of non-target species; and (4) reducing the environmental impact of trawls—i.e. the minimization of their physical contact with the benthos itself.

There is, in this four-part categorization, a rather striking divide between mechanical mechanisms, on the one hand, and biological mechanisms on the other. The first and fourth categories (fuel efficiency and benthic impact) involve purely physical modifications of the net. Such improvements do not require the presence of fish or other target animals in the trawl. They are manifest in the very act of towing the net through the water, and would therefore constitute a form of progress even were the trawl simply being towed through a large, heavily chlorinated swimming pool. By contrast, the second and third categories (selectivity and bycatch reduction) involve modifications that are meaningful only insofar as there are fish moving into and through the net. These improvements manifest themselves in the net's ability to sort out fish of different sizes and species with a certain level of precision. Their effects are felt only in the presence of biological “raw material”. This notable division between mechanical and biological design elements—a distinction which at first glance seems to follow the traditional fault lines of the academic rift between nature and society—is in fact far from arbitrary. Indeed, acknowledging its existence is crucial to any understanding of technical progress in trawl nets, for the trajectory of progress on one side of the divide looks nothing like the trajectory of progress on the other.

When it comes to the purely physical or mechanical improvement of trawl nets, progress occurs in a way that looks very much like Simondonian concretization. As an example we could point out some of the numerous inventions that, in recent years, have increased the fuel efficiency of nets. One such invention is the development of Ultra Cross knotless netting. Traditionally, the mesh netting used in fishing nets was constructed by knotting twines of a certain material and a certain diameter together at regular intervals. A simple knot known as the sheet bend was typically used for this purpose (Figure 3.2a). Knotted construction was necessary, of course, not only to create the mesh spaces themselves (known as *lumens*), but also to preserve their size and shape, thereby preventing the cumulative distortion of the entire net over time. Knots were in this sense an indispensible part of the net’s ability to function.

However, there were a number of downsides to this manner of netting construction as well. Firstly, because knots have a relatively large surface area compared to unknotted twine, they greatly increase the hydrodynamic drag acting on the net. Since any given fishing vessel has only a limited ability to overcome hydrodynamic drag (an ability that depends mainly on the power of its engines), the overall dimensions of a knotted trawl net must be kept relatively small in order for it to remain towable. A second weakness of knotted construction is literally that: weakness. Putting a knot or bend into a line dramatically reduces its breaking strength, and the strength of any particular knot continues to decrease as the angle of its loop(s) decreases. In other words, as the angle formed by a knotted line becomes sharper or more acute, that line becomes more and more likely to break

(Hameed and Boopendranath, 2000). By contrast, the breaking strength of the line *increases* as the number of loops in the knot increase. Accordingly, one straightforward way to improve the strength of the netting is simply to use a double sheet bend, which, as its name implies, adds an extra loop into the standard knot (Figure 3.2b). However, in doing so it also increases the surface area of each knot and—since there can be many thousands of such knots in an entire net—significantly worsens the overall hydrodynamic efficiency of the trawl. Furthermore, in bottom trawls the use of a physically larger and more prominent knot such as the double sheet bend increases the amount of abrasion suffered by the netting as it rubs against the

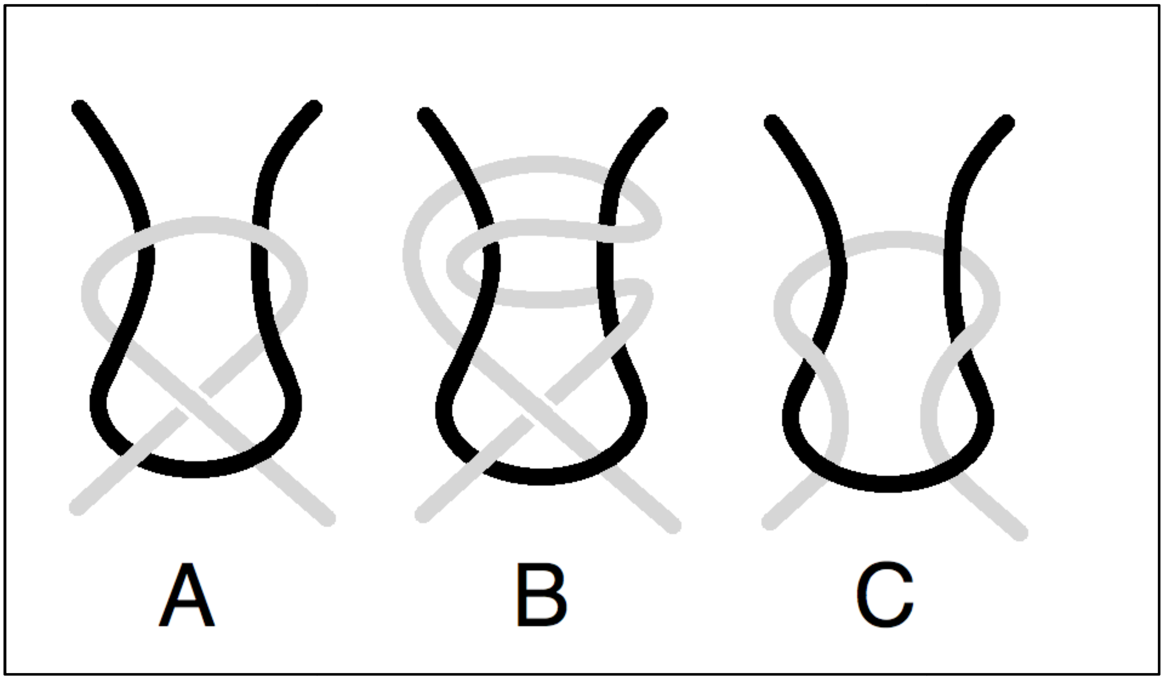


Figure 3.2 Common knots used in trawl netting. (A) the sheet bend, (B) the double sheet bend, and (C) the reef knot.

ocean floor, decreasing the net's effective lifespan. So while the use of a double sheet bend was productive in one way, it was clearly counter-productive in others.

An additional problem with sheet bends of either type was that over time they were prone to slippage under the tremendous physical loads that typify commercial trawling. In order to decrease this slippage, and prevent the resultant distortion of the overall net, the single or double sheet bend could be replaced with the reef knot, which was far less prone to such problems (Figure 3.2c). However, reef knots are also structurally weaker than sheet bends, so their use prevents slippage only at the cost of reducing the breaking strength of the netting. While this could be counteracted through a number of additional design modifications, all of them were suboptimal. For instance, the netting could be strengthened through the use of thicker twine, but this solution would increase the overall drag of the net. Or, in order to prevent the codend from rupturing under the strain of the catch, net manufacturers could wrap an entire second (and sometimes even a third) codend over the first. This solution, of course, increases the drag of the net as well. So no matter what knot was ultimately used, and no matter what ancillary methods were employed to ameliorate its negative effects, the underlying functional discord that characterized trawl netting remained unresolved. Breaking strength could be increased, but only if fuel efficiency was decreased. Slippage could be decreased, but only if breaking strength was reduced. While each knot could be seen as an improvement over the others in some way, the netting continued to work in a way that was fundamentally at odds with itself. Any of the knots could secure some of the netting's technical aims, but only by sacrificing its other aims. In this sense, knotted netting perpetually failed to achieve internal resonance.

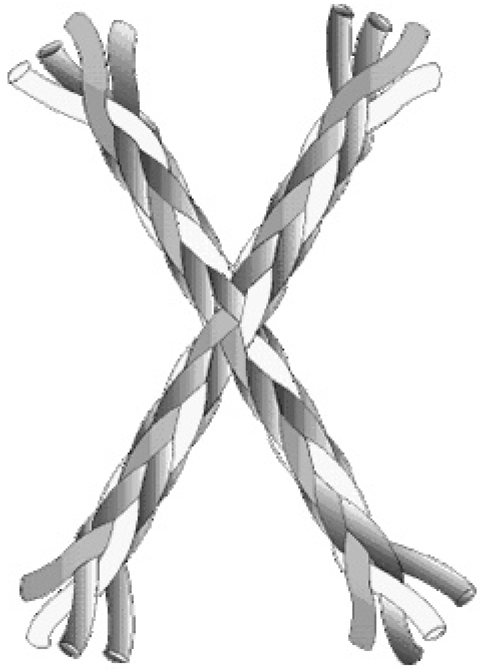


Figure 3.3 The knotless construction of Ultra Cross netting. (Image courtesy of NET-Systems.)

Ultra Cross netting (Figure 3.3) was developed to ameliorate all of these problems. It is a knotless netting in which the twines are seamlessly interwoven with each other to produce a continuous mesh in which the individual fibers are never forced to make a sharply angled loop. The construction of such netting is no simple matter. It required the development of a loom with a huge number of precisely programmed bobbins (more than 800 of them), arranged in an overlapping concentric pattern, that together are capable of weaving such an intricate design. Ultra Cross constitutes a concretizing improvement. In other words, it resolves underlying conflicts in the operative functioning of the technical object by spreading a particular sub-function out across numerous preexisting components (or, what is the same thing, by making each component partially responsible for a larger number of sub-functions). How? Firstly, Ultra Cross greatly increases the breaking strength of the netting by eliminating the sharp loops involved in the construction of any knot. Yet in doing so it also reduces the surface area and drag of the netting, increasing the fuel efficiency of its operation. Furthermore, the reduced profile of the netting means it does not easily abrade, even when it is dragged across the ocean floor. Finally, it is wholly impossible for knotless netting to slip, so there is no major distortion of the individual lumens or the overall net over time.[14]

Because it is stronger, more fuel efficient, and more stable than traditional knotted netting of any kind, Ultra Cross allows any given fishing vessel to use a larger net than it has in the past, at no additional fuel cost. Alternatively, it enables that vessel to deploy a standard sized net but increase the duration of any fishing trip, since the vessel conserves so much fuel with each tow. Either way, the result is a significant increase in the amount of water flowing through the net over time, and a corresponding increase in the amount catch that the vessel is able to bring back to the quay. Ultra Cross resolves the internal discord that characterized knotted netting of every type. By taking the task of lumen maintenance, which had been concentrated entirely within the structure of the knots, and spreading it out across the surface of the netting itself, Ultra Cross resolves all of the netting's previously conflicting requirements in a wholly positive way. It helps the netting achieve a high degree of internal resonance by eliminating the inherent imprecisions and self-limitations of knots.

The same pattern of concretization also characterizes other forms of progress in the fuel efficiency of trawl nets. This is true, for example, of the seemingly perpetual advances in netting materials themselves. In recent years a new material commonly known as Dyneema has become enormously popular in the construction of new fishing nets (it is also called Spectra: new synthetic polymers are typically labeled with a multitude of proprietary tradenames, which in terms of their underlying chemistry are interchangeable). To chemists, Dyneema is known as ultra high molecular

---

[14] Some initial distortion of a new trawl net is inevitable, depending on the inherent elasticity of the netting material used in its construction (see, for example, Sala et al., 2004).

weight polyethylene (UHMWPE). Each molecule of UHMWPE consists of an almost impossibly long chain of hydrocarbons. Whereas the molecular weight of a low density polyethylene molecule might be 90,000, that of UHMWPE is typically between 2 and 6 million.[15] Furthermore, its fibers are produced in a way (known as gel spinning) that allows over 95% of these massively elongated molecules to align themselves in a parallel direction and, accordingly, to pack themselves in very tightly. The strength of their intermolecular Van der Waals bonds is thereby maximized.

As a result, Dyneema is ferociously strong: a fiber only 1 mm in diameter can bear a load of 240 kilograms, making it a remarkable fifteen times stronger, per unit of weight, than steel, and 40% stronger than Kevlar-based (Aramid) fibers. Unlike these and other advanced fibers, Dyneema also has a specific gravity of less than 1.0, meaning that it floats in water (a useful trait for a fishing net). Highly flexible, it is nonetheless very resistant to mechanical fatigue and abrasion. Finally, because it absorbs virtually no water over time and is not easily damaged by sunlight, it does not require the application of any special protective coatings. Due to all of these properties (which we might say constitute its very high degree of technicity), Dyneema netting can be constructed with much lower-diameter twine than the equivalent polyethylene, polypropylene, or polyamide (nylon) netting. Because it is so thin, Dyneema netting exerts significantly less drag than these other materials. Its knots are smaller, but they are also much stronger.

For all of these reasons, the use of Dyneema greatly reduces the internal conflicts that so limit the effectiveness of traditional netting materials. When using conventional polymers such as polypropylene, the requisite level of breaking strength can only be assured by constructing the netting out of relatively thick twine and using knots that are strengthened by the inclusion of multiple loops. This, however, greatly increases the susceptibility of the net to abrasion and greatly reduces its hydrodynamic efficiency, meaning that it must conform to smaller overall dimensions. A

---

[15] A molecule's "molecular weight" is the ratio of its mass to one twelfth the mass of carbon-12. As the ratio of two masses it is a dimensionless number. So for example, the molecular weight of a carbon-12 atom is 12, the molecular weight of a hydrogen atom is 1, and so on.

polypropylene trawl net is internally dissonant in the sense that it cannot be made stronger without also becoming fuel-inefficient and/or smaller; it cannot be made more fuel efficient and/or larger without also becoming significantly weaker. Each technical goal can be achieved only at the expense of the other. Dyneema is a material that largely solves these contradictions. Like Ultra Cross knotless construction, it allows nets to simultaneously become stronger, more fuel efficient, and therefore larger. Like Ultra Cross, Dyneema permits an increase in the overall dimensions of the trawl net and/or allows fishing vessels to stay out for longer periods of time, thereby increasing the overall effectiveness of the net.

Given the wholly positive benefits of such concretizing advances, their win-win nature, it is not surprising that the use of Dyneema and Ultra Cross have rapidly become widespread in industrial fisheries around the world. Fishermen from the Arctic waters north of the Aleutian Islands to the subtropical coast of southern Mexico have quickly accepted both new technologies (NET Systems, 2009). In this sense there has been a fairly rapid global convergence of net designs toward what are clearly the optimal solutions for achieving high fuel efficiency. Indeed, fishermen have also been shown to readily adopt other advances in net design that unequivocally increase the fuel efficiency of their operations (e.g. Parente et al., 2008).

However, it is essential to note that none of these advances enable the trawl net to catch fish in a *qualitatively more effective* manner. Both Ultra Cross and Dyneema improve the net's overall catch levels by effectively increasing the total throughput of water and fish through the net. But the actual capture efficiency of such nets—the percentage of those fish they encounter that are ultimately caught—is not necessarily improved at all. Nor is the species or size composition of their catch. They do not, for instance, help a red snapper fisherman to catch relatively more market-sized red snappers and relatively fewer undesirable fish of other species or sizes. Indeed, larger nets might conceivably catch even more of these undersized or 'trash' fish than their smaller, less fuel-efficient predecessors. Accordingly, even though trawl nets made with Ultra Cross and/or Dyneema may be more technically advanced than earlier nets made out of more traditional polymers knotted together, they do not necessarily catch fish more effectively. Their advances are essentially quantitative in nature, leading to a greater amount of fishing per unit time, but not actually improving the composition of the catch.

It is helpful to think of this distinction in Simondonian terms. Trawl nets can be thought of as structures that perform an operative transduction, just like any other technical object. They bring together two disparate domains—the kinetic energy of free-swimming fish and the mechanical surface energy of the taut fishing net—and convert them, through specific exchanges of energy and information, into something novel: useable chemical energy in the form of immobilized, edible fish bodies. The crucial transduction at the heart of a trawl net, what Simondon would call its technical essence, involves the guidance of the desired fish bodies—i.e., those of a certain size and species—into the back of the net and their immobilization there. This transductive operation becomes more efficient and more precise, attaining a higher degree of technicity, insofar as undesirable sizes and species of fish are correctly guided out of the net. Yet the increased overall throughput of water and fish that results from the adoption of Ultra Cross and Dyneema does nothing to improve the precision or efficiency of this operative transduction. It neither causes a greater percentage of the desired fish to swim into the net, nor does it cause a greater percentage of unwanted animals to swim out of the net. It is in a sense transductively neutral, neither reducing not improving the net's technicity. For all their evidence of concretization, such advances are therefore an exceedingly minor form of progress.

Major forms of trawl net progress—qualitative advances in the process of capturing fish—arise instead on the other side of the mechanical-biological divide. They involve the development of more sophisticated methods to improve the net's size-selectivity (helping it to catch only market-sized fish) and species-selectivity (helping it to reduce unwanted bycatch). Such technical advances result in the improved separation of those fish whose capture is desirable, from those whose capture is undesirable. They make the transductive act of capturing fish more precise and more stable, thereby increasing the net's technicity. And yet here, on the other side of the mechanical-biological divide, the trajectory of technical progress looks nothing like Simondon's concretization.

3.8 Non-concretizing advances: a strange but major form of progress

If progress in the physical fuel efficiency of trawl nets has occurred through a sort of rapid convergence toward concretizing design features, in complete accordance with the pattern identified by Simondon in *The Mode of Existence*, progress in the *selectivity* of trawl nets more closely resembles a kind of wild and uncontrolled profusion of different designs. In the last thirty years, fishermen and net-manufacturers alike have invented and installed innumerable different *selectivity devices* or *bycatch reduction devices (BRDs)* into their nets in the pursuit of a qualitatively cleaner catch (i.e. catching only individuals above the minimum landing size of a specific target species). While the sheer number of such devices, combined with their generally limited documentation, obstructs any comprehensive effort to list them out, we can illustrate their great variety by describing at least some of the more popular devices currently being used and/or studied for commercial fishing:

> ***Square-mesh codend*** – This, as its name implies, is simply a codend in which the netting is turned 90 degrees from its normal, diamond-shaped orientation. The use of standard diamond-mesh netting reduces the selectivity of the codend, since diamond-shaped lumens quickly close themselves off when they are physically loaded in the longitudinal direction. In other words, the weight of the catch in the codend stretches the meshes until they become narrow slits through which unwanted fish cannot escape. Square-mesh netting does not constrict under load and so preserves openings of a consistent size through which undesired fish can pass.
>
> ***Lastridge ropes*** – Another method of holding open the codend meshes is to attach four relatively stiff lines, known as lastridge ropes, ideally about 5% shorter than the codend itself, to the sides of the codend, preventing their elongation under load.

Low codend joining ratio – Alternatively, mesh closure can be prevented by designing the seam at the front of the codend and the back of the extension to have a low joining ratio—in other words, to assure that the number of meshes in the circumference of the codend is roughly equal to the number of meshes in the circumference of the extension. When there is a high joining ratio—i.e. where a inordinately high number of meshes are packed into the circumference of the codend—the codend meshes are more easily closed off under strain, a strategy sometimes employed by fishermen to remain legally within minimum mesh size requirements while effectively circumventing the intent of those requirements (Graham et al., 2008; Catchpole and Revill, 2008).

Square-mesh panel – This is sewn into some part of the diamond-meshed body of the net, and serves essentially as un-closeable escape window through which fish can swim. Often located near the midpoint of the codend, it can also be inserted into any other part of the net.

Soft-panel BRD – This consists of a panel of very large-mesh netting that is stretched nearly vertically across inside of the net, thereby permitting only smaller organisms to pass into the codend. Larger organisms are directed by the netting panel toward an escape window in the top or bottom of the net, depending on which way the device is angled.

Rigid sorting grids – Numerous styles of hard grids can be inserted into the net just ahead of the codend. These typically consist of an oval-shaped metal hoop into which rigid vertical bars—like those of a jail window—are set. The grid is placed into the net at a swept-back angle of roughly 45-48 degrees (Hannah et l. 2003), such that any object larger than the spacing between the bars will be directed by water flow toward the grid's top or bottom. There it exits the net through a strategically placed escape hatch. Often the sorting grid is used in conjunction with a netting funnel, located just ahead of it, which ensures that fish will encounter the grid at the end furthest away from its escape window. This minimizes the number of small organisms that, instead of passing as intended through the bars of the grid, are inadvertently swept out though the exit window and lost.

JTED – If most sorting grids are meant to assure the escape of large fish, *juvenile and trash excluder devices (JTEDs)* are meant to assure the opposite: the escape of small fish (including unwanted species of diminutive “trash fish”) before reaching the codend. These are typically located in the top of the net ahead of the codend, and tend to consist of three hinged-togeth sections that protrude down, bight-like, into the narrow passageway of the net extension. The first (downward angled) and second (horizontal) panels are metal grids with very narrowly spaced bars. The third panel, which rises back up to the main wall of the net, is composed of a very fine mesh netting that is, for all practical purposes, impenetrable. Large fish hitting the front grid will be pushed down into the main, open passage beneath it and will continue on into the codend. Small fish, on the other hand, will pass between the bars of the first or second panel and escape upward.

Fisheye – This is a relatively small, forward-facing, elliptical hole in the net—not unlike a very short-necked periscope—through which fish can choose to swim. Typically constructed with steel or aluminum frames, one or more fisheyes are placed at any number of locations within the codend.

Radial escape section – This device, often known by the acronym RES, consists of a long mesh funnel in the narrow part of the net ahead of the codend. All fish must pass through this funnel. In the main wall of the net surrounding the body of the funnel are a number of large escape openings, often constructed of square-mesh netting, that are located around the entire circumference of the net. After passing through the funnel into the codend, larger and more powerful fish can swim forward towards these openings and escape.

Jones-Davis BRD – Like an RES, this employs a mesh funnel surrounded by escape windows. However, the exits are typically large holes cut out of the codend netting, and either a round float or a cone-shaped fish stimulator is placed immediately behind the funnel to deflect larger fish and promote their escape.

Fishbox – This is, as its name implies, a boxlike device that is attached to the top or bottom of the codend. Attached to a number of foils or plates, the fishbox is meant to alter the flow of water through the codend and create turbulence that encourages fish to move toward an escape hole.

Cut-away design – This design encourages the escape of strongly upward-swimming species of fish by altering the so-called headline that defines the top of the trawl mouth. In a cut-away net the headline—which typically overhangs the footrope of the trawl by a substantial margin in order to prevent upward escapes—is unusually long and is set back unusually far, such that fish with an upward trajectory will have sufficient time to move out of the path of the trawl mouth.

Horizontal separating panel – This consists of a horizontal panel of netting that runs from the main belly of the net, not far behind the trawl mouth, all the way to the codend(s). It separates the entire net into discrete top and bottom sections and may even end in separate top and bottom codends. Fish that typically rise inside of the trawl net will accumulate in the upper codend, while those that remain along the floor of the net will accumulate in the lower codend. Either codend can be left open in order to release undesired organisms.

Anti-herding device – This device, as imagined by Ryer (2008), would use a large fixed float, located at a certain distance in front of the trawl mouth, to discourage pelagic fish from entering the net without reducing the catch of demersal fish that approach the net along the ocean bottom.

Amidst the emergence so many devices—and many, many others (see Broadhurst, 2000; Eayrs, 2007)—there has been remarkably little agreement on *which* devices should be used and *what* exact design parameters should be used for any given type of device. Instead, for each new selectivity device that is introduced, fishermen and net-makers seem to quickly develop a plethora of closely related designs that employ slightly different panel dimensions, different mesh sizes, different materials, different numbers and arrangements of critical components, and/or are inserted at any number of different locations within the trawl. When it comes to selectivity devices, the field of trawl engineering is not so much a field as an overgrown thicket.

This sheer profusion of design variations, and the utter lack of convergence on certain particularly effective designs, can result in highly differing devices being used in exceedingly similar fisheries. For example, in Nordic bottom trawl fisheries there has been remarkably little agreement over whether sorting grids, escape windows, or codend mesh adjustments provide the most consistent means of improving selectivity, so all three are used with apparent abandon (Grimaldo et al., 2008). Eayrs (2007), advising prawn fishermen on the adoption and installation of selectivity devices in their trawl nets, must apologetically explain that, despite researchers' concerted efforts to develop new and improved selectivity devices, they have been unable to optimize or standardize the designs of any of them. In fact, one of the only consistent trends in the progress of trawl selectivity devices is this profusion itself, even within individual nets. In other words, trawl nets are being constructed with an increasing number of selectivity devices used in conjunction with one another, and this trend is likely to continue in the foreseeable future (Eayrs, 2007). It has become common practice to insert multiple selectivity devices into a single net: e.g., the use of square-mesh panels combined with a sorting grid, a modified codend, and/or ancillary devices such as fisheyes.

This proliferation of internal parts violates the Simondonian maxims of concretization in at last two ways. Firstly, there is the simple matter of numbers. Simondon clearly describes concretization as a process of design modification that improves the performance of a given technical object *without the addition of new technical structures*. Additional componentry, in his formulation, has a complicating and cluttering effect on the object's functioning. Concretization proceeds in quite the opposite manner: by splitting up the responsibility for a particular sub-function among a large number of already existing parts, thereby assuring that the sub-function does not become isolated within any small set of design features.

Indeed, Simondon makes this proscription against cluttering so clear that he must later return to it in order to add a sort of asterisk, acknowledging that the addition of an entirely new structure is sometimes necessary in the design of a technical object, but only if it helps to better identify and remedy a counter-productive tendency that was previously unrecognized within the object's operative functioning. So in trawl nets, the incorporation of an additional selectivity device may be an acceptable form of progress, to Simondon, only if it helps to perform a previously unrecognized task—say, encouraging the escape of Atlantic cod from a flatfish trawl. However, very few of the selectivity devices currently in use have been developed for the specific reduction of some newly prohibited species of bycatch. If anything, most devices seem to redundantly perform a single selective sub-function within the trawl net. For example, a single net might use a cut-away design, a horizontal separating panel, a square-mesh window, and a modified codend, all for the lone purpose of minimizing haddock bycatch. Each additional device, in such a case, is clearly not meant to deselect a different kind of fish whose capture has been found newly undesirable. This form of progress is puzzling, for in its proliferation of redundancies it is clearly something other than concretization.

A second counter-Simondonian feature of progress in trawl net selectivity concerns the so-called margin of indetermination. As we have already seen, concretization proceeds through a continual displacement of the technical object's indetermination to a specific and highly localized moment or margin in its operative functioning. Where trawl net selectivity is concerned, that flicker of indetermination occurs in the perpetually unresolved moment in which any fish suddenly encounters the selectivity device and is either selected (i.e. is caught) or deselected (i.e. is allowed to escape) by it. In early trawl nets, this crucial moment of indetermination was located in a single, specific place or moment within the functioning trawl net, just as Simondon would predict. Selection was designed to occur only in the extreme backmost portion of the net, the codend, where it could be modulated through off-season alterations of the netting's mesh size. Fish of the proper size and shape were selected in the final possible moment of the trawl net's functioning, when they had already passed through the entire trawl and had become concentrated with the rest of the catch in that codend.

However, as the technology of size and species selection has progressed, this margin of indetermination has seeped outward, becoming less and less localized within the trawl net. New inventions in selectivity have tended to quite literally be advances, proceeding in a forward direction from a single physical location at the very rear of the net toward ever more advanced positions within it. The indeterminate moment of selection, once marginalized in the meshes of the codend, has spread inexorably forward into the anterior portions of the net. Hence, it might now include square mesh windows in the middle part of the codend, a specific joining ratio at the front seam of the codend, one or more sorting grids at the rear of the extension, escape panels in the bottom of the trawl's belly, horizontal separator panels directly behind the trawl mouth, cut-away designs on the mouth itself, and even Ryer's (2008) proposed anti-herding device located far out in front of the mouth. This process of forward expansion has not been uniform, of course, and there are some exceptions to it. Fisheyes and fishboxes, for example, are fairly new inventions that are inserted into the codend rather than into the front of the trawl. Similarly, horizontal separator frames that are inserted into the extension (behind the main body of the net) have only recently begun to receive attention (Krag, Holst et al., 2009; Krag, Madsen, et al., 2009). However, despite these counter-examples, the overall trend of forward movement is clear.

So, too, are the reasons for it. The forward migration of selectivity devices has occurred as net-designers have begun to more fully understand the benefits of separating out unwanted fish as early as possible in the trawling process. Deselecting unwanted fish early in their passage through the trawl reduces their chances of being lethally injured in the crowded codend as they escape, while also preventing the codend from clogging with unwanted catch. The movement to insert selectivity devices in a more forward position therefore makes a good deal of sense. Yet insofar as it entails the growing diffusion and delocalization of the trawl net's margin of indetermination, it is also precisely the opposite of what we are supposed to see during the process of technical concretization.

3.9 Can *The Mode of Existence* explain this lack of concretization?

Given the prevalence of concretizing advances such as Ultra Cross netting in the purely physical aspects of trawl progress, this almost diametrically opposed trend in selectivity devices is puzzling. But perhaps Simondon offers a plausible explanation for this phenomenon. After all, in *The Mode of Existence* he identifies a number of reasons why any particular technical object might fail to achieve real, concretizing progress. Indeed, as I pointed out in the last chapter, much of the work reads like a technical *Reinheitsgebot,* a litany of technical objects that, for whatever reason, are unfit for progress and must therefore be thrown out of his analysis. Furthermore, there exist other aspects of his theory might conceivably serve as loopholes through which non-concretizing objects might simply be discarded, even if such sleights of hand were not necessarily anticipated by Simondon. Do the explanations or loopholes contained in *The Mode of Existence* apply to trawl nets? Are the nets simply unfit for progress? Do they fall on the wrong side of some carefully stated prerequisite for concretization? Or can they in some other way be disqualified from the analysis of technical progress? To answer these questions, let us consider each of the possible explanations or loopholes contained within *The Mode of Existence*.

One potential argument would be that Simondon's theory is meant to apply only to technical *individuals*—which are essentially machines—and not to the other two levels of technical reality that he identifies: the far simpler structures that he calls technical *elements*, and the far more complex webs of semi-autonomous, interconnected objects that he calls technical *ensembles*. This assertion, though disputable, is nonetheless lent some support by certain aspects of Simondon's text, such as its heavy reliance on machine-based examples when illustrating the notion of concretization. So perhaps the problem with trawl nets is that they are not machines or technical individuals. If not, though, then what are they? Technical elements? This would require trawl nets to be relatively small modular units that have very specific, relatively uncomplicated technicities and that can be combined in novel ways or even inserted into other, larger technical objects to produce an entirely new kind of object. Does this modularity and this transductive simplicity describe trawl nets? The answer is clearly no. Indeed, trawl nets are themselves composed of many smaller technical components such as panels of netting or even individual mesh lumens, each of which have their own distinctive design features, and all of which must be combined together to form an entire trawl net. If anything deserves the label of technical element, it is these combinable, constitutive panels or lumens of the netting, which serve as the actual conduits for the transductive process of fish capture. It is difficult to imagine a way in which complex, fully assembled trawl nets can themselves be conceived as technical elements.

Nor do trawl nets very closely resemble technical ensembles, which are interconnected webs of more or less individualized technical objects working together toward some common end. Perhaps the quickest litmus test for a technical ensemble is this: according to Simondon an ensemble cannot operate without the presence—or at least the continual attention—of human overseers at the interstices between its many constituent parts. This requirement is non-negotiable, since the technical ensemble is utterly incapable of regulating, coordinating, and modulating the collective interactions between its parts on its own. Trawl nets clearly do not require this sort of continual oversight and adjustment between their parts. Humans do not somehow modulate the interactions between the netting panels during the tow. The net is designed to function as an autonomous and already coordinated whole. If anything can be described as a technical ensemble in the act of trawling, it is rather the trawl system as a whole: the combined operation of the fishing vessel, its navigation systems, its underwater echosounders, its deck-mounted hydraulic winches and drums, the trawl warps, the trawl doors, the bridles, and the trawl net itself. This collection of more or less discrete objects (to which we could certainly add others) does require a human crew capable of coordinating its many parts and optimizing their collectively produced results. During the act of trawling, the speed and course of the vessel is carefully maintained and adjusted; the echosounder screen is continuously interpreted and heeded; the length of each warp is adjusted according to the water depth and currents; each door's angle of attack is adjusted before the tow if necessary; and the net as a whole can be made more or less buoyant through the attachment of floats or weights (see Sainsbury, 1996, for detailed descriptions of trawling from a crewmember's perspective). Trawling in this sense is certainly carried out by a technical ensemble, but that ensemble is of a different order than the net itself. In sum, then , the trawl net is neither an ensemble nor an element: it is something in between. Along Simondon's gradient of technical objects it certainly occupies the place of a technical individual, even if its major thrust of progress is conspicuous for its lack of concretization.

Perhaps, then, trawl nets fail to achieve concretization because they are stuck in a state of technical abstraction. After all, Simondon writes that objects are far from concrete at the early stages of their development, when they are still highly abstract. Objects that are abstract are piecemeal contraptions, not unlike Rube-Goldberg machines, that have been designed as logical chains of successive, more or less isolated, subsystems. Each subsystem performs its small portion of the object's overall function with complete independence, foregoing any help from the other subsystems. While such objects do make logical sense on paper, in practice they are often inefficient and prone to catastrophic failure, since the breakdown of a single component within a single subsystem will necessarily prevent the successful completion of the entire technical operation. However, it is clear that trawl nets are not arranged in such logical and fragile sequence of successive actions. They form highly integrated wholes, whose many panels and ropes work together to achieve and maintain a certain overall shape in the water. No part of the net works in a logically prior way to the other parts. No torn netting panel, on its own, can cause the immediate breakdown of the entire net. So trawl nets do not seem to suffer from a chronic state of abstraction.

Let us try a different tack. Maybe the issue is that trawl nets are less machine-like, and more tool-like. Surely, trawl nets could not be expected to concretize if they were merely some sort of enormous, idiosyncratic tool, marked by a profound simplicity of structure and function. However, this explanation also falls short. We have already seen that Simondon defines a tool, in *Mode of Existence*, as "the technical object that permits the prolongation and arming of the body to complete a gesture," and asserts that the 18th century was the golden age of tool development (Simondon 2007, p. 132). In this, it is clear that the term tool is, for Simondon, essentially interchangeable with the terms "artisanal object" and "craft object" (Simondon, 2009). These are objects that receive both their energy and their information from a human user, and are distinguished mainly from "industrial objects" that harness nature itself as a boundless source of energy.

According to Simondon's definition, then, trawl nets seem not to be tools, but industrial objects. After all, their enormous energetic demands are one of their defining features. Their operation was originally made possible only through the use of large sails to harness the immense natural power of the wind, while today the requisite propulsion can be provided only by extraordinarily powerful diesel engines—i.e. by the combustive properties of volatile, fractionally distilled hydrocarbon molecules. Trawl nets capture fish in very large part due to their relentless and overpowering forward motion, and this motion is not the extension of any conceivable human gesture. It is the result of harnessing the power of nature. However, even the label of industrial object is inadequate for trawl nets. For, as we shall see, the capture of fish in a trawl does not simply utilize nature as an energy source and humans as an information source. Fish capture is a process that also depends on the energy expenditure and information processing capacity of the fish themselves. In this sense, a trawl net harnesses the nonhuman world not only as its source of energy, but also as its source of information. There is nothing quite like this in Simondon's descriptions of technical function. Trawls are clearly not artisanal objects or tools. But nor are they purely industrial objects. They are, once again, something else.

A fourth potential explanation is that trawl nets are unable to concretize due to their made-to-measure quality. In other words, concretization does not occur because trawl nets are designed and constructed individually for each customer, according to the specifications of the fishing vessel, the intended target species, the probable fishing grounds, and cost limitations (Fridman, 1973; 1986). Simondon reserves what can only be described as contempt for such made-to-measure objects, equating them with inexpugnable technical abstraction, imprecise manual trade, and pre-industrial primitivism. Indeed, he insists that without industrial standardization and mass production, concretization cannot take place (Simondon 2007, pp. 45-6). "There is," he insists, "nothing essential about the made-to-measure aspect of the artisan's handicraft. … [The] made-to-measure technical object is, in fact, one which has no intrinsic measure; its norms are imposed from the outside; it still has not achieved its own internal coherence; it is not a system of the necessary; it corresponds to an open system of requirements" (ibid). Hence, the tendency of such objects to be weighted down with decorative, superficial, and entirely non-functional accessories and frills. On an automobile (the technical object that seems to repulse Simondon more than any other), design modifications such as oversized spoilers, undersized wheels, spinning rims, exaggerated air intake openings, and hydraulic 'bouncing' systems have nothing to do with the car's functional perfection. They do not enhance or improve its technicity. Rather, they are cluttering distractions from its technical essence and can only make the car perform more poorly than it was originally designed to.

To some extent, trawl nets do fit the pattern that Simondon describes. Trawls nets are certainly made to order, and their manufacturers make no qualms about catering to the whims of each specific customer. When a customer asks the net manufacturer to change the size of the meshes in the belly, or alter the hanging ratio of netting around the net mouth so that it balloons out more, or add a layer of frayed rope ends (chafers) on the bottom of the net to reduce abrasion, or put a second layer of codend netting around the first one, or use ordinary polyethylene in one part of the trawl instead of Dyneema, then the net manufacturer is typically more than happy to do so. But these changes are not purely decorative, they are not overly "charged with psychic and social implications"

(Simondon 2007, p. 48), they are not driven by the demands of status or public perception, and they are in fact never intended to be seen by anyone other than the fish. In no way do they constitute unnecessary and counter-productive modifications or accessories. Rather, they are requested because and only because the customer believes that they will improve the functional performance of the trawl. Even when based on whims, they are not whimsical. The idea that a trawl net's made-to-measure quality is an impediment to its functional progress is therefore off the mark.

Finally, it might be asked whether trawl nets have not simply been derailed from the path of concretization by some kind of pressure toward hypertelic over-adaptation. Hypertelic modifications, Simondon explains, may initially appear to be real improvements, but they fail the ultimate test of technical progress since they do not improve the object's technicity. In other words, they do not increase the precision, stability, and universality of its operative transduction. A concretized object of very high technicity is universal, in the sense that it will give the same, predictable result wherever and whenever it is used, and does not depend on any additional aid from its surroundings. It is resilient enough to flourish in any number of technical settings, and therefore has a great deal of autonomy. By contrast, a hypertelic object might well have undergone numerous, technically ingenious modifications, but its modifications are all focused on adapting it with stupendous precision to a prearranged technical environment. Without that precisely contrived technical environment, the hypertelic object is useless. It is therefore the opposite of universal. It has no autonomy, since it can only be successfully operated in a very specific and indeed singular technical setting.

As an example, Simondon points out that aeronautical cargo gliders (which were used extensively in the Second World War, before the invention of the helicopter) were utterly unable to perform their technical operation (remaining airborne) unless they were attached to a specific towing aircraft. The design of such gliders was therefore focused on achieving total adaptation to the capabilities of the towing aircraft. As a result, they were terribly maladapted to any other technical setting. Trawl nets might seem to be in the same situation as cargo gliders, since they are also towed devices that rely on the propulsion of a towing vehicle. However, trawl nets are never adapted exclusively to their towing vessel (Fridman, 1973; 1986). A net will function poorly if it is too large and too heavy for the vessel's engines to pull, or if it has been designed without any basic knowledge of the size and layout of the vessel's working deck. But trawl nets are also adapted to numerous other factors related to the bathymetry, oceanography, and ecology of the fishing grounds and the particular biology of the target species (ibid). For this reason, even very similar vessels participating in the same fishery may deploy fishing nets that are by design quite different from each other. Conversely, it would be difficult to argue that any given net is overly reliant on any one technical environment. Trawl nets certainly have enough functional independence to be frequently traded or sold between different fishing vessels, whose crews are far more likely to simply alter the rigging of the warps and bridles or adjust the angle of the trawl doors before making any changes to the net itself. So although trawl nets must be towed behind a fishing vessel, they are not simply overadapted technical extensions of that vessel, and their operative functioning is not wholly reliant on any one technical environment. No hypertelic tendency accounts for their failure to concretize.

Indeed, nothing in Simondon's *Mode of Existence* seems to explain the unusual nature of technical progress in trawl net design. Trawl nets are not technical elements. Nor are they technical ensembles. They are not abstract objects and they are not tools. Though made to measure, they are not frivolously accessorized. Finally, they are not hypertelic extensions of some other piece of technology, without which they would simply expire like some parasite removed from its host. What, then, accounts for the steady proliferation of selectivity devices—often wholly redundant ones—in almost every area of the trawl?

The analysis conducted in the first part of this chapter suggests a clear answer to this puzzle. We have already seen that for Simondon, a technical object's mode of progress is closely tied to its mode of operative functioning. In particular, concretizing progress is possible only when objects function through a process called mechanical transduction. Since all other explanations have failed to explain the puzzling nature of trawl progress, should we not consider the simplest option of all? Perhaps trawl nets fail to concretize precisely because they do not operate through mechanical transduction.[16] This solution should not strain the limits of our credulity. After all, the baffling tendencies of trawl net progress are not related to any of the strictly mechanical aspects of the trawl. These, as we have already seen, avail themselves to concretization like any other Simondonian object. Rather, the troublesome aspects of trawl progress are related only to those portions of the net that require the presence of fish in order to perform their operative functions. This quite clearly suggests that there is something about the presence of fish inside of the net—or rather, something about how the trawl net functions in relation to those fish—that keeps the process of concretization at bay. In the following chapter we will examine more closely what that something is. To do so we will analyze how it is that trawl nets actually work, and whether this occurs through a process of mechanical transduction.

Chapter 4 . Nonlinear Function: How do Trawl Nets Work?

---

[16] In my suggestion that mechanical transduction does not adequately describe all forms of technical function, this analysis represents something of departure from the Anglophone world's best-known works on Simondon's technical philosophy. These tend simply to accept and incorporate Simondon's fundamental notion of how technical objects function without questioning the basic validity of that notion. By doing so, such analyses free themselves to discuss larger issues of technical progress—for example, how we are to understand ourselves as human amid the quickening deluge of technical advances in the contemporary age. Stiegler (1998) and Mackenzie (2002), for instance, both use Simondonian thought to analyze technical progress in relation to humanity and temporality. Their accounts, while insightful and difficult to ignore, are of little practical use to this study, since both authors deploy notions of progress that rely for their coherence on the very mode of technical function that I am calling into question. And since specific modes of technical progress emerge from specific modes of technical function, we cannot destabilize our understanding of technical function without also disrupting or altering our approach to technical progress.

In the last chapter I showed that only the minor, mechanical aspects of trawl net progress are marked by concretization. By contrast, instances of major progress in trawl nets—those that increase the nets' transductive precision—show an entirely different trend marked both by the profusion of largely redundant technical elements and the expansion or diffusion of the margin of indetermination. I suggested that perhaps, since no convincing explanation for this kind of progress is given in *The Mode of Existence*, we might instead focus on Simondon's model of mechanical transduction. In other words, perhaps the selective aspects of a trawl net fail to undergo concretization because their operative functioning does not occur through Simondon's process of mechanical transduction.

Nowhere does Simondon indicate that other forms of operative functioning are possible for technical objects, but he nor does he state that such a thing is impossible. In this chapter I will both argue that another mode of operative functioning is possible for technical objects, and show why it has remained largely unstudied until now. The basic question that this chapter seeks to answer is this: if trawl nets do not function according to Simondon's model, then how do they work? Is there another convincing model of technical function that might explain the quirks of trawl net progress? How, exactly, would such a model depart from Simondon's, assuming that it is related to the latter at all?

4.1 The passive filtering model

To begin, I should clarify once again that it is perfectly possible to proffer a Simondonian (i.e., transductive) description of what a trawl net does. Following Simondon, we might say that a trawl net, just like any other technical object, operates through a unique process of transduction. It is a structure that brings together two disparate domains—the kinetic energy of free-swimming fish and the mechanical surface energy of the taut fishing net—and through specific exchanges of energy and information converts them into something novel: useable chemical energy in the form of immobilized, edible fish bodies. The capture process, we might say, actualizes the potential for arrested movement that exists between the netting material and the living fish. In this sense, the operative functioning of a trawl net does not differ in kind from that of any other technical object, such as a stapler or a splitting maul. The specific transduction(s) it performs are different, of course, but its overall mode of functioning fits neatly within the same Simondonian rubric.

For all its correctness, this description probably comes off as a convoluted way of explaining what seems like a fairly self-explanatory process. Indeed, to anyone with even a cursory knowledge of what a trawl net looks like and how it is deployed, the mechanism through which it carries out its transduction probably does not seem to require any explanation. After all, a trawl net is a conical or funnel-shaped bag of mesh that is dragged through the water in pursuit of fish. Fish overtaken by the trawl enter it through the wider, open end at the front, which is known as the trawl mouth. As the net moves steadily forward, those fish accumulate at the narrow back end of the cone, which is cinched shut and is known as the codend. Fish small enough to fit between the meshes of the codend can pass unscathed through the netting and escape. Those larger than the codend meshes cannot do so and are instead captured. Trawling simply involves towing the net for a certain period of time behind the fishing vessel and then reeling it back in so that its catch-filled codend can be hoisted aboard and emptied.

On its surface, this is not a terribly complicated operation. A plausible assumption would be that the trawl net functions as a giant underwater sieve, using its conical meshwork to physically filter fish larger than a certain size out of the surrounding seawater. Indeed, this is probably how fishermen and net-makers themselves understood trawl fishing for most of the last six hundred years—from 1376, when the first complaints were submitted to the British Parliament concerning the use of a "subtly contrived instrument called the wondyrchoum," whose description fits that of a small beam trawl (International Fisheries Exhibition 1884, p. 315), until 1952, when scuba divers were finally able to make the first underwater observations of a towed fishing net in action (He

2010). What those divers observed, as the net trundled past, was not a passive filtering process at all.

Fish were not simply swept up and entangled like pieces of flotsam suspended in the water column. Rather, during the tow each fish actively participated in a series of encounters with the moving net—soirees as unpredictable in their duration as in their outcome. A fish might initially react to the netting by swimming along unhurriedly next to it, languorously keeping pace inside of the net mouth for several minutes at a time. However, later the same fish might abruptly terminate such behavior, darting toward the inner wall of the net and wriggling out through the meshes; or the fish might suddenly drop back further into the net and then recommence its torpid swimming deep inside of it. Other fish might "kick and glide" in front of the net for a certain period, only to suddenly flip around and swim headfirst, with apparently suicidal abandon, directly back into the codend. These observations were confounding to fishing gear technologists. They revealed that even though towed fishing gear often worked, consistently capturing large numbers of fish, it did so in a manner that no one had suspected and that no one could quite explain. It was as though, at least in part, fish were responsible for catching themselves.

Over the sixty years since those first experiments were conducted, ever more detailed observations of trawl nets in action have confirmed the woeful inadequacy of the passive filtering model. Each time that a particular species of fish (or other non-sessile marine animal) has been subjected to intensive study in or around trawl nets, researchers have concluded that trawls "do not simply filter fish out of the sea passively" but rather function through a complex chain of interactions between the trawl and the fish (Beutal et al. 2008, p.191). This is true even of very small organisms whose mobility through the water is necessarily limited. For instance, although the various species of small shrimp (e.g. *Pandalus borealis, P. jordani*) targeted by commercial fishermen are often assumed to be "captured by a pure filtering process" (ICES 2008, p. 41), they have in fact evolved fairly sophisticated escape mechanisms (Arnott et al., 1999) that are employed with some degree of success to extricate themselves from trawl nets (Hannah et al., 2003).[17] Indeed, the only known exception to the rule against passive filtering of fish in trawl nets concerns two closely related species of monkfish (*Lophius* spp.) in the northeast Atlantic, which have been observed in a fairly limited number of video transmissions "to show no directed swimming or response to trawl presence or contact" (Chosid et al. 2008, p. 17; also see Reid et al. 2007). In other words, they do not appear to react even when the trawl collides with and then runs over them. This counterexample is notable mostly for its singularity. It is striking that no other species of fish, crustacean, or cephalopod has yet been observed to remain entirely passive during its encounter with a trawl.

4.2 Trawls as behavioral devices

If trawl nets are not simply giant filters or sieves, then how do they, in fact, work? How is their operative transduction carried out? There is probably no better answer than Ryer's (2008, p. 139): "Trawls," he states, "are inherently behavioral devices, harnessing innate avoidance behavior, which has evolved to mitigate predation, to facilitate fish capture." In other words, trawls are able to act as filters only insofar as they successfully manipulate the hardwired behavior of the fish they encounter. The evolution of trawl designs therefore hinges on—and is a direct consequence of—our slowly improving knowledge of what different fish do in certain situations, and why they do it (Graham et al., 2004). That knowledge is not perfect, or even close to perfect. But at least some aspects of fish behavior are now known with a high degree of certainty. For instance, it is clear that when confronted with an oncoming trawl, most fish engage in three main kinds of behavior: a herding response, an optomotor response, and a protean response (Kim and Wardle, 2008).

---

[17] The characterization of shrimp trawling as a "pure filtering process" is only correct in the sense that shrimp do not enter the trawl mouth by actively swimming into or away from it. Upon sensing the net's approach, shrimp located on the ocean floor typically attempt to avoid detection by remaining perfectly still. However, if the advance portions of the trawl make contact with them, or if they are caught swimming in front of the trawl, they may use a rapid series of tail flicks to elevate themselves by several meters into the water column. Following this evasive maneuver, they may remain suspended and motionless as the trawl mouth overtakes them. However, once inside the net they have been observed to make repeated escape attempts (Eayrs, 2007).

Successful trawling results from the careful and timely manipulation of all three behaviors. It will therefore be useful to examine them in closer detail.

Herding refers to the tendency of many fish, especially pelagic species, to move away from the approach of the trawl net in a way that concentrates them directly in front of the net mouth. Herding occurs because of the unique way in which most contemporary trawls, called otter trawls, are held open during the tow (Figure 4.1).[18] The mouth of an otter trawl is flanked on either side by the trawl *wings*, extensions of netting that stretch forward and outward from the trawl mouth in order to effectively increase its width. The tip of each wing is attached to two or three steel wires called *bridles*. These, which can be tens of meters long or more, connect the wings to a pair of large, vertically oriented shearing devices known as otterboards or trawl *doors*. The doors are essentially hydrofoils stood on end. They exert a spreading force outward as they run through the water and/or along the ocean bottom, and this spreading force is primarily what holds the net mouth open. Each trawl door, in turn, is attached directly to the fishing vessel by another, even longer steel wire known as the *warp*. Depending on the size of the net and the power of the fishing vessel, the trawl doors can be immense. Normally made of solid steel, each door can measure up to six meters (20 feet) tall, four and a half meters long (15 feet), and weigh 6,600 kilograms (14,550 lbs). Not surprisingly, the approach of two such massive objects through the water is easy to see and even easier to hear. Fish located in front of an advancing trawl tend to move perpendicularly away from the paths of the two oncoming doors. This avoidance reaction moves them either outward, entirely out of the path of the trawl, in which case they avoid capture, or slightly inward toward the eventual path of the trawl net, in which case they may still be caught.

---

[18] Beam trawls, it should be noted, are an earlier and currently less common type of trawl in which the mouth is held open by solid bar or beam that spans the entire top of the mouth and is attached on either side to a heavy steel “trawl head”.

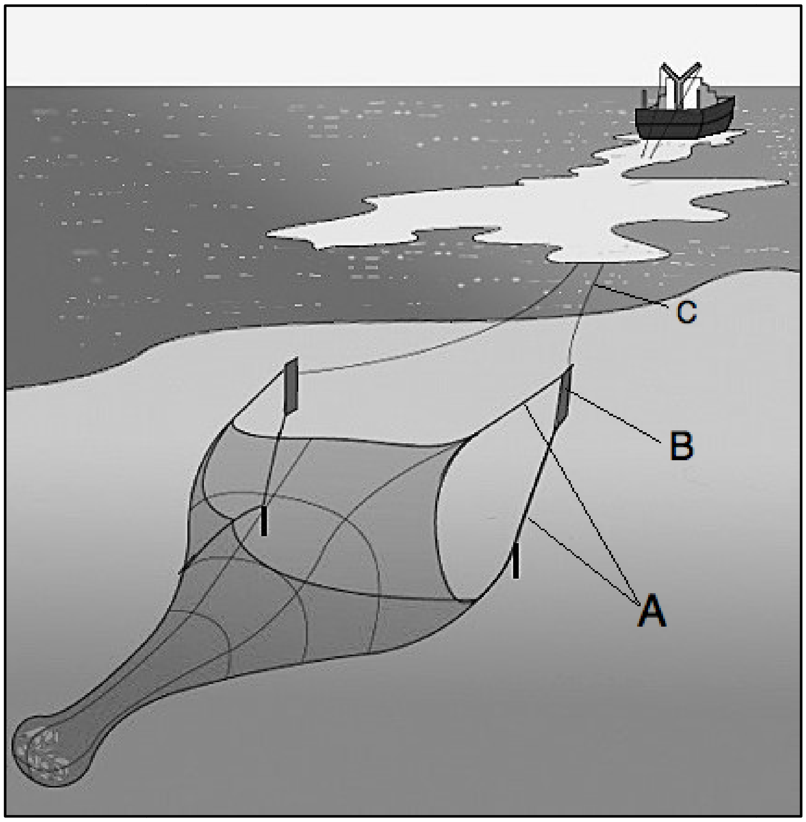


Figure 4.1 The primary rigging components of a trawl: (A) the bridles, (B) the trawl doors or otterboards, and (C) the warps. (Image courtesy of Greenpeace.org.)

However, it is unlikely that the fish's initial reaction to the trawl doors will bring it fully into the path of the net, since only the centermost one-third of the distance between the trawl doors is typically swept by the trawl net. Instead, after the trawl doors churn past, it is the steel-wire bridles that continue herding the fish inwards. While not especially visible underwater, the bridles vibrate intensely under the strain of the tow and therefore make a great deal of noise as they cut through the water. Fish avoid the source of this sound by moving ever inward as the bridles gradually converge toward the wings of net. Finally, the fish encounter those wings and are visually directed by the wing netting inward toward the opening of the trawl mouth.

It should be noted that the herding of fish does not occur through any kind of physical manipulation of their bodies. Neither the doors nor the bridles nor the wings of the trawl typically make contact with the fish (although contact with the bridle may sometimes occur when flatfish attempt to bury themselves in the sediment of the seabed). Rather, herding occurs when fish choose to move away from a perceived (but largely illusory) threat. Herding in pelagic (i.e. midwater) trawls is mostly a response to the ceaseless whir and hum of the bridles, simple steel wires that the fish could easily swim over, under, or between. Herding in demersal (bottom) trawls is mostly a response to the trailing plumes of sediment kicked up by the doors and bridles as they scrape along the ocean floor, clouds of mud that could easily be navigated despite their menacing appearance.

Nor is herding the result of a simple high-speed pursuit, in which the trawl is simply dragged too quickly for fish to escape. Indeed, when it comes to trawling, faster is seldom better: the effectiveness of trawls has been shown to decrease beyond a certain speed that is in every case less than the maximum speed of their quarry (Fridman, 1973; 1986; Itaya et al., 2007). The ideal speed of the trawl is fast enough to require some kind of continuous swimming effort from the fish, but slow enough to avoid stimulating any real bursts of speed. In any event, even a fish incapable of outswimming a trawl in the forward direction could presumably exit the path of the net quite simply by swimming upwards. That none of these possible escapes occurs among herded fish is a compelling testament to their behavioral basis.

Once they have been herded to the front of the trawl mouth, fish often switch over to an optomotor response. This is a tendency, exhibited by many fish species, to swim in a way that maintains a fixed position relative to their visually perceived surroundings (Harden Jones, 1963). In rivers this behavior helps fish to remain in one place relative to the bottom, rather than being swept downstream. In oceans and lakes, where trawls are used, it causes fish to swim closely alongside large moving objects or groups of objects at a steady speed. Trawl researchers often refer to the optomotor response as *station keeping* behavior, since fish exhibiting it tend to maintain a single apparently random station within the trawl net, swimming next to a particular area of netting for an extended period of time without falling back or attempting to surge ahead and escape. Although the two responses are sometimes conflated, station keeping clearly differs from the herding response, in which fish actively attempt to move away from impending contact with the gear (Bublitz, 1996).

Station keeping occurs in all parts of the net. It has been observed next to the wings, in front of the mouth, in the main body or belly of the trawl, in the narrow extension behind that, and even in the codend itself. Typically, however, station keeping occurs near the front of the net and persists until the fish becomes fatigued from the effort of continuously swimming, at which point it drops back into the codend and is effectively caught. But larger fish have been observed patiently swimming along in front of the accumulating ball of catch in the codend, seemingly refusing to consider other options even when they are located within inches of a large hole from which they might escape. Like herding, station keeping is a purely behavioral phenomenon. It keeps the fish inside of the net without any apparent physical means of doing so. The fish will generally not break its station keeping and escape from the net, even in those situations where it could quite easily fit through the meshes with which it is assiduously keeping pace. Because of this, trawl nets are often designed with very large meshes in the wings and, increasingly, in the entire front portion of their bodies. Large meshes create less drag and thereby increase the fuel efficiency of vessel, and they do so without losing any appreciable portion of the catch. Mesh sizes of 32 or 64 meters across are not unusual in the wings and bodies of pelagic trawls, and at least one well-known trawl utilizes meshes of 256 meters. At almost any point before reaching the codend, fish could easily escape from such a trawl if they attempted to do so. But this rarely occurs. The compulsion to swim alongside the steadily advancing netting material is stronger than any conflicting urge to escape.

Were fish-trawl interactions restricted to herding behavior and optomotor behavior, trawls would be spectacularly efficient. They would presumably funnel all fish in their path obligingly back into the net, and then keep them there in a state of motion-induced complacency until the net was hauled in, with almost no direct contact or physical manipulation of their bodies. But there is a third kind of behavior that continually threatens to disrupt and overwhelm these two: protean behavior. The term "protean behavior" was coined by Driver and Humphries (1970), who defined it as, "that behavior which is sufficiently unsystematic to prevent a reactor predicting in detail the position or actions of the actor" (p. 286). This definition is a very slightly modified version of Chance and Russell's (1959) earlier and somewhat narrower definition of "protean displays," which Driver and Humphries clearly seek to repeat almost verbatim (Hanlon and Messenger, 1996). Protean behavior is a defense mechanism employed by many prey animals, and it differs in a crucial way from other known defense mechanisms. Whereas defensive behaviors such as mimicry, crypsis (hiding), and biochemical release are repeated more or less identically each time they are employed by the prey animal, and can therefore be learned and anticipated by predators, protean behaviors "are resistant to learned countermeasures by the predator" (Driver and Humphries 1970, p. 298). They might therefore be expected to evolve where the predator is capable of learning from its hunting experiences and then altering its own hunting methods to better foil its prey. This is certainly the case with predatory fish, which have been shown to improve their ability to capture and handle prey with experience, and whose prey would therefore benefit from escape behaviors that are impossible to learn (Arnott et al., 1999).

Protean behavior can take a number of different forms, but in every case its defining feature is its sheer, genetically hardwired randomness (Miller, 2000). The most common form is probably the so-called single erratic, wherein the path of a prey animal evading capture "takes the form of highly erratic zigzagging, looping, spinning or bouncing" (Driver an Humphries 1970, p. 286). This behavior, characterized by a large number of sudden and unpredictable changes in speed and direction, emerges not as an initial response to some distant menace, but only as an escape mechanism of last resort, when the predator has gotten too near for its prey to escape using speed alone. It is a characteristic response of many marine organisms that are startled or threatened at close range. For example, squid react to immediately proximal threats by jetting away erratically—

though they also show other protean behaviors during their escape, such as blanching themselves light or dark, ejecting ink, and flashing through numerous random body patterns in succession (Hanlon and Messenger, 1996). Most fish also employ an erratic protean response when threatened at close range. Indeed, given the universal lack of hiding places in the generally transparent and three-dimensional aquatic environment, it would be energetically very wasteful for fish to flee from every potential predator that was detected at a distance. A far more conservative energetic strategy is essentially to wait and see, allowing potential predators to get close enough to make their own predatory move (if one is coming) before reacting. At this point, of course, an erratic protean escape is typically necessary, given the predator's proximity and higher swimming speed.

Video observations have shown that protean behavior of the erratic kind is commonplace among fish inside of trawl nets (Kim and Wardle, 2008). Many such fish will initially drift backwards or swim lazily alongside the netting, only to burst abruptly into a frenzied pattern of escape attempts. The recorded paths of these bursts exhibit, with an almost uncanny degree of perfection, the classic pattern of single erratic escape trajectories described by Driver and Humphries in their landmark article (Figure 4.2). And although the path illustrated in Figure 4.2e was recorded in the codend of a trawl, the onset of such behavior can potentially occur anywhere in the trawl, even in front of the mouth. As Eayrs (2007) acknowledges, some fish

> do not respond [to the oncoming net] by swimming with the trawl. Instead they will enter the trawl mouth either passively or with burst-speed swimming maneuvers in random directions. … Fish that are burst-speed swimming typically contact the trawl netting at high speed. Some become gilled in the netting and some may escape through the meshes. This may continue until they make their way into the codend. (Eayrs 2007, p. 67)

As fish get closer to the codend, the act of being crowded into an ever-narrowing tube of netting is likely to elicit protean responses even from previously tranquil fish. These, too, will swim at burst-speed in apparently random directions, colliding haphazardly with other fish and with the netting itself (ibid). Such behavior is not simply pointless or, even worse, potentially injurious. For if their haphazard approach brings them toward an open lumen at a high enough speed and a steep enough angle, the fish is likely to penetrate the netting and escape from the net (Kim and Wardle, 2008). If

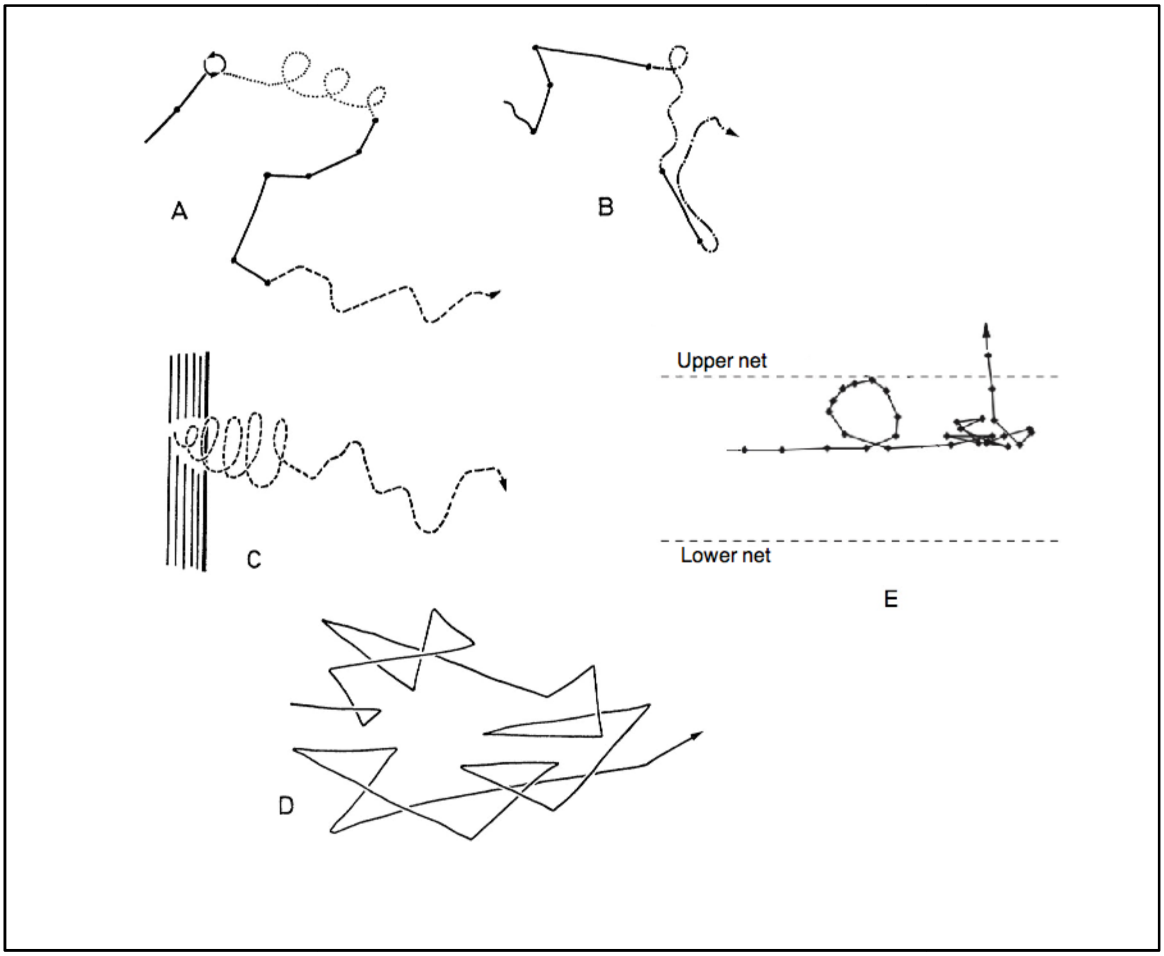


Figure 4.2 Protean responses, as manifested in the escape paths of different animals. Examples A–D are from Driver and Humphries' (1970) classic study, while example E (modified slightly from Kim et. al 2008) shows the recorded path of a fish escaping from the codend of a trawl net.

the approach angle or speed is wrong, fish may either give up and drift passively backwards if they are fatigued, or burst into another frenzy of movements if they are not.

A similar form of protean behavior called the scatter display is characteristic of many species of schooling fish. "Scatter displays," Driver and Humphries explain, "arise from the centrifugal fleeing of individuals from a tightly packed cluster … [in response to] sudden predatory attack" (Driver and Humphries 1970, p. 290). Scattering is the sudden outward explosion of the individuals forming a previously cohesive group. It serves as an effective defense mechanism because it arouses numerous "incompatible orienting responses" in the predator, which is often unable to single out any individual prey organism to pursue (ibid; also see Landeau and Terborgh, 1986). Of course, for the individual prey animal, the behavioral outcome of a scatter display is largely the same as a single erratic display. In both cases the animal launches quite suddenly into a chaotic series of high-speed escape trajectories. And just like erratic displays, scatter displays are often triggered within trawl nets:

> Many fish species can have their schooling behavior disrupted if they are herded or confined into a small area, such as the tapered section of netting immediately ahead of the codend. In this location fish may respond by suddenly 'exploding' in all directions…" (Eayrs 2007, p. 60)

Once this scatter response occurs and the fish begin to fling themselves toward the netting, their odds of escaping the trawl net are, of course, greatly improved. So whereas herding behavior and optomotor behavior both make fish capture more likely, protean behavior, both in its individual and its group forms, works in the opposite direction. It constantly threatens to disrupt and (for individual fish) even terminate the capture process. Indeed, insofar as it allows fish larger than the minimum landing size to escape through the wide meshes ahead of the codend, it can even be described as counter-functional.

The exact triggering mechanisms that elicit what trawl researchers call an "erratic response" (Kim and Wardle, 2003) or "panic response" (Wardle, 1993) from any given fish are extremely difficult to pin down. It has so far proven impossible to say exactly when, or why, an individual fish will abruptly shift its mode of interaction with the net from an optomotor response to a protean response. Although this switch typically occurs when the fish is relatively close to the net, rather than far out in front of it, close examinations have shown that there is in fact no clear relationship between a fish's behavioral response and the distance separating it from the fishing gear (Kim and Wardle, 2003). Distance seems to be at best a contributing factor. Many instances of panic behavior—though not all of them—seem to stem not from proximity, per se, but from sudden, unexpected movements of the netting material (jerking, shaking, undulating) during the tow (Kim and Wardle, 2008; Hannah et al., 2003). As Queirolo et al. (2009) explain, "it is necessary to design fishing gear that generates little or no fluttering of the mesh as [this] may elicit unwanted behavioral response in fish," thereby compromising the efficiency and/or selectivity of the net (p. 93—citing Engås et al., 1999). Trawl nets, in this sense, function best when they achieve a very high degree of stability underwater. This staves off for as long as possible the impending onset of protean behavior in the targeted fish.

This sort of perpetual mollification, this tenuous deferment of the inevitable, is only possible when fish *perceive* the net as something that is not overtly threatening, alarming, or capricious. Indeed, it is only through careful management of the fish's perceptions that the trawl net is able to elicit the desired herding and optomotor responses from the animal instead of causing it to erupt into a panicked frenzy. In the following section I will examine the ways in which the constitutive actions and information of fish perception differ from those laid out in Simondon's model of mechanical transduction. My argument will be that the technicity of a functioning trawl net, the effectiveness of its operative transduction, depends on a peculiar form of combined action/information known as an *affordance*.

4.3 Affordances and Technical Objects

Herding and station-keeping, as we have seen, are productive and predictable. They lead a fish to fatigue and eventual surrender. Panic, on the other hand, is counter-productive and unpredictable. It greatly increases the odds that a fish will escape and/or become injured, even if its inherent randomness means that such an outcome cannot be guaranteed. Insofar as a trawl net catches its quarry by balancing and coordinating these three different behaviors, its operative functioning clearly involves a process of informational and energetic exchange—just as the objects in Simondon's *Mode of Existence* do. But in trawl nets the transductive process is highly unusual, for it is the fish themselves, rather than Simondon's human observers, which gather and process information about the ongoing technical operation. And it is again the fish that decide how to respond to that information by directing their energy toward certain swimming behaviors. The operative functioning of trawl nets is fundamentally behavioral and perceptual; it is inseparable, in other words, from the fishes' biological modes of monitoring and then reacting to their environment.

Accordingly, trawl nets do not and indeed cannot function according to Simondon's theory of mechanical transduction, in which living beings are necessarily kept outside of the object's operative transduction as external observers and coordinators. Indeed, from a Simondonian perspective trawl nets appear to be turned essentially inside out, each one comprising a relatively thin veneer of mechanical elements (mostly sheets of netting) wrapped around a living, observing, freely acting, behavioral core. Peeling away the layers of trawl nets, honing in on what Simondon would call their technical essence, we find not some narrow and cleverly pre-orchestrated mechanical exchange, but the perpetual openness of animal bodies and minds navigating freely through their surroundings. A trawl net's technical essence is not mechanical but wild and sensory and unsettled. It functions through the informational and energetic exchanges that characterize animal perception and behavior. These exchanges fit poorly, if at all, into Simondon's notion of mechanical transduction.

Rather, they have received probably their best treatment in James Gibson's (1986) widely regarded work on animal perception and behavior. It is to this work that we now turn.

The main novelty of Gibson's approach to perception (an approach he called "ecological") was its utter rejection of behavioral scientists' traditional division between the perceiving subject and the perceived object. This conventional view held that perception was a process based primarily on the transmission of sensory stimuli. In other words, that perception was possible because the environment emitted discrete sensory signals (wavelengths of light, etc.) that could be received by the appropriate sensory apparatus of an observer, transmitted along neural pathways into his or her brain, unscrambled, and then mentally reassembled into a realistic picture of the world. Stimuli were tiny, encrypted messages sent out by a pre-existing and fully formed physical world. They alone were able to cross the gulf separating the physical world from the mind of the observer, allowing him or her to mentally reconstruct that world. Gibson recognized that to rely on the notion of sensory stimuli, when explaining perception, was tacitly to argue for the existence of this division through the middle of the world; the absolute separation of each observer from his or her surroundings. If such a rift existed, however, he could certainly find no evidence for it. Gibson therefore argued that animals do not perceive some kind of preexisting, valueless environment that is immaculately shorn from their own valuating minds and bodies. They do not perceive a starkly objective world that is separate or even separable from their subjective selves. Instead, animals perceive their environment in terms of what it specifically furnishes or *affords* them. A patch of particularly muddy ocean floor, for instance, might afford a flatfish such as a flounder a good place to bury itself and hide from predators. A wildly flapping fishing net might afford clear predatory danger to a haddock swimming in front of its mouth. An isolated drift line of flotsam in the open ocean might afford good foraging possibilities to a juvenile bigeye tuna. Gibson coined the term *affordances* to describe these perceivable furnishings of the environment:

> The *affordances* of the environment are what it *offers* the animal, what it *provides* or *furnishes*, either for good or ill. … I mean by it something that refers to both the environment and the animal in a way that no existing term does. It implies the complementarity of the animal and the environment." (Gibson 1986, p. 127)

Affordances can be thought of essentially as the *action possibilities* that the individual animal perceives in its surroundings (or, according to Gibson, the action possibilities that it *should* perceive, even if it does not). In other words, the animal perceives the environment not as an inert background for subjective action, but rather as a wholly constitutive part of its own possible actions.

Because each action possibility is a property neither of the animal itself, nor of the environment itself, but of both of them in relation to each other, affordances are not the same for different animals observing a single environment. Gibson emphasizes that they can only "be measured *relative to the animal*" (ibid). A very long, skinny branch near the top of a tall tree affords a good hunting perch for an osprey, but the same thin branch affords only falling and certain injury to a full grown black bear climbing up the tree trunk. Gibson explains that for any animal perceiving its environment, there is no such thing as information that is solely about its surroundings—what he refers to as exterospecific information. Rather, all perceptual information is always exterospecific and propriospecific at once: it is about the animal's surroundings *and* about its own body at the same time (Gibson 1986, p. 75). To gain information about one's surroundings is therefore always to gain information about one's potential corporeal actions amid those surroundings.

It is important to reemphasize that affordant information is not packaged in the form of stimuli. It is not based on the emission of individual signals or commands that can be received and then decoded by the brain. As Gibson writes, "The information for perception is not transmitted, does not consist of signals, and does not entail a sender and a receiver" (p. 63). Rather, ecological information is simply out there in the world, passively available to the observer. It can be actively gathered but it is not aimed at a recipient. Accordingly, the informing moment itself is always uncertain. As Gibson writes, “perception may or may not occur in the presence of information” (p. 57). In this, the environment surrounding any animal resembles a painting or photograph on display in a museum: it certainly contains a potential wealth of visual information, depending on how carefully one chooses to look at it, but it “does not consist of signs or signals and is not so obviously a message from one person to another” (p. 63). This constitutes a crucial point of departure from Simondon’s theory of technical function. In that theory, the functioning of the technical object is modulated by the submission of purposeful signals to its mechanical transducer by an attentive human operator. The fish clearly plays the role of transducer in a trawl net, taking in information and then adjusting its actions accordingly, but it is a transducer quite unlike the ones we have already examined. Trawl nets operate through the affordances gathered by living, breathing, swimming transducers.

Simondon was hardly ignorant of the fact that living creatures can act as transducers. Indeed, in *The Mode of Existence* he specifically attempts to differentiate the transductive capacities of living bodies from those of the mechanical transducers that form the heart of his technical philosophy. As he acknowledges, “The elemental living thing, the animal, is in itself a transducer when it holds chemical energies in reserve and then actualizes them in the course of the different vital operations” (Simondon 2007, p. 160). In other words, the very biochemical processes that sustain life on a cellular and molecular level are transductive. However, Simondon emphasizes that this kind of biological transduction should in no way be confused with the processes that are carried out in a technical object’s mechanical transducer:

> In effect, the living thing is not exactly a transducer like those that machines can have; it is that and something more; mechanical transducers are systems that have a margin of indetermination; information is what provides determination. But [in the technical object] it is necessary that this information be supplied to the transducer; the latter does not invent it; it is given it by an analogous mechanism to perception

> in the living being, for example, by a signal that comes from the way in which the effectuator functions … By contrast, the living thing has the capability of providing itself a piece of information… (Simondon 2007, pp. 160-1)

In this passage, Simondon notes quite explicitly that mechanical transducers require the directed input of information through signals, while living transducers utilize their powers of perception to actively gather information on their own. This distinction clearly anticipates my own argument about the need to consider perceptual processes (*i.e.*, affordances) when dealing with the transductive capacities of living animals. However, Simondon's simultaneous assertion that the reception of signals by a technical object occurs through a mechanism analogous to animal perception is valid only in the loosest sense. As we shall see, animal perception is in fact an informing mechanism so incomparable to mechanical signaling that its existence within the technical object will effectively undermine Simondon's entire theory of technical evolution. If Simondon himself did not recognize or expound upon this incongruity, it is most likely because he considered animals to be transducers only *in themselves*, as biologically functioning individuals. He quite clearly did not anticipate the possibility of direct animal involvement in an object's operative functioning; did not consider that an animal might not merely comprise a transducer in and for itself, but also act as the transducer for a certain kind of technical object; did not, in sum, envisage the development of complex behavioral devices like trawl nets.

Simondon is not alone in his apparent oblivion toward behavioral devices. Indeed, even those who have recognized the important role that affordances might play in the operation of technical objects have typically failed to make any explicit distinction between behavioral and mechanical devices. This omission is unfortunate, for it has meant that discussions linking technical objects to affordance have remained firmly centered on questions of usability, rather than questions of operative function. In other words, the notion of affordances has been taken up to account for the way in which human users employ technical objects, but not to address the way in which those objects actually perform their operative functioning. For example, in his study of the more-than-human driving assemblage that he dubs the "driver-car," Dant (2004) considers the role that affordances play in a person's skillful handling of an automobile. But he does not, of course, use the notion of affordances to explain how the components of the car—the gears and pistons and drive shafts—actually perform their operative transductions. So, novel though Dant's formulation of the driver-car assemblage is, it remains within the Simondonian rubric of technical objects that function through mechanical transduction. In it, perception and behavior are considered to be necessary aspects of using a technical object, but they have no place within the innermost transductive process that constitutes its technical essence. In such accounts, affordance and technicity do not mix. Affordance is related not to technicity but to usability. This is not entirely surprising, given that the seminal work on affordance and technical objects—Don Norman's (1988) eminently readable *The Psychology of Everyday Things*—was written by a designer about the nuances of successful product design. Such usability-based analyses are not wrong by any means, but neither do they address what is so unusual and compelling about trawl nets: the fact that their operative functioning itself occurs through affordant exchanges of information and energy; the fact that they are behavioral devices.

Perhaps no one comes closer to an analysis of behavioral devices than Latour (2002), even if he seems to do so unwittingly. Writing on technology and morality, he argues that because technical objects are always encountered affordantly, they necessarily enhance rather than restrict our freedom. By this, Latour explains, he means that each time we heft a hammer in our hand or turn the ignition of a car, our subsequent actions do not suddenly become narrowly choreographed. Instead, through this encounter a whole new array of action possibilities opens up for us. We suddenly become an entirely new person, or a whole array of new potential people. We can suddenly do or be any number of things that we could not do or be a moment ago. Latour therefore proclaims that, "all technologies incite around them [a] whirlwind of new worlds" (Latour 2002, p. 250). Technical objects, he insists, are perpetual openings into multiple futures. Though written with characteristic wit and charm, Latour's analysis of affordances does not, up to this point, differ appreciably from that of other authors such as Hutchby (2001), Dant (2004), and Bloomfield et al. (2010). That is to say: Like them, Latour deploys the notion of affordances to describe how humans *use* or *operate* technical objects, rather than to explain the operative functioning of their technical components. He maintains the separation of technicity and affordance, remains focused on objects whose transductions are Simondonian and mechanical. His next move, however, very nearly changes this.

In order to illustrate what he means by the affordant openness of technical objects, Latour recycles one of his favorite and most heavily called-upon examples: the technical object known as a speed bump or, in France, a "sleeping policeman". This, he admits, is an object with which his more devoted readers will by now be quite familiar. Yet it is precisely here, taking a familiar path along which he has moved so many times before, that Latour inadvertently strays into new territory. In a regrettably brief passage, Latour argues that when a speed bump is first installed in some village, it does not initially do its job of calming down the traffic. In fact it accomplishes just the opposite, inciting a kind of temporary chaos, full of misunderstandings and disputes and (we might presume) honking horns and swerving cars and even sudden accelerations. The reason for this, he suggests, is that a speed bump does not present drivers with any single clear chain of actions to follow. Rather, it is a device that inspires any number of different reactions in different drivers. It affords many things, opens many action possibilities, at once. The calming effect of the speed bump only emerges over time, as the correct action is learned; in other words, once word has spread through the village that an approaching speed bump affords negative things like a broken axle or a bump on the top of one's head if it is taken at high speed.[19] Until such learning takes place, the operative functioning of the speed bump is highly unpredictable. Its brightly painted prominence above the roadway can be read in any number of ways or, depending on the driver's attentiveness and eyesight, not read at all. There is, at least initially, no way of knowing how the speed bump will be perceived by each driver, nor how its perception will modify their behavior behind the wheel.

---

[19] This emphasis on learned affordance is not new. Among others, Hutchby (2001), Dant (2004), and Bloomfield (2010) have all argued that the perception of affordances often involves social learning and other forms of historical contingency that are missing from Gibson's original formulation.

What Latour has chosen to describe, of course, is not a mechanical device but a behavioral one. A speed bump is not *used* by the drivers that encounter it. They do not *operate* the speed bump in order to achieve a desired technical outcome. Rather, drivers are in essence both the device's raw material and part of its componentry. Their affordances and behaviors are not simply a means of coordinating and modulating the speed bump's operative functioning, but actually *constitute* that functioning. The speed bump works by making available certain perceptual cues that, if picked up, will modify the behavior of (most) drivers in a desired manner—just as a trawl net works only insofar as it offers the correct perceptual cues to inspire particular behavioral movements from the fish. Once either device has altered the observer's behavior, its work is essentially complete.

Latour does not make this switch to behavioral devices consciously. Indeed, nowhere in the article does he differentiate behavioral from non-behavioral technologies. Yet his point is important, even if it was unintended: behavioral devices function in a way that is particularly difficult to predict. Their operative transductions rely on animal perceptions, and as we have already seen, what animals perceive in the world around them are affordances or multiple action possibilities. The primary difficulty of such a device—especially when the animals in question are mostly incapable of learning the correct behavior—is exactly what Latour first pointed out. Each time the animal-object encounter takes place, the former becomes a whole new animal, or a whole array of potential animals. Whereas most technical objects (think of the stapler) perform an incredibly refined transduction, honing in on a very precise arrangement of technicities that will optimize and stabilize their overall functioning, behavioral devices operate through a perpetually branching profusion of possible transductions that is difficult to contain or stabilize. It could be said that the former kind of transductions work because they have subtracted away all possible outcomes but one. Behavioral transductions, on the other hand, work even as they unavoidably multiply the possible outcomes of the technical operation.

Because there has been little research among social scientists into the peculiarities of behavioral devices, and probably also because the work of Simondon has only recently begun to receive sustained attention from Anglophone readers (De Boever et al., 2009), the question of whether a technical object might utilize a living transducer instead of a mechanical one, and function through a regime of biological affordances rather than purely physical or chemical signals, has yet to be examined. Nor have the potential consequences of such affordance-based function for the design and evolution of technical objects been studied. In the following section I will therefore reexamine the issue of selectivity devices in trawl nets. We shall see that the necessary admission of Gibsonian affordance into their technical functioning has a number of specific consequences, and that these consequences prevent trawl nets from functioning in a linear manner.

4.4 The non-linear functioning of trawl selectivity devices

In the previous chapter I explained that Simondonian technical function requires, and indeed revolves around, the action of a mechanical transducer. To review, this transducer divides the technical operation along two axes, cleanly separating (1) its causes from its effects and (2) its energetic pathways from its informational channels. It is this twice-divided structure of the mechanical transducer that safeguards and enhances the causal linearity of the technical object's operative functioning, assuring that the object can be employed repeatedly without producing unforeseen results. In this section, I will show how the notion of affordant technicity disrupts both of these necessary divisions. In other words, I will illustrate how the functional basis of trawling in animal affordances makes it impossible to achieve that separation of action from information, or of causes from effects. Without these separations, as we shall see, trawl nets cannot and do not function through reliably linear mechanisms. Rather, they are an exceedingly rare example of what we might call functionally *nonlinear* technical objects.

In order to make this point, let us return to trawl net selectivity devices, whose untamed rush of progress, described in Chapter 3, seemed to confound all of our Simondonian expectations.

There is a great deal that we could say about the intricacies of the operative functioning of such devices. The transduction that they bring about—compelling certain sizes, ages, and/or species of fish and crustacean to swim freely out of the net instead of continuing into the back of the codend—may appear simple, but is in fact deceivingly complex. For the sake of clarity, I will therefore restrict my discussion of it to the minimum that is necessary. My goal is not to provide an exhaustive or blow-by-blow description of everything that occurs during the operation of every type of selectivity device. Rather, drawing from a large body of recent fisheries literature, I simply wish to identify four aspects of all selectivity devices that illustrate their irreparable deviance from the model of mechanical transduction. These four observed traits are also four powerful arguments against their ability to function through linear causal mechanisms. We might refer to them as (1) nondiscretion, (2) propriospecificity, (3) catalysis, and (4) indetermination.

(1) Nondiscretion. As we have seen, Simondon's notion of mechanical transduction requires the transducer to act as a stabilizing gap or separation between the technical operation's causes and effects. The technical operation itself is a discrete act constituted by the momentary bridging of that gap. The rift and its crossing are necessary, since they are what enable the human observer to judge the effectiveness with which the causes of the technical operation produced their effects. They allow the technical operation to be interpreted as a matter of inputs (*x+y*) and outputs (*=z*). Yet in his theory of affordances Gibson argues that perceptual acts of information pickup do not work in this manner. They are not discrete events that can be marked off by some definable passage of physical time. Rather, perception occurs through what he calls 'ecological events' and these are always non-discrete: they have no clear beginning and no clear end. Because perception is a continuous act, every perceived event tends to emerge seamlessly from previous events, and then to blend smoothly into other subsequent ones with no rigorously definable before or after. "What we take to be a unitary episode is therefore a matter of choice and depends on the beginning and the end that are appropriate, not on the units of measurement" (Gibson 1986, p. 101). To define any ecological event as a discrete act is a wholly arbitrary exercise, since ecological events involve the gradual and continuous pickup of information by a moving observer.

Because trawl net selectivity devices are perceptual and behavioral devices, they necessarily function through such ecological events. We might therefore expect their moment of functioning to be nondiscrete. Indeed, this is the case. As researchers examine the selective capabilities of trawl nets ever more closely, it becomes increasingly difficult to say when the act of selection actually occurs. The original presumption—and the one on which current measures of selectivity are still based—holds that fish are selected during the finite period of time when they are inside the codend of an actively towed trawl net. However, a growing body of evidence refutes this assumption. On one hand, it has become clear that fish begin to perceive and respond to the approaching trawl long before it actually reaches them and initiates their herding behavior (Handegard and Tjøstheim, 2005). Underwater acoustic observations have shown that fish begin to dive into deeper water during the period of relative quiet that characterizes the pre-trawling preparations onboard the fishing vessel. The fishes' diving behavior begins up to fifteen minutes before the vessel passes over them and roughly twelve minutes before the approach of the fishing vessel even becomes audible. It has been speculated that the early diving reaction may be a response to the sudden noise of the heavy trawl doors landing on the sea floor, but the accuracy of this suggestion remains unproven. What is clear, however, is that by diving into or out of the eventual path of the trawl net, fish initiate the process of selection well before they even see the trawl doors or hear the noise of the vessel's engines and hull. "It is evident," Handegard and Tjøstheim write, "that the handling of the boat when setting the trawl affects fish behavior," and this behavioral modification is what initially determines whether fish will or will not be selected for capture (Handegard and Tjøstheim 2005, p. 2420).

The thought of fish reacting to the ominous thud of the trawl doors hitting bottom would seem to suggest that self-selective fish behavior is a reaction to discrete, momentary stimuli (in this case, a sudden, faraway clunk). But this is not the case. In the study already cited, Handegard and

Tjøstheim (2005) found that fish react more strongly to the noise of the trawl warps than to any other sound-emitting component of the trawling system. This conclusion was in itself unsurprising, since trawl warps emit a distinctive sound in the particular range of low frequencies (peaking at 7 and 14 Hz) that has been shown to incite the most alarm among some species of fish (Enger et al., 1993). What was far less expected was the researchers' utter inability to detect that noise using their own highly sensitive listening devices. Listen as they might, the characteristic thrum of the trawl warps was impossible to distinguish from "the ambient noise due to turbulence in deep ocean currents [which] is high at these [same] frequencies" (Handegard and Tjøstheim 2005, p. 2420). The only apparent explanation was that fish were reacting not to the emitted sound itself, but rather to the more subtle ongoing *changes* in that sound. In other words, fish were attuned not to specific moments of discrete stimuli, but to relatively gradual changes in their overall auditory environment. The researchers' acoustic measurements were not resolved in time, and so it remained unclear what those changes might be. This description, of course, dovetails quite neatly with Gibson's notion of nondiscrete ecological events with no perceivable start and no perceivable finish.

If the onset of selective behavior in a trawl net is difficult to pinpoint, so is its conclusion. Many species of fish, as it turns out, make their escape after the actual tow is finished, when the trawl net's selective work is supposed to already be complete. This post-tow selection takes place when the net is being hauled back toward the fishing vessel by hydraulic winches mounted on its deck. As the net is pulled in it typically pulses forward and backward, a lurching movement that creates moments of slack in the netting of the codend and permits some of the catch to escape. The winching of the trawl warps stops when the trawl doors reach the vessel, at which point they must be removed from the rigging and affixed to the vessel (typically hung on the gallows over its stern) so that the bridles and the forward part of the net can be hauled in (Sainsbury, 1996). During this interval the trawl net is necessarily left floating, slackly, at the surface of the water for a period of time, providing yet another opportunity for fish that were not initially selected by the trawl net to escape from the codend. The exact length of this second escape opportunity depends largely on the type of fishing vessel that is used, since different vessels necessarily employ different methods of haulback. Side trawlers leave the net in a slack position for the longest period of time; stern trawlers leave it slack only at the end of haul-back, when the net is being tackled and hauled aboard; and stern trawlers with ramps do not ever need to leave the net in a slack position at the ocean surface (Madsen et al., 2008).

A number of recent studies have shown that the magnitude of post-tow selection reaches unexpectedly significant levels. Based on qualitative underwater observations, Grimaldo et al. (2008) found that very few undersized fish at all managed—or even tried—to escape from the codend meshes during the tow itself. Most selection occurred, instead, as the net was hauled up toward the surface after the tow was complete. Schmidt (2009) observed that the great majority of freshwater vendace (*Coregonus albula*) make an active escape from pelagic trawls during the interval when the vessel stops to haul the net in. Madsen et al. (2008) found that the haul-back and surface slack periods accounted for 20% of whiting (*Merlangius merlangus*) escapes from a trawl net, 33% of haddock (*Melanogrammus aeglefinus*) escapes, and 66% of Norway lobster (*Nephrops norvegicus*) escapes. The rate of escapement from the net, measured in the number of escapes per minute, changed differently for all three of these species during haul-back and surface time: for haddock, the towing escape rate increased by a factor of 2.7 during haul-back and by a factor of 1.7 during the surface slack period; for whiting, all three escape rates were comparable; and for Norway lobster the haul-back and surface periods both saw the escape rate increase by a factor of 7 over the towing rate (Madsen et al., 2008). So selection not only occurs after the tow is over, but actually tends to accelerate for certain species during that time. Furthermore, the measured selectivity of a net for any given species during the tow is a poor predictor of the net's subsequent selectivity during haul-back operations, presumably because the latter type of escape occurs through different behavioral mechanisms. Scandol et al. (2006) lament that the prevalence of such escapes during haul-back makes it difficult to judge the effectiveness of any selectivity-related design modifications, and this is in fact the crucial point I am trying to make. The technical act of selection is nondiscrete: it has no clear endpoint, just as it had no clear beginning, and accordingly it defies any means of reliable measurement.

***(2) Propriospecificity*.** There is another curious aspect to the way in which the selection of fish occurs during haul-back of the net. It involves the highly variable rates at which different species manage to escape during this period. Some species seem to actively escape in droves during haul-back, while others do not show any appreciable changes in behavior (Broadhurst, 2000). One of the primary reasons for this variability is that fish, during haul-back operations, are not merely reacting to their perception of the suddenly slack netting and the suddenly negligible water flow around them. They are not responding only to exterospecific information about their physical surroundings. Rather, their actions emerge from propriospecific information about their own bodies as well—in particular, how well their bodies are able to adapt to the rapidly changing environmental conditions as they are brought from trawling depths up toward the ocean surface. As one study put it,

> The effect of [depth] on the escape of fish seems to be related to the decompression of the swimbladder that takes place during the haul back. Accordingly, fish (normally of bigger size than those that escaped at depth) show increasing panic behavior (as the trawl approaches to the surface) and force themselves through the meshes of the codend. (Grimaldo et al. 2008, p. 278)

Escape rates during haul-back skyrocket in certain species because, as the net is hauled upward, the sudden changes in pressure threaten to severely damage their bodies. In response the fish begin to panic, just as a human would panic if he or she were steadily pulled down into hundreds of feet of water without a chance to equalize the pressure in his or her ears. This is particularly true of gadoid fish such as the haddock and whiting mentioned above, since they are physoclists: their swim bladders are not connected to their guts, so they are unable to quickly evacuate excess volumes of gas from their bladders (Madsen et al., 2008). As physoclists are hauled to the surface, their internal organs quite literally threaten to explode. This might explain why, in one study, a delay of fifteen seconds during haul-back "was effective in allowing large numbers of red spot whiting (*Sillago flindersi*, Sillaginidae) to escape but had no effect on the behavior of other species in the cod end" (Broadhurst 2000, p. 54).

As all of these examples show, the information conducive to fish escape comes not just from the environment but from the fishes' own bodies. Indeed, by the time panicking fish manage to escape from a surfaced net, the damage has in many cases already been done. Fish that escape from the trawl net at or near the surface are likely to have higher mortality rates than those that escape during the tow (Madsen et al., 2008). They die either from their internal injuries or from the related impairment of their swimming abilities, which leaves them particularly vulnerable to predation by seabirds. Serious injury may also result for Norway lobsters hoisted to the surface in a trawl net. Due to the decreased salinity of seawater at the surface, as compared to the ocean floor, lobsters can suffer severe retinal damage and even blindness when they are hauled up to the surface (Harris and Ulmestrand, 2004). Hence their sudden change in behavior during that operation and the seven-fold increase in their escape rate.

As these examples make clear, selectivity during haul-back operations is not simply a built-in property of trawl net design. The information required for fish to escape from the codend does not come exclusively from the physical structure of the meshes or the selectivity device(s), and it cannot be designed into the trawl net like some sort of signaling device. Rather, the information required for selection is inseparable from each individual fish's anatomy, physical conditioning, and corporeal self-awareness. Selective information cannot be dissociated from the fish's location in the water column and its own perceived array of action possibilities, as conditioned by its anatomy and physiology. All of this makes the operative exchange of information in a selectivity device highly unpredictable and difficult to optimize.

Propriospecific information is no less important for successful escapes during the tow. There is widespread agreement that the selection of fish during the tow depends on the visibility of the netting and/or the selectivity device, since fish depend primarily on their own eyesight to decide where they can most plausibly escape from the trawl net, and where they cannot (e.g. Chosid et al., 2008; Ryer, 2008; Bullough et al., 2007; Gabr et al., 2007; Glass et al. 1995). Fish generally choose to avoid highly visible netting and, conversely, to swim towards highly visible escape holes. In theory, then, the effectiveness of a selectivity device could be maximized by simply designing it to be highly visible underwater. However, this is extremely difficult to achieve, for visibility is not a property of the selectivity device alone. Visual information is not purely exterospecific. Instead, as Kim and Wardle (1998) have shown, the visibility of trawl net components depends crucially on the actions of the fish itself. They note than the color of any selectivity device is more or less irrelevant for most trawling operations, since at depths of greater than 20-30 m light becomes entirely monochromatic. Visual perception in such conditions becomes primarily a matter of brightness contrast: the perceived difference in the luminosity of adjacent objects. In other words, the visibility of any particular netting panel or selectivity device is largely a function of what is visually aligned to be next to it. And this, of course, it largely a question of parallax as the fish moves about inside the net: many different components might potentially line up next to each other depending on the exact vantage from which they are viewed. Moreover, in the daytime ocean environment, virtually all ambient light percolates straight down from the faraway surface, directly overhead. As a consequence, when looking directly upward, the background of any sheet of netting will appear uniformly bright. When looking directly downwards, into the depths, the background of the same netting will appear to be uniformly dark. This means that netting constructed with white twine will shine extremely brightly when looking vertically downwards onto it, will appear as a dark silhouette when looking vertically upwards at it, and will, at some specific angle in between, blend in perfectly with the background luminance and become invisible. So the visual stimulation provided by the trawl net during the tow cannot be calculated in advance. It depends on precisely where the fish is positioned within the trawl, what the ambient light levels are (itself a complicated function of depth, water turbidity, cloud cover, position of the sun in the sky, and so on), and which direction the fish is looking relative to the surface.

Propriospecificity complicates the functioning of selectivity devices in other ways as well. For instance, fish have been observed swimming alongside the escape hole of a selectivity device for long periods of time, or even physically resting their bodies on top of the device, without making any active attempt to use it as an exit (Eayrs, 2007; Hannah et al., 2003). In such cases, propriospecific information, either in the form of muscle fatigue or the deeply ingrained optomotor impulse, clearly affects the fish's decision on how to react to the exterospecific information it has gathered about the immediately adjacent selectivity device. Its affordances of the surrounding environment are closely linked to an awareness of its own bodily capacities. There is simply no way to account for such propriospecific information in advance and then design it into a trawl net; the indeterminism of information simply cannot be minimized or removed from the net's operative actions. Rather, the action and information of selectivity in a trawl net are inextricably intertwined in the perceiving and self-perceiving body of the fish.

(3) Catalysis. If propriospecific information contributes in crucial ways to the operative functioning of trawl net selectivity devices, so too do a number of other non-designable causal factors. Among these additional functional determinants of selectivity are the light levels and water flow conditions within the net, the sounds that the net makes as it moves through the water, the time of day and time of year when the tow takes place, the geographic location of the tow, the temperature of the water, the size of the catch already in the codend at any given moment, the age of each individual fish, the social relations of all the fish within the trawl, and their prior experience with predation. All of these quasi-functional factors play a pivotal role in assuring that fish correctly respond to selectivity devices, and yet none of them are a controllable part of the trawl's design. Depending on the input of these additional causal factors, or the lack thereof, trawl nets might select fish exactly as they were intended to by their designers, or they may fail to systematically select fish in any way. In this sense, trawl nets and their selectivity devices are only partial causes of their technical act of selection. They help to bring about the desired technical result—indeed, they are quite obviously indispensible to it—but they cannot bring it about on their own. Characterized by their incomplete or partial causality, trawl nets may be considered as catalytic technical objects. They contribute to the behavioral and technical operation of fish selection, but do not determine its outcome. In order to illustrate this point, let us examine the other causal factors that make selectivity a catalytic property of trawl nets.

Light levels are crucial to the functioning of any selectivity device since, as I have already written, the primary response of most fish to a trawl net is visual. Initial herding behavior does not occur unless fish are able to see the trawl doors and net approaching—a requirement that appears to explain why the catch rate of many species declines precipitously at night (Ryer, 2008). Once inside the net, fish require either some sort of ambient light in order to make an active escape (Gabr et al., 2007) or total darkness in order to be passively deselected—sucked, in other words, unwittingly out of the selectivity device by the flow of water through it (Bullough et al., 2007). In the absence of light, other sensory systems, such as the mechanosensory lateral line system possessed by some fish species, are unable to guide fish into the net and then out of a selectivity device (Ryer, 2008). Some form of illumination is therefore necessary for trawl nets to function with any non-random degree of selectively. Yet light levels within and around the trawl net are difficult to control, let alone measure or gauge. As Chosid et al. (2008) point out, "Light level is a vital component of gear testing, but is difficult to evaluate. Undersea light levels are influenced by water quality, temperature, depth, cloud cover, moon phase, bioluminescence, anthropogenic sources, and sun position in the sky" (Chosid et al. 2008, p. 6). Furthermore, each of these factors can affect a number of attributes of the light in question, including its intensity, its wavelength(s), and its polarization. For these reasons, it is nearly impossible for trawl designers to control for light levels when testing the effectiveness of a particular trawl net (ibid).

The form of light known as bioluminescence is particularly illustrative of this point, for it has proven tremendously difficult to predict, measure, or understand. Until quite recently these flashes of light, produced by certain plankton (mostly dinoflagellates, copepods, and euphausiids) when they are disturbed in the water, were not thought to have any real influence on the operative functioning of trawls. Bioluminescent events were simply assumed to be too few and too weak to affect the suffocating darkness of the deep sea environment. Accordingly, trawls employed at great depths were assumed to function more or less blindly, through wholly mechanical rather than behavioral means. In 2006, however, Jamieson et al. proved this assumption wrong. Mounting unusually sensitive underwater cameras behind the mouth of the net, they found that a pelagic trawl towed at depths of 270 meters in a Norwegian fjord produced roughly five bioluminescent events per meter of head rope (the line framing the top of the trawl mouth) per second (Jamieson et al., 2006). This figure, when extrapolated around the entire circumference of the trawl mouth, meant that roughly 2500 separate bioluminescent events were produced during every second of the trawl's operation. And that was only at the trawl mouth: other large trawl components such as the doors, warps, and floats, which create large pressure waves of their own, are likely to produce bioluminescent events at comparable rates. The observed level of illumination was adequate to clearly light up the netting behind the trawl mouth and, the researchers speculated, might well be sufficient to stimulate normal herding behavior at a distance. At even greater depths of 500 meters, where fewer plankton are able to survive, the amount of observed bioluminescence decreased but still amounted to several hundred flashes of light around the trawl mouth every second. Yet even if this unexpected source of light is strong enough to affect fish behavior (and it presumably is), thereby influencing the selective abilities of deepwater trawl nets, it is not a causal factor that can be reliably counted on in advance. Seasonal, annual, and geographic variations in plankton density and species composition make bioluminescence a fickle and unpredictable phenomenon (ibid). The trawl designer can neither presume that it will occur, nor predict the intensity of the light that it will produce if it does occur. It is a causal factor that remains firmly beyond the designer's control.

We could make similar observations about the flow of water through a trawl net. On one hand, water flow is an essential element of the various ways in which different selectivity devices function. The selection that takes place at the meshes of the codend requires a smooth flow of water to guide fish directly to the open lumens and usher them quickly out of the net. By contrast, the selection that takes place in many other selectivity devices requires the creation of turbulence. This is typical of devices that require the fish to swim forward toward an escape opening, since turbulence makes it very easy to swim or even coast along in the same direction as the net. Areas of turbulence within nets therefore tend to attract fish toward them, especially larger fish that can maintain their swimming activity for long periods. The turbulence that is produced around selectivity devices—especially fisheyes and fish boxes—effectively buys such fish the time they need to orient themselves, locate the nearby escape hole, and then move actively through it (Eayrs, 2007). On the other hand, it can be quite difficult to control the effective (i.e. selective) flow of water through the net. Patterns of water flow can change drastically when large items "such as tree branches, logs or large sharks" are caught, since these "force meshes open at strategic locations" within the net (Graham et al. 2008, p. 347). But even when water is flowing correctly through the meshes and selectivity devices, the behavioral basis of selection can render that water flow ineffective:

> While water turbulence can attract fish toward the escape openings of a [bycatch reduction device], stimulating them to escape can be difficult, particularly for species strongly responsive to the optomotor reaction. Many fish prefer to remain in this location swimming easily with the trawl. Overcoming this problem is difficult and success has not yet been widely achieved. What is required is some way of temporarily disrupting the effects of the optomotor reaction" (Eayrs 2007, p. 68).

So the efficacy of water flow in correctly selecting fish is something that the designer can only control up to a certain point. Beyond that, both the physical production and the behavioral effects of such hydraulic patterns are out of the designer's hands.

As we have already seen , the sounds generated by the trawl also play an important part in the selection of fish. Indeed, it is generally accepted that "acoustic stimuli are the principal mechanism which determines initial orientation to trawl gear" (Bublitz 1996, p. 300). Numerous studies have shown that pelagic and demersal species of fish alike show complex avoidance behaviors in response to approaching trawls (Handegard and Tjøstheim, 2005; Mitson and Knudsen, 2003). In the top 150 meters of the water column, herring (which are pelagic) have demonstrated a particularly strong avoidance reaction to the noise of distant trawls and trawlers, diving quickly down into deeper waters (Vabø et al., 2002). Gadoids show a similar if somewhat less uniform diving response. This initial downward movement is likely to concentrate fish deeper in the water column, at a seemingly "safer" vertical distance from the trawling vessel but more directly in the path of the oncoming trawl net itself (Ryer, 2008). The increasing noise of the doors and the warps then stimulates a second general response in pelagic fish, this one in the horizontal direction. Its net effect is to move fish out of the path of the trawl. As Handegard and Tjøstheim (2009, p. 434) write of trawling noise and pelagic fish behavior, "We gain fish by vertical herding and lose fish through horizontal escapement." Demersal fish, which reside on the ocean bottom, have been observed to respond more or less uniformly to the noise of an oncoming bottom trawl by orienting themselves in the direction opposite from its approach (Ryer, 2008). This orientation prepares them for a subsequent herding response, which is typically stimulated by the sight of the trawl warps bearing down with their associated mud cloud. The sound of the trawl—and the fishes' behavioral responses to it—plays a fundamental part in determining which fish will eventually be retained by the net, and which will avoid capture. It therefore contributes to the trawl net's selection of certain fish rather than others. Yet, as already discussed above, fish seem to respond not so much to the individual noises generated by the advancing trawl, as to the total ongoing changes in their overall acoustic environment. Trawling noise is meaningful only in its relation to the surrounding noise of a fish's undersea habitat. This level of deepwater ambient noise can be significantly altered even by a passing rain shower or a shift in the prevailing winds at the surface (Mitson and Knudsen). As a result, the acoustic signature of a trawl net, like its internal water flow and illumination, cannot be entirely planned out by a trawl engineer in advance. No matter what form is given to a trawl net and its rigging, the sounds that it generates might be perceived by the fish in any number of ways, depending on factors that are entirely outside the control of the designer.

The propensity of fish to be released through a selectivity device is also highly dependent on exactly when and where the trawling takes place. In part, this variability is a seasonal function related to the changing morphology of fish bodies over the course of the year. While it stands to reason that increasing the bodily girth of a fish will make it easier to select (i.e. retain in the net), this is not always the case. Özbilgin et al. (2006) found that for any given body length of haddock, increasing girth meant a higher probability of retention in February but a lower probability of retention in September. This counterintuitive result is probably related to the bodily changes that occur with the animals' annual feeding and spawning cycles: In February, increased bodily girth is related to recent gonad development and corresponds to a fatiguing period in the fish's life; in September, by contrast, increased girth is related to plentiful summer feeding and increased muscle development. Naturally, the well fed, physically fit September fish will be able to swim for longer periods of time and make more effective escape attempts than the less fit February fish. The former will make better use of the escape possibilities presented by the trawl net's selectivity devices. Furthermore, some species of fish migrate to different depths or heights off the bottom at different times of year. Yellowtail flounder (*Limanda ferruginea*) on the Grand Banks, for example, display seasonal off-bottom behavior that makes them less likely to de-select themselves by slipping under the ground gear of a bottom trawl , but more likely to do so by passing over the top of the trawl mouth (Chosid et al., 2008). Such seasonal variations in selectivity are also believed to be related to water temperature. Fish in colder water are physically less capable of sustaining high swimming speeds, and are also less behaviorally responsive to interactions with the trawl: both factors that decrease the effectiveness of the net's selectivity devices (Özbilgin et al., 2006; Ryer and Barnett, 2006).

Fish also show diel patterns of vertical migration through the water column that can affect their likeliness of encountering and reacting to a trawl net (Chosid et al., 2008). In addition, the operative mechanisms of selectivity at night are likely to be different than those at day. Many species of flatfish, for example, are far less likely to engage in herding behavior at night, and can instead be expected to show a startle response, in which they abruptly "hop" or "flip" directly upward off the ocean floor. Such diel patterns are highly variable between different species of fish (Casey and Meyers, 1998).

In sum, time of day, time of year, and water temperature all affect the propensity of fish to escape from an oncoming trawl net. But these patterns of behavior can be difficult to predict. For one thing, there can also be significant geographical variations in the diel patterns exhibited by fish of a single species, as is the case for arctic cod (*Arctogadus glacialis*) and Atlantic cod (*Gadus morhua*), two species of opportunistic feeders whose movements may primarily be determined by the movements of their locally available prey (Chosid et al., 2008; Casey and Meyers, 1998). There is also a great deal of non-geographic but otherwise unexplained variation in the way that fish of any one species react to a trawl at different times of day (ibid). Ryer (2008) has suggested that some of this variation might be related to the exact part of the trawl that fish first encounter. He notes that when flatfish encounter a trawl during the night, their typical startle response of hopping off the bottom can have two very different outcomes. If they are startled by the trawl warps, their reaction will flip them over the wires and safely out of the trawl net's path. If they are startled by the wings or the ground gear of the trawl, their reaction will flip them up directly into the mouth of the net, where they will almost assuredly be caught. So different parts of the trawl work "better" or "worse" at different times of day. Trawl designers cannot, of course, determine in advance which part of the trawl will first be encountered by the fish. Nor can they plan for changes in the fish's behavior that result from changing water temperatures, the changing hour, or the presence of different types of prey. Nor can they know how physically fit the particular fish that encounter the trawl will be. All of these factors clearly alter the ability of the net to select fish, but none of them can be incorporated into the structure of the trawl net itself. The causal role of the net is limited in relation to each of them.

The selectivity of a net also changes according to the size of the catch contained within it. This variability is both physical and behavioral. Physically, as a trawl net's codend fills with up with fish, its shape begins to deform. The accumulating ball of fish in the back of the codend puts an increasing load on the netting ahead of it, stretching that netting out and forcing any diamond-shaped meshes in it to pinch themselves shut. This significantly alters the flow of water through the trawl. Typically the water just in front of the catch becomes quite turbulent as water collides with the densely packed mass of fish, while smooth water flow is maintained through a narrow ring of meshes immediately ahead of the catch that fail to close off. As the size of the catch increases, this phenomenon becomes more and more pronounced (Eayrs, 2007; Scandol et al., 2006), necessarily altering the selective abilities of the net: "Because the bulk of fish inside the codend is constantly increasing, as more fish are retained the configuration of the codend continuously changes. At any particular time, this mechanism has an effect on mesh openings and will affect the probability of retention of a fish entering the codend at that time" (Grimaldo et al. 2008, p. 278) However, the fish's physical access to the codend meshes and the openings of selectivity devices can be hampered even when catch levels are very low. For example, even a small number of flatfish are, due to their very shape, capable of blocking the meshes of the codend and thereby preventing the intended escape of undersized fish (Ryer, 2008).

Behaviorally, the reactions of fish in the trawl net also change as the size of the catch increases. Multiple studies have found that fish are more likely to exhibit protean behavior, and therefore escape, when there are still relatively few fish in the codend (Krag et al., 2009; Jones et al., 2008). The reason for this is that fish have greater freedom of movement early in the tow, and are therefore more likely to brush accidentally up against the netting, triggering a panic response. Other species are more likely to escape as the number of fish in the codend increases. During the tow, large numbers of ocean shrimp (*Pandalus jordani*) tend to become passively snagged on the inner wall of netting throughout the trawl. These shrimp can be considered caught—that is, unless there are large fish present in the codend. The movements of such fish within the net can alarm the shrimp, provoking them to initiate their own form of panic behavior—repeated tail-flipping—and ultimately escape through the meshes on which they were snagged (Hannah et al., 2003). So selection is greatly affected—both positively and negatively—by the presence of other fish within the net. So much so that the physical and behavioral mechanisms responsible for selection can actually change over the course of a single tow. Like the factors we have already examined, this one is wholly unpredictable and cannot be designed into the trawl.

The difficulties involved in designing the function of selection into a trawl net go on and on. For instance, the age of each individual fish plays an important role in determining its behavior within the trawl. Observations have shown that whereas adult individuals of haddock, whiting, and cod respond in relatively predictable manners to particular trawl components and selectivity devices, the behavior of conspecific juvenile fish is inconsistent and unpredictable (Catchpole and Revill, 2008). Even for fish of a given age, however, individual behavior inside the net will depend on each fish's prior experiences in escaping from predators (Ryer and Barnett, 2006). Finally, fish will behave quite differently depending on whether they encounter the net as an isolated individual or as a member of a larger school. Pacific cod (*Gadus macrocephalus*) have been shown to herd for significantly longer periods of time when they are encountered individually, rather than in a school (Ryer, 2008). Likewise, when schooling roundfish are herded by a trawl net, individual fish do not drop out of the group one at a time and fall back into the net, as they would if they were simply responding to their own level of fatigue. Rather, at a relatively early point the school will cease herding all at once and enter the net *en masse*, suggesting that its capture is a matter of group volition rather than individual swimming abilities (indeed, as Wardle [1996] points out, larger fish in such groups are capable of herding for an essentially indefinite duration). Age, experience, and sociality: all are important co-determinants of fish behavior and therefore selectivity within the trawl net. Yet none of them is part of the trawl net or its design process. None of them can be controlled or predicted or evaluated in advance.

Selectivity in a trawl net is a measure that necessarily involves all of the partial causes I have listed above, and assuredly others as well. The design and structure of the net is in itself only one contributing factor among many in the ultimate selection (or deselection) of each fish. The net certainly helps selection to occur, but the scope of its functional role is clearly limited. It catalyzes the essential transduction, but does not overdetermine it. Trawl nets thereby function through a form of technical catalysis. The causality that they provide is necessary for the success of their technical operation, but it is never sufficient in itself. A great number of partial causes, other than those provided by the design of the net, are required to produce the intended technical effect.

(4) Indeterminism. If a single technical effect of the trawl net (the selection of fish) can have many simultaneous causes, the opposite is also true. A single set of causes can ultimately produce different effects. It is never possible to say with certainty which effect will be produced by the relevant causes, and in this sense the trawl net's functioning is highly indeterminate. As DeLanda (2006) points out, the most straightforward examples of causal indeterminacy are thresholds: points, typically unforeseen, at which a given intensity of input suddenly begins to produce new and/or unexpected outputs. There is one such tipping point, in particular, that clearly affects the functioning of trawl nets: the so-called "reaction threshold of fish". This threshold is, once again, behavioral—entirely dependent on the mental and psychological state of the fish at any given time (Handegard and Tjøstheim, 2005). Stated simply, crossing the reaction threshold consists of putting a fish "on edge," fraying its nerves to the point where it will react very strongly to even a small disturbance. Following Handegard and Tjøstheim (2005), we might imagine that as a fish is approached and then overtaken, first by the fishing vessel, then by the trawl doors, and finally by the trawl warps, it is continually subjected to a series of anxiety-inducing encounters. The vessel passes in a cacophony of roaring engines and hissing prop-wash, punctuated by the crashing reverberations of its metal hull against the waves. Then, in rapid pursuit, the enormous, howling faces of the two trawl doors appear from the very depths toward which the fish likely dove in response to the vessel. Such consecutive encounters will unavoidably heighten the fish's state of vigilance. Accordingly, it is likely to react quite dramatically to a third source of noise—the trawl warps—even if it is more subtle and less frightening than the preceding sources. In other words, any fish subjected to steady amounts of stress will, at some point, begin to react quite sensitively to any new stressors. These new stressors may be components of the trawl, or they may precede the trawl's passage entirely. The tolerance of those fish targeted by a trawl "may change based on predator presence, prey availability, etc., causing further variability" in their reactions to the fishing gear (Handegard and Tjøstheim 2009, p. 435). For this reason, the observed reaction patterns of fish to any given survey trawl may vary wildly from year to year, a phenomenon so common that it that has been dubbed "the survey condition" (Godø and Wespestad, 1993). Once the fishes' reaction threshold has been crossed, their anticipated reactions to the trawl will fail to materialize: the same technical causes that were employed the previous year (embodied in the design and operating parameters of the survey trawl) will produce an entirely different set of reactions. Of course, it is difficult to know when shooting a trawl how stressed the target fish are, and how close they might be to crossing their reaction threshold—that is, if they have not done so already.

The existence of this perplexing behavioral threshold implies something subtler but equally important for the operative functioning of trawl nets: it underscores that the fish's perceptions and reactions are always decoupled rather than mechanistically linked. After all, if the reactivity of fish can be hastened through their repeated exposure to stress, this is only because there was some inherent kind of delay to their reactions in the first place. Fish may in fact perceive the net for long periods of time without necessarily reacting to what they see, hear, or feel. Their reactions to the net are not instantaneous, but integrated over time. They result from the steady build-up of impressions about the trawl components that come from continual observation. We might say that the reactions of fish in the trawl net are characterized by a tendency to hesitate, a proclivity toward "wavering or dithering" before finally settling on a particular response that is deemed appropriate (Kim and Wardle, 2005). Nothing underscores the importance of this dithering period quite like the behavioral changes that occur in its absence. Kim and Wardle (2005) developed a complex simulation of fish behavior in a trawl net that mimics with striking accuracy the observed movements of fish in actual nets. However, this was not initially the case. Before the authors inserted a dithering period into their algorithms—in other words, when the rules of cause and effect were still assumed to be mechanistic and linear—the simulated fish behaved something like yo-yos, switching repeatedly back and forth between alternating behavioral responses in a way that had little to do with reality. The authors concluded that,

> … any response by simple linear rules can lead to simple periodic oscillations of movement that do not mimic the sort of response observed in real life. Such a linear model leaves out the fact that the degree of mental attention of a fish to a particular stimulus will vary as its relative importance is assessed in the particular context. This is not a random process but might be seen as the relative importance being turned up or down by the nervous system, for example, by altering mental concentration or attention. One stimulus source may receive more or less attention and become more or less noticed than another. This wavering or dithering, therefore, becomes an essential part of deciding the response reaction at each time-step. (Kim and Wardle 2005, p. 222)

In other words, simulated fish only begin to resemble the observed, biological ones when the causes of their behavior are decoupled from their ultimate reactions. Depending on each fish's (highly contingent) level of anxiety and attentiveness, a certain set of certain perceptual cues may either trigger panic or lead to no observable change in behavior whatsoever. Fish behavior is non-linear with respect to the trawl net. There is no direct, linear relationship between a fish's proximity to the net and its behavioral responses to it; nor between those responses and the amount of time that the fish has spent inside the net (Kim and Wardle, 2003; 2008). A protean response, in particular can emerge at any moment, even when there seems to be no clear triggering mechanism for it. "This," Kim et al. write, "suggests that the process of modeling this sort of fish behavior requires non-linear methods" (Kim et al. 2008, p. 6). Of course, Kim and Wardle's (2005) earlier simulation of fish behavior provides strong evidence to support this assertion: its remarkable degree of verisimilitude was achieved only when non-linear Lorentzian chaos equations were introduced into the fish's decision-making process.

Indeed, the operative functioning of trawl nets, their behavioral selection of fish, bears all of the hallmarks of a nonlinear process. As we have seen, the existence of reaction thresholds in the fish mean that trawl net function is *indeterminate*: its causes, even when repeated identically, do not always lead to the same effects. Trawl net function is also *catalytic* insofar as the net's design or structure is only partially causal. It can never bring about its intended technical outcome on its own; it requires the convergence of numerous other causal factors that are undesigned and undesignable. Nor is there a linear flow of information at the heart of a trawl's operative functioning, no stream of signals transmitted directly between an emitter to a receiver. Rather, the flows of information required for a trawl net to perform its transduction are always partially *propriospecific*. They involve the fish's own corporeal self-knowledge and cannot be separated from its ongoing physical actions within the trawl. Finally, because ecological events have no clear beginning or end, the operative functioning of a trawl net is *nondiscrete*—a property that makes it difficult to even interpret selectivity in terms of specific causes or effects at all. All of these traits make it evident that trawl nets do not function through Simondon's linear model of mechanical transduction, but rather through nonlinear, affordance-based processes.

4.5 The difficulties of optimization

Their nonlinear mode of functioning makes trawl nets difficult to optimize through design, for there is simply no reliable baseline of performance against which the efficacy of any design modification can be judged. This is partly a result of the sheer difficulty in arranging identical independent trials of any given selectivity device. As Catchpole and Revill (2008, p. 19) argue, "it is not possible to make direct comparisons between trials" of different net designs, due to the unpredictable mix of fish that each haul will encounter, variations in environmental conditions, slight differences in the precise volumes of water swept by the trawls, and the use of different construction materials and trawling methods. It has been shown, for instance, that even very small differences in the speed of the fishing vessel (especially as this is influenced by the strength and direction of the undersea currents and the size of the surface waves) can cause a bottom trawl to lift off the bottom enough to affect its selectivity (Weinberg and Kotwicki, 2008; Weinberg, 2003; Weinberg et al., 2002). However, the exact effect of towing speed on the functioning of selectivity devices remains unclear (Eayrs, 2007).

Even when different research teams test a single kind of selectivity device, strict comparisons between their tests are typically impossible due to small differences in the construction of the trawls into which the devices were installed (Ingólfson and Jørgensen, 2006). Nor does the practice of having a single research team repeatedly alternate hauls between a control trawl and a experimental trawl produce reliable results: So great is the unpredictability of the catch achieved by successive hauls, that an "unrealistically high number of hauls are … needed to get a statistically valid result" from this method (Madsen, 2007). To make matters worse, the effectiveness of selectivity devices becomes further muddled by the propensity for large numbers of fish to be lost from the net when is hauled aboard, especially during rough weather—a phenomenon we have already examined (Eayrs, 2007).

Not infrequently, a number of these factors converge to change the selectivity of the trawl net or one of its components. In such cases, it can be quite difficult to know which of the factors is most important in effecting this change. In a 2008 study, the l_{50} estimates that Grimaldo et al. found for a 135 mm diamond-mesh codend (i.e. the length of the fish for which there was 50% retention inside the net) differed notably from those found in an earlier study. The authors attributed this change in selectivity to differences in the twine material used (polyethylene instead of polyamide), and/or the twine's thickness (a single layer of 8 mm, instead of two layers of 6 mm), and/or the water temperature (7° C instead of 12° C). This sort of conclusion—simultaneously detailed and rather vague—is typical in selectivity research, for even when the many factors influencing a device's selectivity are known, their relative importance with respect to each other remains a mystery (Catchpole and Revill 2008, p. 24). It is hard to say which of the many factors at play affects the net's selectivity most.

There are still other instances in which *none* of the known selectivity factors can account for the observed change in a net's selectivity. In such instances, the change can sometimes be attributed to some new, unanticipated factor. One study, for instance, found that the insertion of a sorting grid into a net had no effect on the bycatch of undersized haddock but, oddly enough, actually *increased* the bycatch of undersized cod by 114% (ibid). As an explanation, the authors suggested that perhaps the selectivity device so drastically reduced the number of large roundfish entering the codend that the net no longer benefited from the collective buoyancy of their swimbladders and instead began to sink lower in the water column, distorting and closing off the meshes through which undersized cod had previously been escaping. Such explanations can often seem baroque, as in this case, but they are at least plausible.

Finally, there are cases in which different studies offer clearly conflicting measurements of selectivity for which no probable source of disparity is ever found. For instance, Bullough et al. (2007) determined that the use of a square mesh panel had no effect on the catch of undersized cod or haddock, in direct contradiction to a number of other studies, and were unable to explain why their results diverged from the others. In the same vein, Catchpole and Revill (2008) point out that studies on the size selectivity of square mesh panels have come to a number of clearly contradictory conclusions: the panels allow more fish *above* the minimum landing size (i.e. marketable fish) to escape without affecting the retention of undersized fish; or, conversely, they allow more undersized fish to escape without affect the retention of marketable fish; or again, they allow greater escapement across the entire length range of the fish. As all of these examples illustrate, reliable measurements of trawl net selectivity are fiendishly difficult to achieve. Even the causal explanation for how selection is achieved (and then how it is achieved again, differently) proves in many cases to be irreducibly complex.

Given such obstacles, it is not surprising that fishermen are often uncertain about which selectivity devices they should use, nor that trawl designers (who are sometimes the same person) are often uncertain about how a given device should be designed. Part of this ambiguity concerns where, exactly, any one kind of selectivity device should be located within the trawl net. As we have seen, the water flow conditions within the trawl, as well as the accessibility of open meshes or selectivity devices, can change drastically depending on the size of the catch. This means that, "the ideal location of a [bycatch reduction device] is difficult to predict" since it will depend crucially on the size of the catch at any time (ibid, p. 57). If, for instance, a square mesh panel is to permit fish to escape, it must be located towards the back of the codend, just ahead of the balled-up catch—but not so far back that the fish become exhausted before reaching it (Catchpole and Revill, 2008). Some researchers readily admit that this optimal location, which is tied not only to the number of fish in the net but also to their physical fitness upon reaching the codend, cannot be known in advance (Eayrs, 2007). Others attempt to define an optimal position even while recognizing the possible futility of their goal (Scandol et al., 2006).

Researchers have had an equally difficult time trying to determine what constitutes the "optimum size of escape openings " in selectivity devices (Eayrs, 2007, p. 58). Indeed, it is not even clear whether a large opening will be more or less effective than several small openings at letting unwanted fish out of the net (Catchpole and Revill, 2008). Perhaps most strikingly, there is little agreement over how important it is for certain parts of a selectivity device to be included in its design at all. While mesh panels are often used to direct fish towards the anterior end of a sorting grid, for instance, “it has been difficult to rigorously test the effect of guiding panels, and their importance therefore remains unclear” (Eayrs 2007, p. 62).

The problem is not the absence of such rigorous testing, per se. Rather, it is that even careful observation and rigorous testing can fail to clarify how selectivity devices function. Increased attention can even make the operative function of such devices harder to understand. Hannah et al. (2003) observed a fisheye device in action, off the coast of Oregon, and were surprised to find no directed movements by fish toward its escape hole. Rather, fish seemed to be passively “belched” out of the hole when the net underwent one of its periodic undulations during the haul. They were then either sucked immediately back in through the hole (the exact opposite of its intended function!) or were able to swim away and escape. The passivity of this selective mechanism, along with its relative ineffectiveness, stand in sharp contrast to the much-acclaimed success of fisheye devices at stimulating active fish escapes in the Gulf of Mexico and the South Atlantic (Manjarrés et al., 2008). The Oregon researchers suggested that perhaps trawl nets are operated in consistently brighter conditions in such sub-tropical fisheries, and that this increased illumination makes it easier for fish to see and move toward the fisheye device in the intended manner. If such an explanation were valid, it would indicate that a single selectivity device can function through any number of different mechanisms depending on where it is used, a fact that would even further complicate designers’ already difficult task of optimizing their functionality.

Indeed, given all of the vagaries described above, it is almost certainly impossible for designers to optimize the functioning of a selectivity device. For it is doubtful that there exists any uniquely optimal solution for the problem of how to select any particular species of fish within a desired range of sizes. There are simply too many uncertainties and too many intertwined casual factors involved in the process of selection. The optimal design of a fisheye (its location, dimensions, shape, etc.) will clearly differ depending on the ambient level of underwater illumination, the size of the catch, the bycatch species that are encountered, the surface conditions during the haul, the temperature of the water, and so on. Depending on all of these causal factors, and others, there conceivably exist any number of relatively optimal design solutions to the problem of selecting fish.

This lack of unique optimality is, according to Manuel DeLanda (1993), one of the telltale traits of nonlinear systems. When there are a number of conflicting selective pressures acting on a system—DeLanda describes a peacock's tail that might help the bird's reproduce but also exposes it to greater predation risks—then a number of different optimal results might conceivably be favored. Or more correctly, under conflicting pressures there is no such thing as an optimal result to favor. All results in such a system are compromises between different pressures; all are therefore sub-optimal. Nonlinear systems are characterized by the emergence of relatively stable, non-unique solutions (called attractors) that are different from each other without being clearly better or worse.

Hence, in the design of trawl nets, North Sea fishermen might choose to employ one of three separate selectivity devices—square-mesh escape windows, Nordmøre-type sorting grids, or large-diamond-mesh codends—without ever appearing to converge on the single best-selecting solution from among these (Grimado et al., 2008). Or Baltic Sea fishermen might develop three different types of square-mesh escape windows—the Danish, the Bacoma, and the New Danish—without being able to pick a clear winner and settle on the single optimal design (Madsen, 2007). Likewise, in other parts of the world there may be even less convergence toward any of the "myriad" designs for square-mesh panels (Catchpole and Revill, 2008).

Given such complexity, it should be unsurprising that there are "no reports of widespread voluntary uptake of new selective gear designs" anywhere in the world—in other words, no convergence on specific designs—except where the use of a certain device has specifically been mandated by law (Catchpole and Revill 2008, p. 26; Hannah and Jones, 2007; Broadhurst, 2000). Nor should it surprise us that trawl nets increasingly utilize multiple selectivity devices at once (Wade et al., 2009; He, 2010). It is simply uncertain which devices will work, and how well, in any given encounter with a fish. The best strategy for achieving the desired selection is therefore to insert numerous different devices into a net, even if their intended technical results are redundant. The hope of such a design strategy is that an undesired fish, even after failing to escape from the first selectivity device, or the second, might find its way out of the next one. Given what we know about the way in which trawls function, this lack of concretization is not an example of poor design, but an acknowledgement of the functional nonlinearity inherent to any behavioral device.

In conclusion, we simply cannot expect the designs of trawl nets to concretize in a way that would allow us to forensically reconstruct them using Simondon's version of PEMR. They do not progress in a way that makes them increasingly resemble Cuvier's perfectly identifiable anatomical objects. They do not obey the rules of parthood that ostensibly characterize such organic wholes. Yet by throwing out Simondon's theory of "technical evolution," with its particular relationship between the technical object and its component parts, we leave the basic question of technical assembly unanswered. If trawl nets and their constituent parts are not related organically (i.e., anatomically) then how are they related? What is the relationship between the parts of a net and the collective whole that emerges from them? It is to this question—and its rather astounding answer—that I turn in Chapter 5.

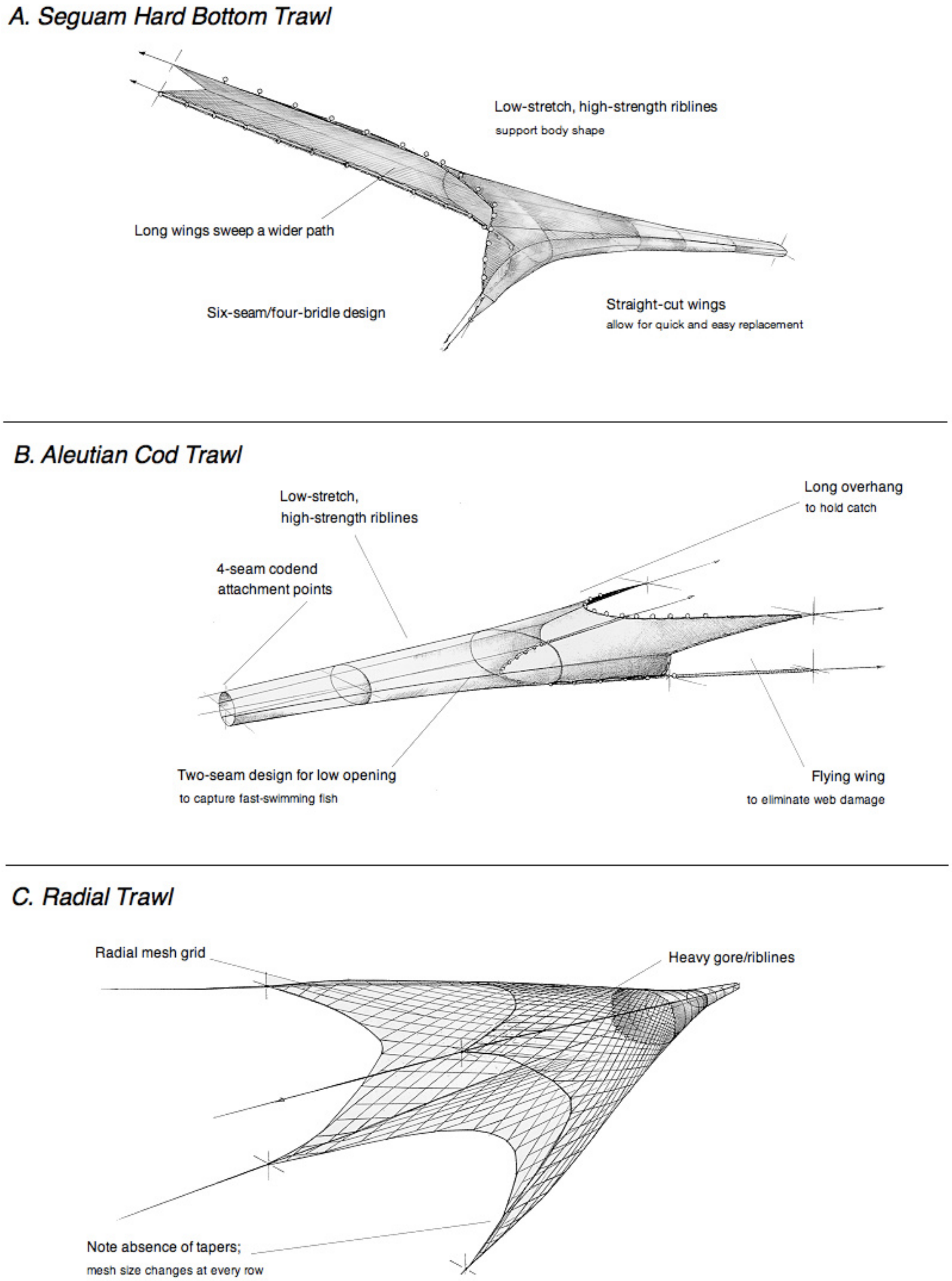
Chapter 5 . Technocenosis: The Structure of Trawl Nets

We have reached a crucial point in this study. Let us then briefly review where we are and what we have so far achieved. In the first chapter I showed that derelict trawl nets cannot be quickly or easily identified upon their recovery. Their fragmentation, complexity, and multiplicity requires that they be forensically reconstructed instead. In the second chapter I used the anatomical work of Georges Cuvier to demonstrate that the forensic reconstruction of complex functional objects has proven possible when certain structural or organizational conditions are met. I abbreviated these conditions EMR, and showed how they were made dynamic or progressive by early plant ecologist Frederic Clements (I called this PEMR) before being adopted nearly wholesale into Gilbert Simondon's philosophy of technical objects. The second chapter concluded that if trawl nets do, in fact, undergo the sort of concretizing progress laid out in Simondon's work, then it is reasonable to believe that they might avail themselves to forensic reconstruction. In the third chapter, however, I showed that the most important aspects of trawl nets fail to concretize. I suggested that the likeliest explanation for this failure lies in Simondon's notion of technical function, since the Simondonian model of concretization (i.e., his version of PEMR) requires energy and information to be exchanged in a specific and linear manner, called mechanical transduction, during the operative functioning of the technical object. The fourth chapter confirmed this suspicion, making it clear that trawl nets function through a wholly different transductive mechanism: the affordance-based perceptions and behaviors of living fish. Accordingly, they function in a nonlinear manner that confounds the optimizing impulse of concretization; and because trawl nets do not concretize, their parts do not become increasingly related to each other according to the structural tenets of EMR. This conclusive invalidation of Simondon's theory raised the obvious question of how the component parts of a trawl *do* relate to one another and how they fit together into the object as a whole. In this chapter I will answer that question. Crucially, in doing so I will also clarify what, exactly, a derelict fragment of netting can teach us about the larger trawl from which it originated.

5.1 The paradox of trawl design

The organization of any trawl net, the relationship between its various parts and the whole, is characterized above all by a paradox. It is a paradox with which I collided rather directly at the outset of my fieldwork, when I first began to interview trawl net manufacturers in Seattle about the prospect of identifying trawl nets from their derelict fragments. I entered this task under the assumption that specific styles of trawl net are employed to catch fish in different fisheries: that different *types* of trawl net exist for each. Indeed, the word “assumption” is somewhat misleading, for it implies that I simply failed to verify the existence of such trawl types before rushing into the interview process. But it cannot be said that I failed to verify this claim. Indeed, a thorough examination of trawl manufacturers’ websites made it abundantly clear that such distinct styles not only exist, but are aggressively marketed by net-makers.

In the days preceding my very first interview, with the management team at NET Systems, Inc., I made sure to do my homework on the company. I studied and restudied the entire product line of trawls featured on the company’s website. NET Systems seemed to offer a specific trawl net for practically every target species and every vessel type used in the northeast Pacific. Each net incorporated a number of specific design elements that served to distinguish it from the others, unique structural features that were highlighted on a striking back-and-white diagram of each net. For instance, the *Seguam Hard Bottom Trawl* (Figure 5.1a) had six longitudinal seams running from the trawl mouth to the codend; it also offered attachment points for four bridles, employed long, straight-cut wings on either side of the mouth, and featured a distinctly high-opening mouth. According to the net’s accompanying description, it was designed to catch Rockfish, Snapper, Pollock, Whiting, and Sole on rugged benthic terrain. By contrast, the *Aleutian Cod Trawl* (Figure 5.1b) had only two seams, featured a low, horizontally elongated mouth, and was built with “flying” or



A. Seguam Hard Bottom Trawl

B. Aleutian Cod Trawl

C. Radial Trawl

Figure 5.1 Three different types of trawl. (Images courtesy of NET Systems) bottomless wings in order to avoid damaging them on rough bottoms. It was designed to be towed quickly, by a powerful vessel, and to catch Pacific cod and sole. The cone of the *Radial Trawl* (Figure 5.1c) was very nearly cylindrical in cross section, had graduated meshes that changed size with every row, and did not employ tapers (angled cuts) at the edges of the netting panels. It was meant to maximize the herding of midwater species such as walleye pollock. All of these trawl nets had distinct overall appearances, employed different arrays of internal components, and were quite clearly intended for different types of target fish, bottom conditions, and/or vessel types. The same can be said of the company's Aleutian Cod Combination Trawl, Aleutian Wing Trawl, Bering Sea Combination Trawl, Hard Bottom Snapper Trawl, Monster Trawl, North Pacific Cod Trawl, Patagonian Semi-Pelagic Trawl, and Wonder Trawl.

I entered the board room at NET Systems confidently armed with this information. Yet when I began to make inquiries about the specific components that went into and characterized each type of trawl net, an uncomfortable silence quickly settled onto the room. The employees seemed either confused by my questions, in which case they stared at me quizzically, or simply embarrassed for me, in which case they stared down at the conference table. Eventually, and hesitantly, in the manner of a group attempting to speak with one voice, they explained that there was unfortunately nothing specific about the construction of any one of their trawl nets that could be used to identify it. No specific component, in itself, was diagnostic of one type of net or another. Nor was there any distinctive collection of components that might indicate the origin or purpose of their parent net. Certainly there were different types of trawl net, and each of them had been specifically designed for a certain kind of fishing (a particular target species, fishing ground, and type of fishing vessel). But when it came to any particular, individual net, there was no clear way of using its component parts to specify which type it belonged to.

The paradox, then was this: Viewed from afar, definite net types could be clearly differentiated. Yet, under close examination, the distinctions between net types became blurred and indefinable. Clearly different, they were nevertheless impossible to differentiate. To me this scarcely seemed possible. How could a uniquely patterned whole (e.g. the Hard Bottom Snapper Trawl) exist, and yet bear no identifiable relation to its assorted component parts? If the parts of a technical object did not add up to make the whole, then what on earth did?

This paradox is not unique to the product line at NET Systems, nor indeed to any specific trawl net manufacturer. It is a universal characteristic of trawl nets, regardless of their builder. Indeed, wherever attempts are made to clearly define and identify trawl net types, the same basic conundrum quickly emerges. In Mexico, for instance, a number of popular bottom trawl types can quite clearly be distinguished: the *balón*, the *semibalón*, the *fantasma*, the *mixto*, the *portugués*, the *semiportugués*, the *volador*, the *cholo*, the *plana*, the *hawaiiana*, and the *siete barbera* (INAPESCA, 2010). All of these nets have slightly different overall appearances (see the rough illustration in Quevedo, 2001) and are used for different primary target species of shrimp. Noting these obvious differences, INAPESCA, Mexico's national fisheries institute, has specifically sought to describe and catalogue them. However, despite its considerable scope and detail, the resultant national fishing gear catalogue offers no diagnostic means of distinguishing one type of trawl net from the others. Each type of trawl is named, and its frequency of use in different regions is noted, but nowhere are the differences between them identified and listed. Indeed, the following, contradictory assessment of *siete barbera* trawl nets is fairly typical of the INAPESCA report: "There are employed basically two different designs of [*siete barbera*] net, which do not maintain significant differences between them" (INAPESCA 2010, p. 30). How are we to understand this enigmatic statement? How can two different designs of trawl net exist, and yet not maintain any significant differences between them? If the two types exist, and they are different, mustn't they by definition maintain significant differences from each other? Mustn't they be differentiable? To answer these questions we must examine the process through which trawl nets are designed and engineered.

5.2 The basic structure of a trawl net

A trawl net, as A.L. Fridman states in his classic (1986) *Calculations for Fishing Gear Designs*, like "most fishing gear [that is] moving through water or set in current … [is a] spatial system of netting, ropes and related attachments subject to the action of various forces" (Fridman 1986, p. 19). We will examine the "various forces" involved in trawling momentarily. But before we do it will behoove us to review the basic parts involved in the "spatial system of netting, ropes and related attachments". These are summarized in Figure 5.2. Every trawl net is framed by a number of heavy ropes to which the netting is attached, and onto which it transfers much of its physical load during the act of fishing. The individual framing rope that runs across the top of the trawl mouth is called the *headline*. Its counterpart along the bottom of the mouth is called the *footrope*.

The headline is typically studded with a number of small, evenly spaced, hard plastic *floats* that prevent it from sagging down or flapping during the tow. In bottom trawls the footrope is usually threaded through various pieces of *ground gear*—mainly rubber discs and hard plastic bobbins—that help the net roll over obstacles on the sea floor. The footrope may also be draped with weighted sinkers and/or *tickler chains*, which are meant to stir up any flatfish that would otherwise remain stationary on the seafloor and pass underneath the trawl. Because the footrope in bottom trawls is often fully occupied by the ground gear, a second, roughly parallel rope called the *fishing line* may be used as a point of attachment for the bottom netting. Although they all take on a somewhat curved shape during the tow, the headline, the footrope, and the fishing line run more or less horizontally; their outer edges are necessarily connected by short vertical ropes called *wing-end lines*. Together, these horizontal and vertical lines define the perimeter of the trawl mouth.

Behind the mouth, framing ropes known as *lastridge lines* run along both sides of the net back to the codend, following and strengthening the net's major longitudinal seams. Most trawl nets

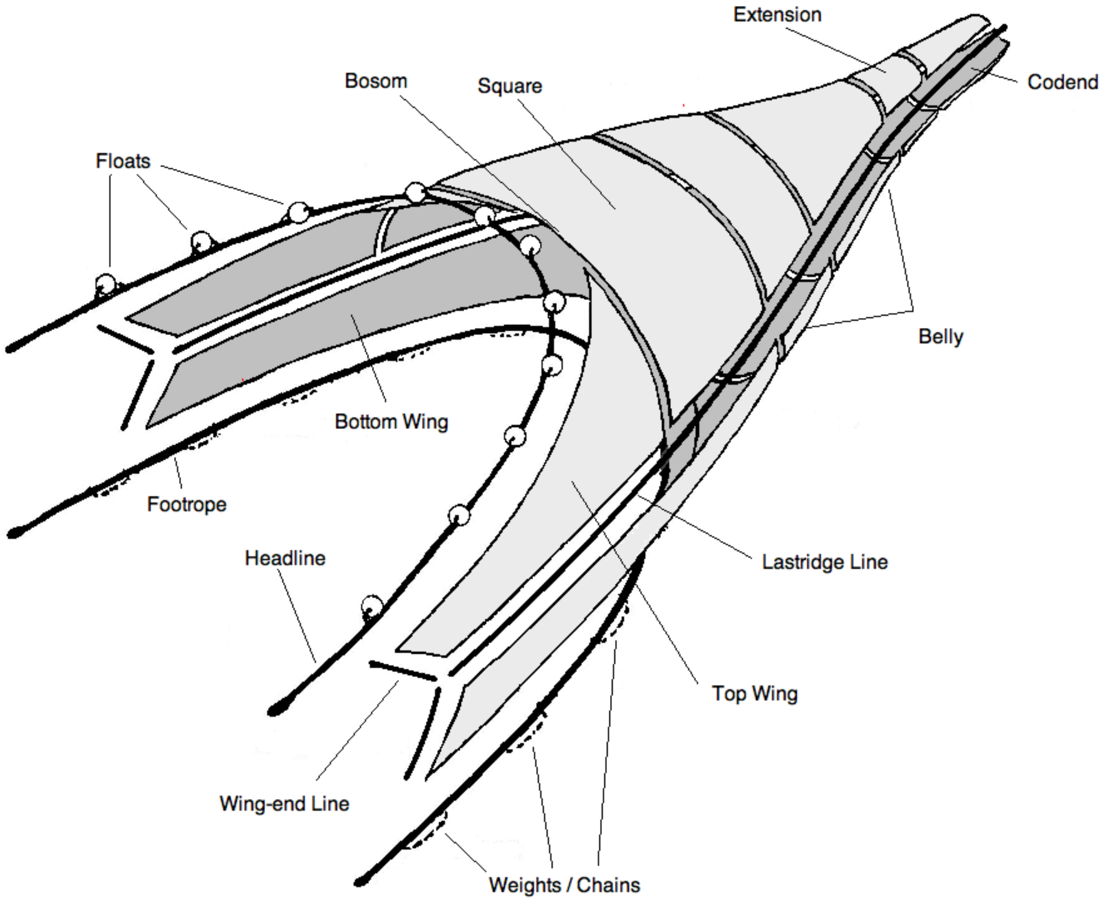


Figure 5.2 Exploded view of a basic two-seam trawl net. (Image modified from Hameed and Boopendranath, 2000.)

either have two seams, and are comprised exclusively of top and bottom panels of netting, or four seams, in which case they also incorporate paired side panels meant to increase the vertical opening of the trawl. Some trawl nets may incorporate six or even eight seams in order to further increase or adjust their vertical dimensions, but such designs are relatively unusual.

The framing ropes serve as the point of attachment for various netting panels, each of which has a specific role and position in the net as a whole. The foremost of the netting panels stretch forward and outward from the sides of the trawl mouth and are known as the *wings*. Depending on the design of the trawl, these may be comprised of top panels and/or bottom panels and/or side panels. The length of the wings typically depends on the propensity of the target species to engage in herding behavior. Midwater trawls, because they target visually acute schools of pelagic fish that have a natural tendency to herd, often use very long wings that will maximize the herding action of the net. Bottom trawls, on the other hand, often have very short wings since they target demersal species that show only a weak herding response.

Accordingly, in bottom trawling many fish may encounter the footrope and fishing line without ever being effectively herded by the wings. The natural response of such bottom-dwelling fish, upon being startled by the footrope, is to put on a burst of upward speed and escape over the top of the trawl mouth. To prevent this, the headline of a bottom trawl is typically positioned further forward than the footrope. The top of the trawl mouth is then defined by a canopy of overhanging netting called the *square*. The front edge of the square is often attached directly to the back edges of the top wing panels. However, additional triangular panels of netting known as *jibs* may be used to shape and strengthen the connection between the wings and the square. The relatively short central portion of the square's leading edge, which remains uncovered by the wings and jibs, is known as the trawl's *bosom*.

Behind the wings and the overhanging square lies the main body of the trawl net. This is known as the trawl *belly*. Wide near the mouth, the belly tapers smoothly back into a much narrower aft end, producing the net's characteristic funnel shape. Most often, this portion of the trawl net is composed of multiple bottom panels, collectively known as the *bellies*, and at least one pair of matching side panels (except, of course, in two-seam nets that have no side panels). The top panels of the belly are referred to as the *batings*. Behind the rearmost belly and bating panels is a long tube of netting known as the *extension* or *lengthener*. This typically has the same circumference as the codend, but is built less ruggedly. Its job is simply to guide fish back into the codend and, in some cases, to increase the amount of tiresome swimming they must undertake on their way there.

Lastly, at the rear of the extension is the *codend* itself. This is a tubular section of very strong, relatively small-meshed netting that is typically built with only a single seam in order to minimize its points of potential failure. The back of the codend is tied closed during the tow using a line that passes through a number of metal rings. The middle of the codend is often girdled by a circumferential line known as the *splitting strap*. When cinched off, the splitting strap can be used to separate the total catch into smaller portions at the end of a tow. This allows extraordinarily large catches to be hauled aboard bit by bit, preventing the possible failure of the vessel's winches and cranes or even the capsizing of the vessel under excessive catch weight.

Each netting panel is either laced directly onto the framing ropes or, at the trawl mouth, is sometimes hung onto a separate line called the *bolsch line*, which is itself tied to the framing rope at regular intervals of a few meshes. Where neighboring netting panels abut one another in the transverse direction, perpendicular to the direction of trawling, they are typically *joined* by using a length of twine to braid in an extra row of meshes between the two panels. Where netting panels abut in the longitudinal direction they are typically *seamed* by overlapping 3-6 of their meshes and then lacing the end of each panel to its neighbor with a series of closely spaced half hitches. In this way the numerous component panels of a net are assembled together into a truncated—and often partially flattened—cone shaped structure that is undergirded by a heavy rope frame.

5.3 The shape of a trawl net

As Figure 5.2 illustrates, the basic architecture of a trawl net is fairly simple. It consists of only a limited number of parts, and these are organized in a fairly uncomplicated manner. However, the assembly of such an apparently straightforward structure is actually quite difficult in practice, and requires the designer to overcome a number of unusual engineering obstacles. The primary difficulty in this regard is the non-rigidity of the structural material, an inherent pliability that is accentuated by the dynamic, fluid nature of the underwater environment. As Fridman writes, fishing gears must take a highly compact form onboard the fishing vessel but "must assume relatively large dimensions while fishing, [and] must easily change their shape and position in space" to accommodate changing fishing conditions (Fridman 1986, p. 16). "For this reason," he continues, "the main structural material is textile netting, which is flexible, permeable and anisotropic (i.e., physical properties such as strength and elasticity may differ in different directions) (*ibid*)." Yet the problem with netting material, for the engineer, is that it "has virtually no resistance to axial compression, bending or twisting, so it cannot maintain a rigid shape" (Fridman 1986, p. 19). Accordingly, the engineer must expend an inordinate amount of effort simply "calculating what shape and position a fishing gear will assume in operation" (*ibid*). Because fishing nets do not maintain any particular shape over time, and because they have relatively short service lives, the trawl engineer cannot apply traditional strength and stress analyses to them. "Instead," Fridman explains,

> it is necessary to estimate the shape and spatial position of the gear as controlled by the equilibrium of external static and dynamic forces during the fishing operation. Estimation of these forces is complex because they in turn are affected by the shape and position of the netting which in general is not known in advance and which can change easily. (Fridman 1986, p. 16)

The engineer, in other words, is left with no clear analytical starting point: in order to estimate the net's shape and position at any moment, he or she must know the magnitude of the external hydrostatic and hydrodynamic forces acting on the net; but in order to estimate these external forces, he or she must know the shape and position of the net. The solutions of these calculations are intertwined with each other, forming a kind of catch-22 that can be circumvented only gradually, through successive numerical approximations and adjustments.

This iterative process would be vastly simplified were trawling characterized by steady motion and, accordingly, a consistent interplay of physical forces. But trawling is an inherently jarring process in which the speed and direction of the fishing gear are perpetually in flux. The vessel surges and plunges through the surface waves, putting enormous periodic strain on each of the trawl warps; the warps themselves tremble and shudder violently as they are forced through thousands of feet of seawater at a steep angle; the net frame shudders and jerks as the ground gear bounces and scrapes its way across the bottom; the netting panels themselves stretch, buckle precariously, and rebound as they encounter unexpected debris items or encounters sudden large schools of fish; and the entire net slowly torques one way and then the other, releasing this built up energy in sudden, convulsive undulations every few minutes. This unpredictable array of motions makes it tremendously difficult to calculate with precision the various forces whose equilibrium will determine the shape of the gear during the tow.

Though it is laborious, the determination of a net's operational shape is of crucial importance. It is the single aspect of trawl design that most clearly affects the swimming behavior of fish and can be most easily adjusted to accommodate that behavior. In particular, Fridman singles out two separate aspects of the trawl net's shape that deserve special attention: the shape of its mouth, and the shape of its body (i.e., the bellies and batings). The shape of a trawl mouth, he writes, should be designed to accommodate the typical swimming distribution of the target species—the way in which its individuals tend to swim in relation to both the ocean bottom and other conspecific individuals. A shrimp trawl, as Fridman points out, should have a mouth with a very low vertical opening and a very wide horizontal reach, since shrimp tend to congregate in dense shoals only a meter or two above the ocean floor. Above that level, any additional height in the vertical opening will constitute a waste of netting, since the upper part of the opening will almost never encounter any shrimp. Similarly, fish that distribute themselves as widely scattered individuals across a broad vertical section of the water column should be targeted by a trawl whose mouth is expansive in all directions. This will maximize the net's probability of encountering and capturing its widely dispersed quarry.

The shape of the trawl body that trails behind the mouth, composed of the bellies and batings, should be designed according to the target species' propensity to become startled and exhibit protean escape behavior. This, of course, depends on the fish's behavioral hardwiring as well its physiology. Strongly swimming fish with sharp vision and no tendency to engage in crypsis (i.e. hiding behavior) are likely to become spooked by the approach of any netting panel that is angled sharply inward. The visual obtrusiveness of such netting, its looming effect, may stimulate a panic response and lead the fish to escape from the front portion of the net. For this kind of target species, a long, gently tapered trawl body is clearly more appropriate than a short, abruptly foreclosed one. By contrast, a shrimp trawl can incorporate a highly truncated body with no adverse affects. Its belly panels can be angled sharply inward from the mouth toward the codend, since shrimp are unlikely to make a successful escape even when confronted with the sudden appearance of a netting panel in their visual field. The angle of the netting in a shrimp trawl is therefore limited only by the desired pattern of water flow through the net: if the angle increases too dramatically, the prevailing water currents could begin to carry the shrimp straight out through the meshes rather than sweeping them within and alongside the meshes into the codend.

We should note that, while other aspects of fish behavior and anatomy also affect the design of the net, these do not take the form of design options that may be freely manipulated by the net engineer. For instance, the mesh size of the codend is selected as a function of the target species' minimum landing size and body morphology. However, minimum landing size is typically fixed by law, and morphology is fixed by the fish's evolutionary history, leaving the designer with little freedom to experiment with novel solutions. Indeed, in most commercial fisheries the mesh size in the codend (rather than just the minimum landing size) is now directly mandated by law, essentially eliminating that portion of the design process. Likewise, the sustained swimming speed of the target fish determines how fast the trawl net must be towed in pursuit of it. For any given fishing vessel and its engines, this required towing speed dictates how much total drag the net may permissibly generate and, therefore, its maximum dimensions. Again, this design parameter is predetermined by the swimming speed of the fish and the available towing power of the fishing vessel, conditions that the trawl designer must take into account but is in no way free to modify. When we consider the design problems for which a trawl engineer is actually seeking to create novel solutions, rather than simply plugging in certain sets of preordained numbers, we are left with the problem of achieving a certain shape for the trawl mouth and a certain shape for the trawl body. This constitutes the primary goal of the designer; all of his work revolves around this task.

It is worth emphasizing that the shape of a trawl net does not depend in any strict way on its physical dimensions, or those of its pieces. Fridman warns any would-be engineer that, "we cannot judge the shape of the trawl net during trawling from the shape of the individual parts from which it was assembled," since "in operation the trawl behaves as an entity" (Fridman 1973, p. 257). In other words, even the best laid blueprints of all the trawl components may fail to indicate the resultant shape of the finished net. Hannah et al. (2003) forcefully attest to this shortcoming in their study of shrimp trawls used off the Oregon coast. Their underwater video recordings make it clear that regardless of net manufacturers' concepts of how a net will look in operation, they actually "have no information on the shape any particular net takes while fishing" and may produce something whose shape departs quite drastically from their own expectations (Hannah et al. 2003, p. 24). This holds true despite the development, in recent decades, of hugely expensive testing facilities for scale models (see Winger et al., 2006) and the concomitant rise in sophisticated software packages meant to simulate the interplay of forces acting on a trawl during its operation. Fiorintini et al. (2004), for example, were dismayed to find that a full-size experimental trawl net built with hexagonal Raschel netting behaved quite differently than the simulations and flume tank tests had led them to expect.

Much of this difficulty stems from the basic fact that the shape of the mouth and the shape of the body are, like any shape, non-metric parameters. They are entirely independent of any of the net's particular physical dimensions. A circle is a circle, no matter what its size. So for example, "The dimensions of a trawl mouth opening cannot be estimated accurately from the trawl diagrams and drawings. Trawls of the same size may have very different mouth openings, whereas trawls of very different size may have similar mouth openings" (Fridman 1986, p. 157). Indeed, metric comparisons between trawl nets are essentially useless: they contain very little valuable information about each net's respective shape. For this reason, Fridman advocates using M. Nomura's (1969) method of net-by-net comparison, in which the *relative* structural dimensions (i.e. size ratios) of different trawls are used to characterize their designs, rather than their strict dimensions. A certain design might be characterized by its entire length in relation to the length of its headline, and/or the length of its lower wings in relation to the length of its headline, and so on. Still, this is a descriptive rather than a predictive method. How does a trawl designer ensure (or try to ensure) in advance that a trawl will take on a certain shape in the water?

5.4 Physical forces related to trawl design

As I have already indicated, the shape that any net assumes in the water depends on the particular equilibrium of static and dynamic external forces acting on it, as a whole, during the tow. Let us therefore examine those forces. There are two hydrostatic forces that affect a trawl during its operation: weight, which causes the net to sink, and buoyancy, which causes it to float. Weight and buoyancy are static in the sense that they do not change with changes in the net's motion. They are constant for any set of assembled materials.[20] Between these two forces, numerous possible equilibria exist. That is, when planning and calculating the hydrostatic forces acting on a net, its overall buoyancy might be balanced in any number of ways with its cumulative weight. If the two

---

[20] To clarify, because an object's buoyancy depends in part on its volume, buoyancy remains constant only for materials that maintain a constant volume. Deep underwater, increasing pressure means that compressible objects will in fact become smaller in volume, reducing their buoyancies.

have precisely equal values, then they will effectively cancel each other out and the net will remain neutrally buoyant. A slight increase in weight will cause the net to slowly sink, while a slight increase in buoyancy will cause it to slowly rise. The desired equilibrium between these forces depends on whether the net is meant for surface, midwater, or bottom trawling.

The same can be said of the relative hydrostatic forces acting within different *portions* of the net. Most notably, the weight and buoyancy of the top section, in comparison to those of the bottom section, will affect the vertical opening of the trawl mouth and body. Yet there is no correct equilibrium between the two: the chosen values depend entirely on how tall and/or wide the designer wishes the trawl to be. Accordingly, the precise weight and buoyancy of each individual component does not matter, per se. What matters is rather the cumulative relations between their weights and buoyancies. If the trawl operator desires that a heavier and more durable material be used in one netting panel, that change can easily be compensated for by adding buoyancy or decreasing the weight of another panel. Or such compensation can be dispensed with entirely, resulting in a slightly modified net shape and position in the water. In this sense, the engineer is not, at this point, dealing with the hydrostatic properties of specific components; rather, weight and buoyancy are considered somewhat abstractly across the entire body of the net.

The hydrodynamic forces acting on the net during the tow are hydrodynamic resistance, or drag, which acts in the direction opposite the net's direction of travel, and shear, or lift, which acts perpendicularly to the net's direction of travel. Drag and shear are dynamic forces: because they are generated by the motion of the net through the water, their values change with changes of the net's speed and direction. Generally, as speed increases, so do the forces of drag and shear. As with the hydrostatic forces, these can be balanced in any number of ways. Most fundamentally, the drag of the net must be balanced with the available towing power of the fishing vessel—often measured as the vessel's *bollard pull*. Put simply, the fishing vessel must have sufficient towing power to overcome the total hydrodynamic resistance of the net at the desired fishing speed, plus an extra 10-20% of that power for scenarios in which an extra burst of speed or power is needed (to avoid an obstacle, say, or to free a net that is snagged on the bottom). The total permissible drag of the net is therefore limited by the vessel's towing power.

However, this limitation does not strictly curtail the total amount of netting (by surface area) that may be utilized, for the magnitude of the drag and shear forces depends largely on the angle of incidence between the netting the water. Any piece of netting placed at an oblique angle with respect to the oncoming water will generate both drag and shear. As the angle of any given netting surface becomes increasingly normalized (i.e. closer to 90 degrees), the shear forces decrease and the drag forces increase. Conversely, as the angle of incidence becomes less normalized, the drag forces will decrease and the shear forces will increase. Because shear forces help to pull the net open horizontally and vertically, they are limited only by their potential, at extreme values, to grossly distort the netting in either direction. Achieving the desired shape of the net therefore depends on striking a certain balance between the forces of drag and shear. By changing the angle of the netting, one achieves either more shear and less drag, or more drag and less shear. Either way, what matters, once again, is not so much the precise magnitudes of the hydrodynamic forces on each component, as it is the relationship between both forces across the entire body of the trawl.

The initial processes of balancing shear with drag, and weight with buoyancy, are not matters of exactitude. As I have already mentioned, the speed and position of the trawl changes quite frequently during the act of fishing. This variation affects the net's height in the water column and alters its coefficients of shear and drag, changing its overall shape. Yet the net continues to function quite adequately during these periods of minor alteration. The trawl designer therefore works under an assumption that the shape of the net, as governed by the external forces acting on it, will undergo constant small changes during the tow. It will not remain completely rigid underwater. Accordingly, there is a relatively wide range of acceptable values for the forces generated by the net.

Because of this operational leniency, and because the effects of any localized design alteration can be easily counteracted in some other part of the trawl, each netting panel that is eventually inserted into the net will be required only to remain within relatively broad overall limits on the amount of drag, shear, buoyancy, and weight it generates. Only when the netting components surpass these extreme boundaries will the net begin to flap or shake or collapse in on itself or simply assume an undesirable tilt in the water. When shaping the net, the job of the engineer is therefore to calculate the probable ranges of these external forces by using specific information on the fishing vessel, its expected catch volumes, and the typical surface conditions, currents, weather, and benthic topography of the fishing grounds. Armed with this information, the engineer ensures that the net will not become dangerously unstable and/or lose its shape entirely by surpassing its acceptable hydrostatic or hydrodynamic limits.

5.5 The selection of components for inclusion in the trawl

Once the probable external forces acting on the (still unconstructed) net are known, allowing its overall dimensions and approximate shape to be roughly plotted, the engineer turns his attention to the internal forces that will affect the netting during the tow. With this switch, the assembly process undergoes something of a major shift. Instead of merely planning a projected overall shape for the desired net, the engineer must begin to select the exact collection of component parts that, when united, will allow the net to achieve and maintain that shape. In other words, the rather abstract question of what shape to give the overall trawl quickly morphs into the applied problem of picking which parts to use in its construction.

In its rapid shift to the problem of choosing parts, the assembly of a trawl is not exactly an invention in the classic sense. It is not the creation of an entirely new technical object from scratch. Rather, trawl assembly is primarily a process of *selection*. It revolves around the problem of choosing, from a multitude of preexisting design elements, those which will be able to productively coexist within the finished net, maintaining the stability of its desired form during the act of fishing.

This is emphatically not to say that the field of trawl design is entirely bereft of inventions. New design elements are of course invented from time to time. Yet this does not alter the basic task of the engineer. Newly invented components simply become available, like any other component, for selection or inclusion within a particular net design. They are, so to speak, simply thrown into the engineer's big bin of parts. This inherently selective (rather than inventive) nature of trawl assembly is quite important, for as we shall see, the paradox of trawl net design results from the peculiar manner in which its various components are selected.

5.5.1 Functional Equivalence / Functional Under-determination

To see what makes the selection of trawl components so unique, let us pick up the description of the trawl design process right where we left off. Having already calculated the external forces that are likely to act on the net as a whole, the engineer must then ensure that each of the net's individual parts will be capable of withstanding and distributing the *internal forces* generated by the trawl during its operation. This means that specific panels of netting must be provisionally chosen for inclusion within the trawl. If the enormous tensile loads produced during the tow exceed the breaking strength of any of the selected netting panels, the net may fail catastrophically. Indeed, despite the monumental size and nearly unimaginable strength of most modern trawl nets, "trawls have been burst, particularly in the batings and lengthening piece, when towed inadvertently through large, dense shoals of fish at high speed" (Fridman 1986, p. 75). The engineer must prevent such failures by selecting netting components that, in addition to producing the external forces that give the net its desired shape, are also capable of withstanding the immense physical strains of trawling.

To the engineer, this challenge seldom presents much of a problem. For there are typically many different ways in which the requisite strength can be achieved in a given sheet of netting. The breaking strength of mesh netting is not dependent on any one aspect of its construction. Rather it is the collective result of many different design parameters. Among other things, breaking strength is related to the elasticity and linear density of the yarn material employed, the diameter and permeability of the twine built from that yarn, the method of twine construction (twisted, braided, etc), the knot type or method of knotless construction, and the size of the spaces or lumens in the mesh. Numerous combinations of these factors, as well as others, might all conceivably produce meshes with the necessary strength to withstand the trawl's internal forces. For instance, a single strand of wet, knotted polyester twine (built of continuous filaments) with a diameter of 1.0 mm has essentially the same breaking strength as a twisted strand of polyethylene twine with a diameter of 1.42 mm (Fridman, 1986). Either type of fiber might therefore be used, when constructing the mesh, to meet the breaking strength requirements of a certain netting panel. In fact, any type of twine with a breaking strength equal to or greater than theirs may be used in their place without jeopardizing the integrity of the trawl as a whole.

The relation between the mesh and the overall net is therefore one of *satisficing* rather than maximizing: as long as the meshes satisfy certain basic physical conditions, they will suffice to produce the desired functional effect in a given part of the net. This basic *functional equivalence* means that different kinds of netting are in general highly replaceable by one another—especially since, unlike in the codend, "in the forward parts of the net the mesh size is not so critical" (Fridman 1986, p. 154). Accordingly, the engineer's choice of which mesh type to employ is *functionally underdetermined*. There quite simply is no clearly optimal choice of which mesh to use, based on the functional requirement of breaking strength. This is not to say that the engineer's selection of mesh is therefore entirely random, for whatever netting is selected must obviously comply with certain functional requirements (*e.g.* breaking strength) if the net as a whole is to avoid rupture. But such functional criteria, on their own, are not sufficient to dictate that one particular type of netting be included in the trawl instead of another.

This kind of functional ambiguity is not restricted only to breaking strength. It applies to virtually every other functional parameter of the netting as well. To illustrate this, we could more closely examine the individual meshes themselves. It is productive here to think of each individual *mesh cell* as an entity. A mesh cell consists of the twine surrounding a single lumen or "eye" in the mesh. It is the fundamental unit of trawl assembly, since its basic structure and shape are repeated over and over again to form any particular netting panel. This collection of more or less identical mesh cells is the legally accepted definition of trawl netting (see MARTRAWL, 1998). Now: It is clear that any particular species of mesh cell, any mesh cell that is built according to a particular set of technical specifications, will also have a particular set of functional attributes. Each will be able to tolerate a certain range of tensile loads without failing, and each will have its own specific buoyancy and weight. Likewise, for any given trawling speed (predetermined by the swimming speed of the target fish) and at any given angle of incidence with the oncoming water (predetermined by the engineer's blueprint of the overall net), every species of mesh cell will produce a certain amount of shear and drag. Indeed, what makes one species of mesh cell distinct from another is precisely how, due to its manner of construction, it produces and/or distributes these forces (MARTRAWL, 1998).

In order to makes sense of this collection of attributes, it is helpful to think of the functionality of each type of mesh cell in a somewhat more abstract manner. We could say that each species of mesh cell has a given weight, W; that it has a buoyancy of between X and Y, depending on the depths at which it is used; that it can tolerate physical loads up to a certain intensity, Z (its breaking strength); and that it will exert drag of between A and B, and lift/shear of between C and D, depending on its precise speed and its angle with respect to the direction of water flow. We can then visualize this collection of physical properties by treating each type of mesh as a unique volume (or hypervolume) in multidimensional phase space, as pictured in Figure 5.3. In other words, if use each of these functional gradients (weight, buoyancy, physical loading, drag, and shear) as a separate axis in multidimensional space, we cold plot out a unique functional volume that each kind of mesh cell would occupy. Visualized in this way, it becomes clear that each species of mesh cell will have a particular functional "niche" in which it can be used. In other words, each is restricted to uses

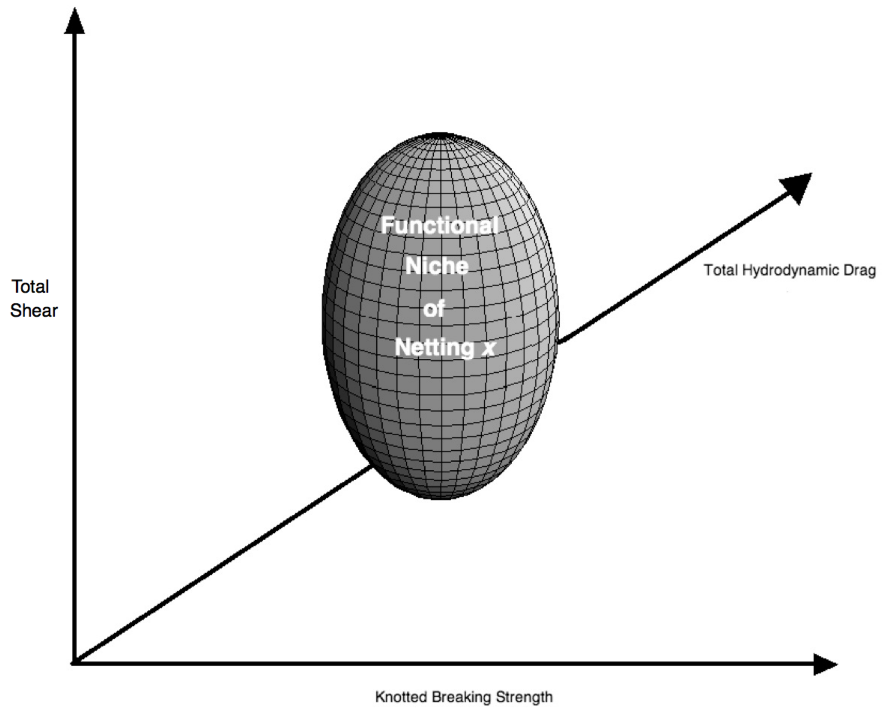


Figure 5.3 The "functional niche" of a given netting type can be visualized as its volume or hypervolume in a multidimensional phase space whose various axes define basic functional parameters. Because each type of netting has its own unique functional capacities, each will have a distinct shape and occupy a distinct region within this phase space.

that comply with its own functional limits. If its weight or drag or buoyancy surpass the range of desirable levels for a particular portion of a particular trawl, then it will not be included in that trawl's assembly. It will be selected against, in favor of some other more suitable species of mesh.

The ambiguity of different mesh types, for the designer, stems from the fact that their functional niches quite frequently overlap. That is, different kinds of mesh can often tolerate the same physical conditions, and can therefore be employed indiscriminately to fulfill the same basic requirements in any given net. The netting ultimately chosen for any particular location in a trawl will be required to absorb a certain amount of internal physical loading. In addition, for the trawl to maintain its desired shape, that netting will also be required to have a particular weight and buoyancy (within a certain range of acceptable values) and to produce a certain amount of drag and shear (again, within a certain range of acceptable values). These "job requirements" mark out what is essentially a region or volume in the graph above. The problem, for the designer, is that this region may be occupied by any number of different mesh cell types. A large number of applicants will potentially qualify for the job.

This functional under-determination is further compounded by the fact that the trawl engineer does not generally incorporate single mesh cells into the trawl. Rather, what is selected for inclusion in a particular trawl net are netting panels: sheets consisting of a certain number of more or less identical mesh cells that are repeated in continuous rows. The trawl engineer is not only choosing mesh cells, per se, but *populations* of mesh cells. The act of trawl assembly is the process of bringing these different populations together to produce a desired overall effect. And what matters from an engineering standpoint is not so much the individual behavior of a single mesh cell (although this remains important), but rather the emergent behavior of the entire population of cells within a netting panel. The size of the netting panel, the actual number of mesh cells that it contains, therefore becomes an additional parameter that must be adjusted in the assembly of the trawl net. For example, by significantly increasing the net's use of one type of netting and significantly decreasing its use of another, an engineer might alter the shape of a net quite radically without changing either its overall dimensions or the kinds of netting that it contains. So too, it is the precise interaction the various netting panels that becomes important, beyond the individual interaction of one mesh cell with another.

Within a given trawl net, the choice of what netting panels to include is inherently under-determined, just like the selection of individual mesh cells. For, different netting panels are themselves beset by a certain degree of functional equivalence. The shape of a completed trawl net depends largely on the overall outlines of its respective top, side, and bottom sections. Yet any number of netting panels might conceivably fit into a given section without changing its overall shape. For instance, an engineer might determine that the bottom belly of a trawl should be an elongated trapezoid, 55 feet length and tapering from a width of 31 feet in the front to 8 feet in the rear. Knowing these dimensions, and calculating from them the necessary taper ratio of the trapezoid's edges, the engineer can quite obviously use different combinations of joined panels to create the required overall shape. As long as the overall dimensions and outline of the belly section remain the same, it makes little difference whether the belly is comprised of four large panels or seven smaller ones. Two trawl nets that assume an identical size and shape in the water might therefore be constructed from different numbers of panels of different shapes.

Indeed, what matters most in the shaping of a trawl net may be neither the exact identity of the mesh cells it contains, nor the precise number of netting panels that contain those meshes, but rather the ways in which the various panels themselves are connected to each other: the ways in which each population of mesh cells interacts with the other populations. The shape of a trawl can be adjusted quite considerably by changing the way in which one netting panel is attached to another. Since it is precisely at their shared edges, or seams, that the netting panels transfer the trawl's energy to each other, altering those seams necessarily affects the distribution of energy between netting components and changes the overall shape of the net. This is true both of the actual shape of a seam, its arcs or angles, as well as the specific technique by which one panel is physically joined to another at that seam (*i.e.*, how the two are knotted together). The overall shape assumed by a trawl net during the tow results from the entire system of energy distribution between its various panels. So even if the sizes of two adjacent panels are fixed, alterations to their seam can still affect important changes in the shape of the net.

The same can be said of the interconnection between the netting panels and framing ropes at the trawl mouth. Here, something known as the *hanging ratio* of the meshes is crucially important. On a two-dimensional trawl net diagram, two netting panels will appear identical if they feature the same mesh size, the same number of meshes, and are cut along their outer edges according to the same taper ratios. However, during trawling those seemingly identical panels can assume quite different three-dimensional shapes depending on their respective hanging ratios. The hanging ratio of the netting refers to how tightly (or slackly) its meshes are stretched along the particular framing rope(s) to which they are attached. Netting that is not stretched taut along the framing rope, and is instead laced to it at relatively short intervals, will balloon outwards more and will have meshes that open less fully than an identical panel of netting that is stretched out more tautly along the framing rope. So again, when considering the three dimensional shape of a trawl, it is not necessarily the collection of meshes themselves that matter most, but rather the way in which their outer edges are articulated with respect to other parts of the net. Identical panels of netting can take on very different roles in the operative functioning of the net, depending on their precise relationship to the other trawl components.

In sum, when selecting the actual component parts of a trawl net (mesh types and the netting panels in which they are contained), the engineer's decisions are not determined in any strict way by their functional fitness for the task. Functional criteria are in no way absent from this process of selection, but they are by themselves seldom stringent enough to force a decision about which parts to include and which parts to exclude. This is true of both the mesh cells themselves and the various populations that they form within the net. So in a strictly functional sense, the engineer can select neither between the various kinds of netting that might be employed in a trawl, nor between the assorted constellation of panels that might contain such netting. Different configurations meshes and panels might satisfy the trawl net's basic functional requirements equally well.

5.5.2 Arbitrary Factors of Selection

If the physical capacities of different kinds of netting overlap, meaning that they are functionally equivalent at a given location within the net, and if different assortments of panels can be joined to produce the desired shape of the overall net, meaning that they are also functionally equivalent within the net as a whole, than what other (i.e. nonfunctional) criteria are used to select between the designer's many options? From the engineer's standpoint, the answer is that, "Many general characteristics and even technical specifications of the new design may be chosen arbitrarily. Consequently, two different persons may provide different solutions for the same technical assignment" (Fridman 1986, p. 138). In other words, because the mathematical "problem of designing fishing gear can be solved in many ways … therefore, design theory has no fixed, routine solution or recipes and the designer has to be creative in each particular case under these circumstances" (Fridman 1986, pp. 133-4). From the engineer's perspective, the choice of some design elements over others ends up being essentially arbitrary or random.

The apparent randomness of these decisions is certainly valid from an engineering standpoint. But the engineer's perspective is fairly narrow. It is based on criteria that are strictly physical and mathematical. From the perspective of a fisherman (or fishing crew, or fishing firm), the assembly of a trawl net is obviously a far less restricted endeavor. For the fisherman, a successful trawl net does not only do two things—dissipating its internal tensile loads while also balancing its external forces in a desired manner. Instead, a successful net must also fulfill numerous other requirements that we might well call *quasi-functional*. These requirements are not directly related to the physical forces shaping the trawl underwater, and they accordingly fall outside of the engineer's specific realm of technical expertise. Nonetheless, they have important implications for the overall success or failure of the net in its operating environment.

For example, while the primary consideration when designing a trawl net should as Fridman states, be "the behavior of the fish the trawl is designed to catch", there is "still no dependable mathematical model for describing the interaction between a trawl and the fish during the trawling process" (Fridman 1986, p. 152). Fish behavior therefore falls outside of the purview of the engineer. Accordingly, the necessary "Conformity of a given gear to fish behavior and fishing conditions is appraised empirically"—*i.e.*, through the act of fishing—"and the main requirements for the designed gear are usually set subjectively, taking into account fishing experience" (Fridman 1986, p. 134). The desk-bound engineer neither performs these empirical appraisals, nor has the relevant fishing experiences from which to draw wisdom. Rather, the empirical expertise on fish behavior and fishing conditions must come from the crew of the fishing vessel. Accordingly, one of the most important non-mathematical factors that "affects what design features may be incorporated" into a net is "the general knowledge and ability of the fisherman who must use the new gear" (*ibid*). It is the fisherman who often decides which exact components the trawl should include, since he or she is the one with applicable practical experience in trying to capture the target fish. It is precisely because the design specifications of a net rely on each fisherman's personal and therefore "subjective" preferences, Fridman writes, that the design process is often "more of an art than a science" (*ibid*). Such preferences cannot simply be ignored, but neither can they be explained or predicted by the engineer. From the engineering standpoint they are random.

Random though they may be, they still affect the engineer's basic task of selecting components for the net. When a fisherman expresses his aversion to a particular netting material, or his fervent desire for a triple-enforced codend, or his preference for a certain amount of overhang in the wings, these requests immediately make some components more available to the engineer, and other components less available. In a sense, precisely because the choice of whether to use a certain type of polypropylene mesh or a functionally equivalent type of polyethylene mesh is under-determined, its determination relies in large part on the non-rigorous and therefore apparently random preferences of the fisherman.

There are numerous other examples of such "arbitrary" decisions by the fisherman, which make certain components available or unavailable during the assembly of a trawl net. Fish behavior is one thing, but net design is even further removed from the engineer's computational expertise by the fact that any fisherman's net does far more than simply catch fish. It is tightly bound up in active relations with numerous other players and processes in the fishing industry. In other words, fishermen have a great number of other operational concerns that go beyond the mere subtleties—or suspected subtleties—of fish behavior. They must find a way to purchase the net and then make it pay for itself. They must make use of the rigging components and expertise available to them at the time of purchase. They must be able to store the net onboard the fishing vessel during transit to and from the fishing grounds. They must be able to handle it on the confined stern deck of a trawler while fishing, often in heavy seas and under strict time constraints. They must entrust its operation to crewmembers who may have differing levels of experience and skill with fishing nets. They must be able to repair the net as quickly and cheaply as possible in the event that it should tear. And so on. The technical specifications of the finished net, including the exact pieces of netting from which it is assembled, will necessarily be catered to all of these quasi-functional requirements (Fridman 1986, p. 134).

Some of these requirements, it should be noted, could quite clearly be plotted as additional axes in the multi-dimensional space of the netting's functional niche. They follow the same *satisficing* logic of functional limits that we have already encountered. For example, the cost of a particular netting material must fall below a certain maximum price limit if its inclusion in the net is to be economically viable for the fisherman. Likewise, the net's bottom panels may be required to have dimensions smaller than *x* by *y*, ensuring that they are relatively inexpensive to cut out and replace when damaged. These criteria do not require any precisely optimal solution for the final design of the net. Rather, they set a range of acceptable solutions; a zone of tolerance within which the net would accomplish the basic goals set out for it.

By contrast, other quasi-functional factors involved in trawl net design are essentially chance-driven. They are beyond the designer's choosing, and their value cannot simply be selected from some range of acceptable values that was plotted in advance. For instance, whether or not a certain manufacturer has access to a new material like Dyneema—and how much of it—is a factor that lies entirely outside of the fisherman's control. Likewise, the designer has little say over sudden regulatory changes that mandate the inclusion or exclusion of certain components within newly built trawl nets, abruptly making certain design specifications legally unavailable. To the trawl engineer, all of this means that the basic availability of different design elements is characterized by a certain degree of randomness. Specific design elements are quite suddenly made available or unavailable for inclusion in the net, depending on the unpredictable criteria presented by each fisherman as well as other factors beyond even the fisherman's control. The design of any trawl net therefore emerges through an uneasy mixture of lawfulness and luck: compliance with loose functional and quasi-functional criteria, on one hand, and wholly arbitrary decisions defined by stochastic events, on the other.

5.6 Variability and Pattern

As the foregoing discussion indicates, the list of operational requirements for any trawl net can be quite lengthy. It is probably inevitable, then, that these many requirements are not always wholly compatible with each other. The desired characteristics of the net often prove to be mutually antagonistic, in which case "compromise decisions must be taken to handle such contradictions" (Fridman 1986, p. 153). For example, minimizing a net's hydrodynamic resistance and maximizing its fishing power (e.g., increasing the size of it mouth) are incompatible with the goal of minimizing its cost. Accordingly, the finished net, whatever its form, will represent a compromise solution that only partially satisfies these competing aims. Such compromises have, by definition, no unique or optimal solution: they are settlements that may take many possible forms. Any of them may still produce the correct overall equilibrium of external forces acting on the net, and may therefore produce the same general type of trawl.

The self-evidently satisficing quality of such compromise solutions, the fact that they cannot be optimal and so must simply suffice, simply reinforces the point I have already been making. There are many possible solutions to the problem of designing each specific trawl design. Even when multiple nets are assembled for the exact same purpose, the collection of components that goes into each one will almost inevitably show a great deal of variability. As we have seen, the engineer's purely functional criteria are not enough to determine which components should be used. The ultimate assembly of each net therefore depends to a large degree on the essentially arbitrary desires of the fisherman and the equally arbitrary changes in regulations, market conditions, and so on. In those cases where neither the engineer's criteria nor the fisherman's preferences are sufficient to select one component over another, the choice may essentially come down to the flip of a coin. If we imagine five trawl nets, all of them intended for a single fishery, going through this under-determined and stochastic process of assembly, it is clear that we will probably end up with five at least slightly different designs. Such variability emerges quite naturally from the process of assembly itself; it is the direct result of the functional under-determination of trawl components and their stochastic availability.

However, this variability is not absolute either. Net design is not wholly random. After all, the design of each net is based on a number of functional and quasi-functional criteria, even if these requirements are seldom very precise. Furthermore, it is likely that many of the nets assembled for use within a particular fishery will be affected by the same "arbitrary" events, whether these are new fishing regulations, supply shortages of a certain netting material, or the use of similar kinds of fishing vessel. Accordingly, when examining a large number of nets from any particular fishery, we might still expect some design features or combinations of design features to be more common than others. But there is no way of knowing what these particular features are when studying only a single net. Indeed, even the comparison two or three nets is unlikely to clarify which features are common and which features are random. Accordingly, any design commonalities between nets of a certain "type" will emerge only through the examination of a large number of those nets. Typological design information exists at a scale above that of the individual net; it is always a property of a population of nets. An individual net only ever contains partial or fractional information on such commonalities of design.

We can see this quite clearly in one of the only detailed studies that has ever been published on how to distinguish between various types of closely-related trawl nets: Watson et al.'s (1984) *Relative efficiencies of shrimp trawls employed in the southeastern United States*. The authors of the study attempt to identify and describe the most common types of trawl employed by fishermen in the U.S. shrimp fishery in the Gulf of Mexico, eventually distinguishing between flat trawls, balloon trawls, semiballoon trawls, jib trawls, super X-3 trawls, and tongue trawls. The design differences that the authors identify between these types of trawl are for the most part subtle and few in number. Yet, they lead to very significant changes in the operating shape of each net and, therefore, the efficacy with which it captures various species of shrimp and fish. What were these critical design parameters separating one type of trawl from another?

Firstly, and most importantly, there are the cutting ratios of the various netting panels around the mouth of the trawl—both on their exposed "hanging" edges and along their joints or seams with other panels. The authors explain that by subtly altering the cutting rates between the wing panels and jibs, the entire wing can be shifted outwards, causing the mouth of the net to take on a lower and wider shape. The same cutting ratio alterations affect how tensile loads are distributed at the joined edges of the panels, causing the netting to distort more or less under the strains of fishing. Secondly, net types are distinguished by the number of major longitudinal seams they contain—typically either two or four. The presence of more seams increases the vertical spread of the net, making it taller and rounder, while incorporating fewer seams causes the net retain a low, wide profile. Thirdly, the authors show that different types of net use slightly differing methods of attaining the required setback of the lower portion of the net, such that the top of the mouth overhangs the bottom. This setback depends primarily on the relative displacement with which the upper and lower netting panels are joined (i.e. the position of the tope panels in relation to the bottom panels). Fourthly, the hanging ratio of the netting panels around the trawl mouth affect how much the netting is free to balloon outwards during the tow. And finally, the shape of the net's bosom (i.e. the cutting ratios along the front of its square), which will determine whether or not it can employ an additional bridle at the center of the headline.

Crucially, none of these parameters has anything to do with the technical specifications of the netting itself. Rather, they affect the collective shape of the panels' edges and the ways in which they are joined to the other panels and the framing ropes. Just as we might have predicted, the detailed specifications of the mesh cells are relatively unimportant, as are the number of panels employed to a achieve the desired overall shape. In a tongue trawl, for instance, the characteristic "tongue" of netting that juts forward from the bosom can be constructed either by adding an additional netting panel to the front of an existing trawl, or by retaining the same number of panels and integrating a tongue into the shape of the existing square. In each case the number of panels constituting the tongue, as well as their shape, differs. But the resultant shape and efficiency of the overall trawl is largely the same.

Arguably the most important lesson of Watson et al.'s study, though, concerns the amount of effort required for its completion. The authors introduce their work by emphasizing how rare such overview studies of trawl designs are. They attribute that paucity to the complicated and time-consuming nature of the work, pointing out that their own study was made possible only by their extended participation in another NOAA project that required working with a very large number of commercial trawl nets over a period of several years. In addition to this, the completion of their study required the use of scuba divers to take underwater measurements of another one hundred trawl nets during towing. So it was only after working with and observing a very large number of nets in action that the general patterns of their designs became apparent. And though the authors were ultimately able to distinguish "eight basic designs of the otter trawl used in the southeastern United States shrimp fishery" (Watson et al 1984, p. 8), those types were discerned only with great difficulty from a much greater diversity of net designs characterized by "innumerable" different modifications and rigging arrangements. By recognizing the immense diversity of designs, the authors are not wavering on their conclusion that eight overall trawl types exist. But they are conceding that, amid such variation, the defining features of each type emerged and became clear only through a very large number of observations.

This brings us back to the central paradox of trawl net design: the existence of different types of net that, for all their obviousness, do not allow any one net to be conclusively distinguished from any other. We have already seen that many different combinations of netting components might produce the same overall physical effects—drag, shear, buoyancy, and weight—and therefore constitute the same general type of trawl. Yet for this very reason, none of the individual parts are necessarily diagnostic of that overall pattern. The characteristic features of a certain type of trawl can therefore never be diagnosed by examining a single net, or even a small number of nets. A trawl net type, if it exists, will necessarily be characterized by shared overall traits or tendencies that emerge as commonalities between multiple, fully-assembled nets. Trawl net types can therefore be recognized only through repeated observations or examinations of individually assembled nets. Distinctions between types emerge slowly, in the form of shared features that reappear with a relatively high frequency amid the "arbitrary" thicket of individual design variations. In this sense, types might be distinguished with relative ease when looking at a large number of assembled nets together, but it is virtually impossible to identify a type of net through the examination of a single exemplar or, especially, through the examination of a select number of its components.

What kind of object is a trawl net, then? What can we say about the relations between its parts and the whole? We are clearly not dealing with a forensically reconstructable object in the mold of Cuvier's skeletons, Clements' vegetational units, or Simondon's highly concretized technical objects. A trawl net's parts are never so finely honed that they could be other than the way they are. We are attempting to study something else, but what is it? What can we know about it? What should we name it? As it turns out, we do not need to come up with a name, for the kind of object embodied by trawl nets already has one. Indeed, it has been assiduously described, studied, and debated for nearly one hundred years. To understand this requires that we return to the early years of plant ecology and the man whose work would eventually overturn the Clementsian notion of vegetation: Henry A. Gleason.

5.7 The Gleasonian Dissent

The story of Henry Gleason and Frederic Clements is told and retold among geographers. The chiasmatic perfection of the two men's professional trajectories (a striking divergence of fortunes made all the more interesting by their intense personal dislike for one another) has made their story into a perfect cautionary tale, for new geography students, about the dangers of academic hubris and dogmatic devotion to predetermined research agendas. Given the great amount of attention their tale has garnered over the years, I will review it only briefly (for full overviews, see McIntosh 1975 and Nicolson 1990).

Gleason was for many years the only real thorn in Clements' side. As Nicolson (1990, p. 93) puts it, "Between 1917 and 1945 only one major ecologist in the whole of America dissented from the general consensus surrounding the reality of [Clementsian] vegetation-units—Henry Allen Gleason." Like an embedded thorn, Gleason's needling critique of Clementsian ecology was not taken seriously at first, but became increasingly irritating over time and, after years of slow ferment, led to the outright demise of the established Clementsian order. The process began in 1917 when, only months after the celebrated release of the Clements' magisterial *Plant Succession* (1916), Gleason published a comprehensive retort to that work. A writer and thinker of unparalleled economy, Gleason dismissed the entire Clementsian system of vegetation science in a pithy and well argued article of only 18 pages. His argument, nonchalantly titled "The structure and development of the plant association" (Gleason, 1917), was concise and well reasoned but did not prove to be popular. In fact, the Gleasonian "dissent," as it would eventually become known, was initially ignored by the great majority of ecologists, for whom *Plant Succession* seemed destined to unify and redefine the entire discipline.

Gleason's argument therefore languished in obscurity as the Clementsian system quickly gained adherents, becoming in the span of only a few years the *de facto* paradigm for all landscape ecology research in North America. Gleason attempted twice more to make his counterpoint heard, presenting and publishing slightly modified versions of his paper in 1926 and again in 1939—both times under the more openly combative title "The individualistic concept of the plant association". However, in response to Gleason's insistence, the academic community's lack of interest in Gleasonian ideas simply gave way to widespread criticism and even ridicule (Whittaker, 1962). A botanist by training and by passion, Gleason responded to the early disinterest and increasing hostility of his peers, as well as what he saw as the errant trajectory of ecological research, by abandoning the discipline altogether for nearly thirty years. In 1918 he left his academic post at the University of Michigan and accepted the offer of a well-paid research position at the New York Botanical Garden. There, at the behest of the Garden's director, he refocused his intellectual attention on the botany and taxonomy of South American plants.

Even as a botanist, however, he occasionally presented his critique of Clementsian vegetation science at academic conferences for ecologists. By the early 1940s this began to pay off: his work gained a sustained (if limited) level of interest from some ecologists. Clements, meanwhile, had begun to lose his dominance over North American ecological research. His time was increasingly spent on the construction of a convoluted terminological labyrinth that might somehow reconcile his grand ecological theory with the steady stream of field-based evidence that seemed to contradict it. Through Clements' prodigious neologistic efforts, the organismic notion of plant formations became increasingly intricate, bloated, and arbitrary over the years. With his rival's credulity thus strained to the breaking point, Gleason's comparatively simple and elegant ideas began to catch on in the late 1940s. By the mid-1950s a remarkable paradigm shift had taken place: Clements' once-glorified ideas were abandoned *en masse* by ecologists and Gleason's once-reviled individualistic concept became the accepted standard for all modern ecological research.

5.7.1 A familiar paradox

Gleason's disagreement with Clements stemmed from what he saw as a universal—if seldom remarked upon—paradox of plant formations. On one hand, Gleason acknowledged, the presence of such formations on the landscape seemed an incontrovertible matter of fact: "Of the actual existence of definite units of vegetation there is no doubt", he began his 1917 article:

> That these units have a describable structure, that they appear, maintain themselves, and eventually disappear are observable facts. That to each of these phenomena a definite or an apparent cause may be assigned is evidenced by almost any piece of recent ecological literature. (Gleason 1917, p. 464)

The importance of this basic observation, to Gleason, is underscored by his decision to begin every version of his paper with a similar declaration. He began his 1926 paper by declaring that "Plant associations exist; we can walk over them, we can measure their extent, we can describe their structure in terms of their component species, we can correlate them with their environment, we can frequently discover their past history and make inferences about their future" (Gleason 1926, p. 8). In 1939 he opened his argument by restating that "an association, or better one of those detached pieces of vegetation which we may call a community, is a visible phenomenon" and insofar as it can be measured, mapped, surveyed, and photographed, "a very tangible thing" (Gleason 1939, p. 103). When it came to the existence of discrete areal patterns of vegetation on the landscape, he was therefore in total agreement with Clements.

On the other hand—and here the paradox becomes apparent—Gleason noted that it was impossible, in practice, to clearly and consistently distinguish one such pattern from another. He argued that no matter how thorough the observation and no matter how accurate the description of a distinct area of vegetation, "The great mass of ecological facts revealed by observation and experiment may be classified in different ways … which differ widely in their meaning or even in their intelligibility" (Gleason 1917, p. 464). There was, in other words, no single correct way of classifying the observed characteristics of a plant association, no definitive list of the plants that it was required to contain. Indeed, Gleason pointed out that "an area of vegetation which one ecologist regards as a single association may by another be considered as a mosaic or mixture of several, depending on their individual differences in definition" (Gleason 1926, p. 10). So while different types of plant association clearly existed across the landscape, it was not at all clear what the actual differences between them were. There was no satisfactory way of differentiating one from another. This, of course, is the same contradiction that, as we saw above, hinders the comparative study of trawl net designs.

Gleason's "essential point" was to emphasize two things: firstly, "that precise structural uniformity of vegetation does not exist and," secondly, "that we have no general agreement of opinion as to how much variation may be permitted within the scope of a single association" (ibid). In other words, because the internal structures of plant associations were heterogeneous, it was usually impossible to say where any one plant formation stopped and the next one started. Without homogenous blocks of abutting vegetation, it was not possible to draw clear boundaries between one plant formation and the next. To illustrate this, Gleason used the following example. If many different ecologists were permitted to study two patches of alluvial forest, one along the upper Mississippi River and another along the lower Mississippi River, they would all quite assuredly assign the two forests to different plant formations. The differences between their structures (i.e. their distinct species compositions) would be unambiguous. Yet if the same ecologists walked from the headwaters of the river downstream to its delta, they would likely all choose different locations for the exact boundary between the upper and the lower alluvial forest formations. Because vegetation is not structurally uniform, there is no indisputable place where one type suddenly gives way to another. "This diversity in space," Gleason writes, "is commonly overlooked by ecologists. … Yet it makes difficult the exact definition of any association-type, except as developed in a restricted locality, renders it almost impossible to select for study a typical or average example of a type, and in general introduces complexities into any attempt to classify plant associations" (Gleason 1926, p. 15).

Gleason recognized with greater clarity than any of his peers that this paradox (the existence of differently patterned structures on the landscape that, upon closer examination, can never be differentiated) spelled failure for Clementsian ecology, which relied entirely on the existence of structurally integrated, easily identifiable vegetation units. Gleason argued that, given the innate structural variability of plant associations, it was clear that their component species were not tied together by any sort of overarching functional or structural coherence. There was no overall plan or blueprint defining some optimal assortment of plants in any given area. Rather, plant communities were always at least marginally different from one another, always heterogeneous in relation to themselves, always to some degree unclear around the edges. And, his argument went, if there was no reliable or definitive way to separate one association from another, how could they be said to exist as discrete units, assembled in an organismic manner?

Rather than simply swallowing this paradox, Gleason offered a radically different solution. "It is small wonder," he mused,

> that there is conflict and confusion in the definition and classification of plant communities. Surely our belief in the integrity of the association and the sanctity of the association-concept must be severely shaken. Are we not justified in coming to the general conclusion, far removed from the prevailing opinion, that an association is not an organism, scarcely even a vegetational unit, but merely a *coincidence?*
> (Gleason 1926, pp. 15-16)

This suggestion was wholly repugnant to most ecologists. For if—as Gleason himself admitted—different plant associations existed, if common structural patterns were repeated quite obviously on the landscape, how could they then also be coincidental? Was not their very existence proof of some organizing principal that went beyond mere chance? Wasn't the whole point of ecology, as an academic discipline, precisely to explain such patterns *without* attributing them to sheer chance?

5.7.2 Functional Individualism

Gleason rejected this dualism, showing that if plant establishment were simply understood to be an *individualistic* phenomenon, then vegetation patterns could be coincidental and scientifically lawful at once. His notion of individualism held that the germination, establishment, and growth of any particular plant depended only on its individual physiological tolerance to the environmental conditions around it. Every plant was in this sense a selfish individual: if it found the physical conditions in an area to be tolerable it would grow there, quite independently of what other plants in its vicinity did or did not do. To prove this point, Gleason reminded the reader that even seeds kept in a state of total isolation from other plants could quite easily be coaxed into growing:

> These seeds will germinate between folds of paper, if given the proper conditions of light, moisture, oxygen, and heat. They will germinate in the soil if they find a favorable environment, irrespective of its geographical location or the nature of the surrounding vegetation. Herein we find the *crux* of the question. The plant individual shows no physiological response to geographical location or to surrounding vegetation *per se*, but is limited to a particular complex of environmental conditions, which may be correlated with location, or controlled, modified, or supplied by vegetation. If a viable seed migrates to a suitable environment, it germinates. (Gleason 1926, p. 17)

The individualistic concept therefore maintained that there was no unifying logic to vegetation patterns above the level of the individual plant. A plant would quite simply grow wherever it was able to germinate. Vegetation patterns on the landscape were not the manifestations of some preexisting structural plan, but merely the sum of many such individual germinations.

Clements was right to perceive Gleason's individualistic concept as a threat. For if vegetation patterns really emerged in such a straightforward manner from basic botanical phenomena, then the extraordinary intricacy and precision of the Clementsian system—the delicately interleaved machinery of formations, associations, consociations, faciations, and lociations—could be sliced away more or less in their entirety by Occam's Razor. Their replacement, the basic physiological tolerance of a plant to its environment, was not only simplistic but openly imprecise. For although Gleason asserted that "the functions of the individual plant demand a proper environment for their operation", he also pointed out that the properness or improperness of that environment was only ever a matter of degree (Gleason 1917, p. 465). Any given species might, for example, establish itself in very different environments, sprouting from the waterlogged and heavily shadowed forest floor in one location, and from a sun-baked crack in a granite cliff at another location. So too, at a single location, the plant's environmental conditions would clearly change from season to season and from hour to hour. Gleason concluded from this that "each individual is capable of existing under a variety of environmental conditions", and that there was accordingly no unique set of conditions required for the growth of any given plant species. Rather, each was tolerant of a range of different conditions.

Gleason argued that this physiological or functional flexibility of the individual plant had a number of important implications for plant associations. On one hand, it clearly meant that the individuals of a given species might be associated with many different assortments of other plants, depending on the precise environment in which they were growing. In other words, one plant might well be involved in several different plant associations. It also implied that, since any number of different species might find a certain environment conducive to establishment and growth, any member of a plant association could likely be replaced by another that was functionally equivalent. So a given association might persist even as one of its component species disappeared and was replaced by another. Finally, it meant that the wholesale disappearance of any plant species would only result from "an effective change of environment beyond the range of demand of any individual"

(Gleason 1917, p. 475). Changes in the functional environment could certainly "produce also a variation in morphological structure", but only if those environmental changes were numerous or severe (Gleason 1917, p. 465). The total collapse of a given plant association would occur only under extreme conditions, when the physical limits of its component species were superseded.

Decades after Gleason first articulated these notions, the ecologist George Evelyn Hutchinson (1957) coined the term "fundamental niche" to describe them. Hutchinson mathematically formalized the notion of the fundamental niche as a unique set of functional limits for each species that would circumscribe a unique hypervolume in multidimensional factor space (just as in Figure 5.3). Visualized in this way, any species could survive in a habitat whose environmental conditions, when plotted, fell within the confines of its functional niche. Hutchinson's paper garnered a great deal of attention and was a groundbreaking step in the development of today's rigorously mathematical ecology. Yet it should be remembered that the iconic functional hypervolume at the heart of his paper was merely the mathematical expression of a concept that Gleason had been verbally articulating for some forty years already.

5.7.3 Stochastic availability

Because different species' ranges of tolerance (i.e. fundamental niches) could overlap, Gleason maintained that purely functional criteria were always insufficient to determine which plant would end up as part of the vegetation in a given area. No plant fulfilled a unique functional role within the broader plant association. Each was in some sense replaceable. What, then, did account for the establishment of certain plants in a given area, rather than other plants that were functionally equivalent? Gleason's answer could be boiled down to one word: luck. He wrote that the establishment of any species in a new area required the germules (i.e. seeds) of that species to be readily available as colonizers. This, in turn, required a nearby population of the species in question. Furthermore, germules from that nearby population needed to be transported by their migratory mechanism—wind or water or animals—to the right place at the right time. This meant that if two populations of different potential colonizer species both exist within migration range of a newly denuded area, the "choice" of which plant will become established may well come down to a single errant gust of wind or the random path of some large mammal. Colonization depends on the availability and dispersal of seeds, and those factors are most often stochastic.

According to Gleason, the structure of any given patch of vegetation was therefore determined by a mixture of lawfulness (i.e. each plant's limits of environmental tolerance) and chance (i.e. each plant's availability). Adjacent areas of vegetation might support non-identical assemblages of plants "because of differences in the surrounding plant population, from which the inhabitants of an area are drawn; because of accidents of migration and the time available for it; and because of environmental differences" that determine what can and cannot survive there (Gleason 1917, p. 473). It is worth noting that nearly one hundred years later, this combination of lawfulness and stochasticity is still widely agreed to be the defining feature of ecological community assembly, whether expressed in theoretical terms such as Roughgarden's (2009) mixture of "supply-side" and "interactional" ecologies, or in the more empirically and mathematically rigorous terms of Tilman's (2004) 'stochastic niche theory'. Ecological communities are compelling to researchers precisely because of their puzzling under-determination: the unpredictable dynamics and structures that result from their inherent blend of lawfulness and contingency (Putnam, 1994).

Together, the joint phenomena of niche overlap and stochasticity (useful terms that, to be clear, were not used by Gleason) meant that plant associations would virtually never take the form of discrete or homogenous units. Rather, different associations would blend more or less seamlessly into each other, a function of their slowly changing environmental conditions over space and their ever-changing distance from various sources of seed. Amid such continuous structural variation, the classification of different associations could only be an arbitrary exercise:

> Whether any two areas, either contiguous or separated, represent the same plant association, detached examples of the same one, consocies, or different associations,

> and how much variation of structure may be allowed within an association without affecting its identity are purely academic questions, since the association represents merely the coincidence of certain plant individuals and is not an organic entity in itself. (Gleason 1917, p. 473)

From this it followed that no one stand of vegetation could be inherently more representative of a certain association-type than any other stand. If every observed stand was a blend of different association types, then all stands were equally impure or non-representative. It was not logically possible for one collection of plants to represent a more perfect manifestation of an association type than any other. As Gleason wrote, the individualistic concept of plant associations

> leaves us unable to recognize any one example of an association-type as the normal or typical. Every association of the same general type has come into existence and had its structure determined by the same sort of causes; each is independent of the other, except as it has derived immigrants from the other; each is fully entitled to be recognized as an association and there is no more reason for regarding one as typical than another. Neither are we given any method for the classification of associations into broader groups. (Gleason 1926, p. 19)

Of course, by refuting the existence of any ideal structural template for a given association-type, and by arguing that every observed version of an association is as valid or representative as any other, Gleason was in fact casting doubt on the viability of the term "association" itself. Insofar as the word implied a certain typological ideal, a climax formation in the Clementsian sense, association clearly meant something other than what Gleason intended. He therefore came increasingly to use the term "community" instead—a steady lexical shift that becomes obvious when comparing the three major versions of his paper on plant individualism.

5.7.4 Community and classification

Community, the term that Gleason increasingly used instead of association, did not refer to an abstract or idealized set of interrelated species, but rather to the actual, tangible collection of plants that an ecologist had observed in the field. For Gleason, the biological community, or *biocenosis*, differed from the Clementsian association in that its "slight irregularities" were simple matters of fact, rather than aberrations from the norm (Gleason 1939, p. 104). Gleason argued that every community was by definition unique or aberrant, since each was "the product of its own independent causative factors"—factors that not only varied continually across space but were often stochastic as well. Accordingly, even if a given community exhibited a degree of overall uniformity "sufficient to enable us to recognize it as … a unit of vegetation," its structure was also sufficiently random that it could not be considered "a definitely organized unit" that conformed to some preexisting structural plan (ibid).

We might, for example, imagine a distinctive unit of vegetation amid a landscape of balsam fir and white birch forest—say, a community of black spruces and eastern tamaracks growing in a large, boggy depression. The general structure of such a community, its distinctness from the surrounding vegetation, would be visible to even the untrained eye. Still, that distinct unit of vegetation would not contain any perfectly predictable and consistent mix of species. Inevitably, its species composition would differ from the species compositions of other, nearby bogs. So too, its own mix of species would exhibit considerable internal variation from one side of the bog to the other. None of the bog communities in such a landscape—nor one side or the other of a single bog—could be labeled as somehow more typical or more bog-like than the others. None of them could be considered as representatives of some transcendent boreal bog association, whose structure they were striving to duplicate. Rather, if various bog communities display structural similarities it is because they have similar environmental conditions and draw their germules from similar surrounding populations of parent plants. They end up looking similar because their conditions and processes of assembly were similar, not because of some idealized model or plan according to which they are assembled.

To reiterate, Gleason was not denying the existence of such distinct patterns of vegetation on the landscape. Associations were not fictitious, and he was not claiming that to perceive them was to fall victim to some kind of illusion. Rather, he simply sought to point out that biological communities, in their fantastically irrepressible heterogeneity, "introduce many difficulties into any attempt to define or classify association-types" (Gleason 1926, p. 21). His notion of functional individualism was emphatically not an argument for some alternate and superior method of classifying plant communities, but a condemnation of the pursuit of classification itself. What was wrong with the Clementsian system was not the exact pattern of its assembled jigsaw pieces, but rather its overzealous attempts to achieve typological rigor of any kind. Gleason insisted that the classification of any community was a problem for which there could be no optimal or exclusive solution: a puzzle, built like an interlaced gossamer of overlapping and interconnected webs, whose pieces were multiply and inexpressibly related and could therefore never be articulated by a definite set of boundaries. Instead, communities were in his view structured through something very like Wittgenstein's (1998) 'family resemblance': While their overall resemblances were often undeniable, none of their shared features could be singled out as necessary or sufficient criteria for their inclusion in a particular class.

As a simple example of family resemblances (Figure 5-4), we might imagine that an ecologist samples some Community A and finds it to contain plant species *w*, *x*, *y*, and *z*. The ecologist then samples Community B and finds it to contain species *v*, *w*, *x*, and *y*. Because the two communities have species *w*, *x*, and *y* in common, it would seem logical to classify them as belonging to the same kind of plant association. Their resemblance is unquestionable. However, what if the ecologist goes on to sample Community C—finding that it contains species *u*, *v*, *w*, and *x*—and Community D—

finding that it contains species *x*, *y*, *z*, and *a*? Each of the newly sampled communities shares three of its respective species with one of the original communities: C shares species *v*, *w*, and *x* with B; while

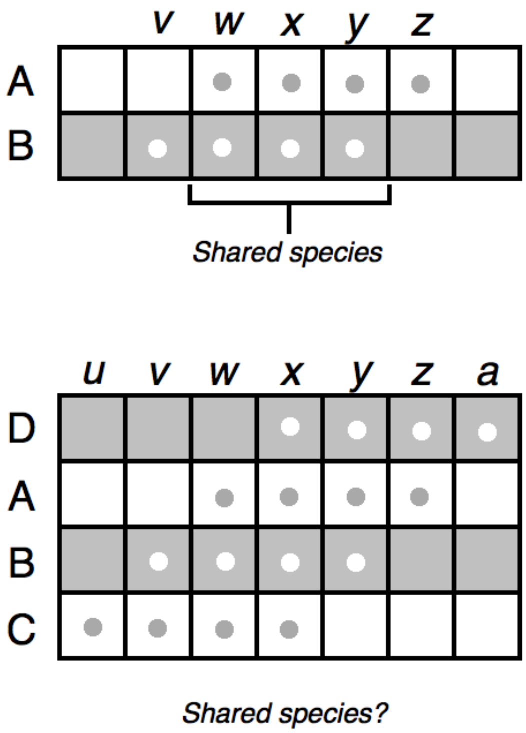


Figure 5.4 Family Resemblances: On their own, communities A and B (top) seem to clearly constitute a single type, since they share 3 of their 4 respective species. But when communities C and D are considered as well (bottom) it becomes unclear which communities should be grouped together into types. It is equally valid to group A with either B or D (and B with D or C).

D shares species *x*, *y*, and *z* with A. Accordingly, C might logically be classified into the same association as B, and D into the same association as A. This makes perfect sense. All of the communities are now classified as a single kind of association. Yet this solution also makes no sense. For, the two new communities have only species *x* in common, clearly suggesting that they should not be classified into the same association. Even though each of them resembles one of the original communities to the same degree that the original communities resemble each other, it is obvious that the two new communities scarcely resemble one another at all. Clear resemblances link the four communities together, but there is no correct way in which we can divide them into classes of resemblance. Community A might just as “correctly” form a class with Community B as with Community D. Community B might be logically paired with Community C or just as logically with Community A. This illustrates Gleason’s fundamental point about the Clementsian ecological project: There were innumerable ways in which any group of communities could be said to resemble and not resemble each other, depending on how the ecologist chose to sort out his or her data. Communities formed a continuous and indivisible spectrum of closely related structures that, despite their readily apparent similarities and differences, could never be cleanly divided into discrete, natural groupings.

Classification was therefore a lost cause, and one which Gleason was quite unwilling to pursue. Staunchly opposed to the taxonomic pretensions and precisions of Clementsian ecology, and wholly convinced that vegetation structure was assembled through individualistic mechanisms, he found it unnecessary to dwell on the precise nature of the relationships that might connect the component species of a community. Nor, quite frankly, did he need to dwell on them. To overturn the Clementsian orthodoxy, it was sufficient for him to point out that, due to the phenomenon of niche overlap, there was no *strictly necessary* relationship between any of a community’s component species. And if the various species within a community were not bound to each other by strict necessities, then their relationships were neither lawful nor, for that reason, scientifically intelligible. As a result, Gleason’s work tells us very little about the ways in which the assorted populations within a community interact.

5.7.5 Whittaker’s novel idea

This unyielding stance against community classification—and its attendant dismissal of interspecies relationships—would be softened only in the 1950s, when the individualistic concept was significantly advanced and its validity conclusively proven by Gleason’s intellectual successor: the gifted ecologist Robert Whittaker. Whittaker did not set out to prove Gleason’s theory correct. His dissertation work was designed to test a different, considerably less stochastic, theory of community assembly in which co-adapted groups of species would occur in successive bands across the landscape. However, Whittaker's data led him to reject that hypothesis. They showed, instead, that the abundance of each species formed a standard Gaussian "bell" curve when plotted against key environmental gradients such as moisture and temperature. This was incontrovertible proof that each species distributed itself in direct relation to the environmental conditions—quite independently of the distributions of other species. In a word, Gleason was correct. Whittaker's research into environmental gradients was therefore responsible, more than any other development, for the wholesale abandonment of Clementsian ecology during the early 1950s and the rapid shift of ecological research toward Gleasonian notions of plant individualism.

But unlike Gleason, Whittaker did not entirely cast aside the notion of classification. Instead, he suggested that the primary error of taxonomically inclined ecologists, both in North America (in which case they typically adhered to the classification scheme of Clements) and in Europe (in which case they likely followed the system of Josias Braun-Blanquet), was simply one of degree. In 1962, he published an astoundingly comprehensive, book-length monograph on the classification of natural communities that, in its acuity and its breadth, effectively united the individualistic North American branch of ecological study with its more traditional and taxonomic European counterparts (Whittaker, 1962). His argument in this work was not that individualistic ecologists (i.e. those focusing on individual plant dynamics) were in the right while taxonomically inclined ecologists (i.e. those focusing on the patterned outcome of those dynamics) were in the wrong. Rather, he made a more nuanced suggestion: Taxonomic ecologists simply conceived of interspecies relations in a manner that, in most cases, was too totalizing and strict. On the other hand, he sought to show that Gleasonian ecology, for all its use of the term *individualism*, did not have to imply that organisms are entirely isolated individuals that do not interact with each other. The true character of interspecies relations was somewhere in between these two views.

Whittaker's analysis began with a basic clarification of the term *association*. He pointed out that the term was frequently used with two distinct connotations: "One may distinguish association in the distributional sense, of relative similarity of distribution, from association in the dynamic sense, of actual interaction between species" (Whittaker 1962, p. 97). While acknowledging that both understandings were valid, Whittaker stated that the dynamic form of association must be considered the primary one, since the areal distributions were themselves clearly the *results* of the ongoing dynamic processes (a claim that few ecologists would refute). Therefore, Whittaker continued, any study of associations must be grounded in an understanding of the dynamics (i.e. interactions) that take place between organisms of different species.

His major insight, in regard to such encounters, was to recognize that different degrees of dependence might characterize them. One species' presence in the community might be entirely dependent on the presence of another particular species with which it interacts. Or the level of dependence between them could be slightly less, such that the first species' presence is simply made very likely by the presence of the second. Or again, the presence of the second species might make the presence of the first only marginally more likely. The presence of the second species could even have a repulsive effect, making the presence of the first species *less* likely. Whittaker carefully considered five such degrees of dependence: (1) Full dynamic association, (2) partial dynamic association, (3) indifference, (4) partial dynamic dissociation, and (5) full dynamic dissociation.

Full dynamic association described the relationship between two species that could each exist only in the presence of the other, such that the removal of one species would necessarily cause the disappearance of the other. Full dynamic dissociation described the opposite relationship: two species with an entirely antagonistic relationship, such that the presence of one would implicitly rule out the presence of the other. Such uncompromising relationships, in which the mere presence of one species would make the inclusion or exclusion of another absolutely mandatory, were as Whittaker pointed out the bread and butter of ecological classification schemes. And with good reason. Their all-in or all-out approach vastly simplified the taxonomic ecologist's task of distilling the mash of different species on a landscape into a number of discrete association types or climax communities. Hence, the balsam fir, in its climax state, would always occur with white birch to form the balsam fir-white birch association. The hickory, in its climax state, would always occur with woodland oak species to from the hickory-oak association. Supposedly, each species belonged exclusively to one type of climax vegetation, and *always* occurred with a certain set of other species, making the unambiguous classification of those climax communities possible.

Yet, the problem with such "full" interspecies dependencies was that they were exceedingly rare in nature. Whittaker argued that there were very few imaginable scenarios in which two species were absolutely required to inhabit a certain area together, or, conversely, in which two species could in no way eke out any kind of existence in each other's vicinity. In fact, he could think of only two possible ecological relationships that might be characterized by full dynamic association: the case of a parasitic organism that is fully and exclusively dependent on a single species of host organism; and the case of two species whose relationship was fully and exclusively mutualistic, such that they are wholly dependent on each other—and only each other—for food, shelter, protection, or procreation. While Whittaker did not deny the possibility that such one-dimensional ecological relationships existed somewhere in the world, he pointed out that their occurrence in any ecosystem is clearly the exception rather than the rule. At least they were possible, however. When it came to full dynamic dissociations, pairs of species that, like matter and anti-matter, could absolutely never coexist within the same region, Whittaker was utterly stumped. He could think of no pertinent examples at all.

Nor were most species wholly indifferent to the presence or absence of other species in the same community. Whittaker wrote that such indifference was certainly possible, as with "The herbs of a forest floor," which "may be dependent on the existence of a forest canopy and soil; but [are] not dependent upon, or necessarily associated with any particular species of canopy tree" (Whittaker

1962, p. 99). Conceivably, this kind of neutrality might characterize any set of species that coexisted in a certain community without ever directly interacting. Yet, the majority of living things in any ecosystem clearly tended to interact with one another. Seldom did they live in total isolation. To explain their interactions, the ecologist was left with only two possible degrees of dependence: partial dynamic association and partial dynamic dissociation. These partial associations, Whittaker wrote, necessarily described the vast majority of interspecies relationships tying together any community.

Partial dynamic associations were those in which "species B provides some need of species A, but other species can also supply this need" (Whittaker 1962, p. 97). This dynamic described most trophic interactions, those nutritional exchanges in which the sun's energy was redistributed among primary producers (chlorophyllic plants), primary consumers (herbivores), secondary consumers (carnivores, omnivores, or scavengers), and so on. A golden eagle, for instance, could depend on white-tailed jackrabbits for food. But it might also fulfill its trophic needs by preying on a different species—say, black-tailed jackrabbits. Likewise, if neither jackrabbit species is available the eagle might feed on a hoary marmot, a young mule deer, a red fox, or a newborn lamb from some pasture. The golden eagle is clearly dependent on each species, but it is not wholly dependent. Its dependence is a matter of degree. That degree, as Whittaker sought to illustrate, can vary from one particular relationship to the next:

> A parasite may utilize as hosts several related species; a plant-eating insect may feed on a few species, closely related or taxonomically scattered, or may graze on almost any available plant regardless of species. Vertebrate herbivores may feed with little regard for species as such, though with selection for palatability and with preference for some larger groupings—as for grasses, for forbs, or for shrubs. Predators probably in general take available prey without regard for species as such, though with limitations of size, possibility of capture, and sometimes

> palatability. A similar lack of species restriction, within generally suitable food properties, appears among many scavengers and saprophytes. A range of degrees of partial dynamic association may thus be observed… (Whittaker 1962, p. 98)

Some species might depend for their survival on a very small number of other species, three or four sources of nutrition, upon which they are therefore quite highly dependent. Others, however, might depend on a large number of highly substitutable species, in which case their dependence on each one "grade[s] into indifference" (Whittaker 1962, p. 99). In either case, the important fact is this: a certain trophic distribution of energy occurs between the component species of a community, yet that energy distribution can usually be replicated even after one of the species is substituted by another.

Partial dynamic *dissociation*, on the other hand, described interspecies relationships that were characterized by a certain animosity or hostility: those in which the presence of one species would to some degree deter the establishment of another particular species in the same area. Whittaker recognized competitive relations as the most "familiar examples of partial dynamic dissociation" (ibid). Interspecies competition was not an example of full dissociation because it did not result in a landscape inhabited solely by individuals of the "winning" species, to the complete exclusion of the "losing" species. Rather, it meant that for any two competitor species A and B, "in the range of environments more favorable to A than B the population of B is reduced, and in environments more favorable to B than A, A is reduced" (ibid). Given the correct environmental conditions, the presence of a superior competitor A would make it more *difficult* for the inferior competitor B to inhabit a given locale, but would not make it impossible. Indeed, as we have already seen, the potential habitability of a landscape was individualistic: a function of the environmental tolerance of the individual organism. But competition could lead to a reduced population of one species simply by making fewer resources available to it. In such a situation, the presence of individuals of the "loser" species B would simply be made *less likely* by the presence of individual organisms of species A. Neither partial associations nor partial dissociations would determine the absolute presence or absence of the species within a particular community. Rather, such partial dependencies affected their relative population sizes.

The notions of partial association and partial dissociation led Whittaker back to Gleason's paradox: the seemingly contradictory fact that structural patterns in plant associations were both entirely obvious and wholly ineffable. The paradox, Whittaker explained, resulted from the mixture of functional individualism, on the one hand, and partial (i.e. non-choosy) dependencies on the other. The members of a biocenosis were independent of each other insofar as their own physiological capacities determined whether they were suitable inhabitants of a given functional environment. But they were also related to each other insofar as they could only partake in the ecosystemic distribution of energy with certain species, and not with others. They were both related and unrelated. Whittaker wrote that

> This answer may be offered to the paradox at the heart of the association problem: Species populations are distributed individualistically, but within a web of interactions; independently in the sense of distributional diversity, but not independently in the sense of unrelatedness. [Therefore] the paradox is not a contradiction. (Whittaker 1962, p. 101)

What Whittaker was trying to formulate was something very like DeLanda's (2006) exteriority of relations. The components of a community were independent of one another in the sense that they were all characterized by their own unique sets of capacities. They were not made for and only for their particular role in a particular community. At the same time, however, within a given functional whole they were necessarily bound together and to some degree dependent on the surrounding components with which they made contact.

In sum, the biocenosis was a bounded yet underdetermined meshwork of individualistic parts immersed in partial dependencies. Recognition of this led Whittaker to a novel idea. If associations between species were only partial then there was, of course, no guarantee that any particular community would reveal those associations. A single stand of vegetation, when observed, was unlikely to produce reliable information about the full extent of the possible associations between its component species (and with other species that may not be present). But if a sufficiently large number of such stands were sampled, those associations would almost certainly begin to emerge statistically. For instance, when sampling only a handful of stands, two species that never occurred together in a single stand would rightly appear to have no association. But if the degree of association between them were 10% then one could expect, after sampling one hundred different stands, to find the two species growing together in ten of those stands. A weak pattern of association would gradually begin to emerge, over many iterations. Accordingly, structural patterns might appear with perfect clarity when looking at a large number of stands across the landscape, but were reduced to something fractional and incoherent in any single stand. Whittaker referred to this as "the fragmentary nature of [individual] observations," and argued that because of such fractional coherence (to modify a term used by John Law [2002]) it was "reasonable to emphasize the *process* character of that which is observed, and to regard the community as not so much an object as an *event*" (Whittaker 1962, p. 104). The study of biological communities was by necessity a perpetually ongoing endeavor, since "The species composition of a stand and its relation to habitat are best understood not through the study of that stand alone, but in relation to other stands in the landscape" (Whittaker 1962, p. 107). While full dynamic associations can be comprehended perfectly through a single observation, the fractional coherence of partial associations only becomes legible through repeated and ongoing iterations.

5.7.6 Clusters instead of classes

This iterative understanding of associational clarity meant that the work of distinguishing associations was an interminable task. To fully discern all of the associations between species would require sampling an infinite number of stands. For this reason, Whittaker wrote, "The living community itself is … beyond total knowledge and complete representation. It is … clearly impossible to determine and represent all the interrelations among [the] organisms [in a community]" (Whittaker 1962, p. 104). The impossibility of this task was compounded by the fact that vegetation patterns on the landscape were, in their "full detail, exceedingly complex" (Whittaker 1962, p. 109). Because of this complexity, perpetually finer details could be expected to emerge as the number of observed communities increased. This level of minute detail, and its increasingly fractional character in any one stand, meant that

> It is generally impossible to interpret adequately the relations of species and stands to one another and the landscape by observation alone. It is consequently necessary to develop abstract representations of the pattern, representations which show some relations of communities … but show these in a form more easily comprehended and apart from the complexity of the whole. (ibid)

In order to tease these complex relationships out of his data and to construct more comprehensible representations of them, Whittaker pioneered the use, among ecologists, of a set of numerical analytical techniques known as *ordination*.

Ordination was essentially a process in which every individual community's structure (i.e. its species composition) could be directly compared to that of each other sampled community in turn. This series of comparisons required that a large number of descriptors for each community be recorded—typically, these descriptors were quantitative measures of the observed population for each species found in the community. After assembling this matrix of descriptors for each community, the ecologist could compare it directly to the corresponding matrix of each other sampled community (Figure 5.5). This pairwise comparison usually involved putting two data matrices side-by-side and from their values calculating an overall similarity coefficient between the two (many possible coefficients have been developed over the years).

	Descriptors							
Object $\mathbf{x}_1$	2	2	5	2	2	4	2	6
Object $\mathbf{x}_2$	1	3	3	1	2	2	2	5
R	1	4	2	4	1	3	2	5

Gower's Similarity
Coefficient = 0.66

Figure 5.5 Similarity Coefficient: Quantitative measurements of eight different descriptors for two separate objects or communities $\mathbf{x}_1$ and $\mathbf{x}_2$. If the maximum deviation, R, of each descriptor across all of the sampled objects is known, then Gower's coefficient of similarity can be calculated for the two objects. (Example from Legendre and Legendre, 1998.)

This comparison could be repeated for every other pair of sampled objects or communities. Once a similarity coefficient was calculated between each of the communities, all of the coefficients could themselves be arranged into an *similarity matrix* in which the series of communities ($\mathbf{x}_1$, $\mathbf{x}_2$, $\mathbf{x}_3$, … $\mathbf{x}_n$) is listed along the top and again along the side (Figure 5.6). This was a comprehensive numerical representation of how similar each community was to each of the other communities. (Notice that each community, when compared to itself, has a similarity coefficient of 1.) But in order to be ordinated, each of those similarity coefficients needed to be transformed into a dissimilarity coefficient instead. The simplest way of doing so was to subtract each of them from 1. The dissimilarity coefficient could then be treated as a Euclidian distance, such that all of the communities could be plotted at a certain metric distance from all of the others. Two communities with a very high similarity coefficient (i.e. which resembled one another very closely) would have a

	$\mathbf{x_1}$	$\mathbf{x_2}$	$\mathbf{x_3}$	$\mathbf{x_4}$	$\mathbf{x_5}$	$\mathbf{x_6}$	$\mathbf{x_7}$
$\mathbf{x_1}$	1.0						
$\mathbf{x_2}$	0.66	1.0					
$\mathbf{x_3}$	0.41	0.3	1.0				
$\mathbf{x_4}$	0.59	0.2	0.35	1.0			
$\mathbf{x_5}$	0.23	0.41	0.7	0.5	1.0		
$\mathbf{x_6}$	0.15	0.66	0.78	0.9	0.75	1.0	
$\mathbf{x_7}$	0.61	0.21	0.15	0.48	0.3	0.9	1.0

FIgure 5.6 Similarity Matrix. A hypothetical similarity matrix for seven different communities. Notice that the similarity coefficient of 0.66 that was calculated between communities $\mathbf{x_1}$ and $\mathbf{x_2}$ appears where they intersect in the similarity matrix.

very low dissimilarity coefficient and would therefore be plotted at a very small distance from one another. Two communities with a very low similarity coefficient would have a high dissimilarity coefficient and would be plotted at a large distance from one another. From this general procedure an overall graphical representation of the communities can be created in which each is represented by a single point, whose distance from the others depends on their numerical (dis)similarity to every other sampled community. Within such a representation, the points will typically be clustered into loose groups. Points (i.e. communities) within a cluster are relatively similar to each other, and relatively dissimilar to points far away from the cluster.

Although some clustering methods attempted to create hierarchical divisions between points based on the distances separating them, ordination was a technique (or rather, a family of techniques) that did not presuppose any particular relationship between points. Rather than classifying data points into nested groups, ordination sought only to *order* those points along a limited number of axes or gradients. Ordination produced visible clusters of points by reducing the data set to only the handful (typically two or three) of variables or axes that had the greatest effect on the objects' overall variation. Thus simplified into two or three dimensions, the distances separating the communities on each axis could be related, by a knowledgeable researcher, to the pertinent environmental gradient(s).

This technique took seriously the notion that certain sets of communities were structurally similar to each other, but it did not attempt to draw any strict boundaries between such sets. Nor did it attempt to define, in advance, which particular characteristics of like communities made them similar to one another. Rather, the edges of clusters could be defined more or less arbitrarily, depending on the purposes of the researcher. Each point, and the biological community it represented, remained a unique and individual whole. Though many such points clustered together in multidimensional phase space could be assumed to share similar dynamics of assembly, they were not assumed to fit any preexisting mold. Their proximity in the abstract phase space of the ordination revealed only a tendency to converge toward a similar set of ecological dynamics. Different communities were those which occupied different regions within this multi-dimensional, dynamical phase space—although really, all points in the ordination were different from each other. It was just a matter of how great that difference was.

5.8 Technocenosis

The assembly and the internal organization of plant communities, as formulated by Gleason and Whittaker, bears an uncanny resemblance to the assembly and organization of trawl nets that we studied at the outset of this chapter. We can enumerate their parallels: (1) Like a trawl net, a biological community is an open but limited energetic system that distributes energy between interacting populations of different species. (2) As with the selection of a trawl net's components, the selection of one species over another in the establishment of a biological community is functionally underdetermined. (3) This under-determination stems in both cases from the functional equivalence of various species or components within a given set of physical conditions—a phenomenon known as niche overlap. (4) Accordingly, just as with the components of a trawl net, the various populations within a community are to some degree replaceable or interchangeable with one another. (5) The ultimate organization of a biological community, like that of a trawl net, is therefore largely dependent on the stochastic availability of certain species (and not others) at the time of assembly. (6) Just as for the components of a trawl, the individualism of the various species within a biological community does not mean they are unrelated to each other, but rather that their associations are only ever partial. (7) For this reason, the characteristic structural patterns of different biological communities, just like the common structural patterns of different trawl nets, are seldom forthcoming when examining a single exemplar, but may emerge quite clearly in the comparison of large numbers of exemplars. Both systems of assembly are characterized by the same forms of underdetermined and stochastic selection, acting on multiple interacting populations, to produce the same kind of fractionally coherent objects.

This litany of morphogenetic or form-giving parallels should make it clear that *trawl nets and biological communities share a common mode of existence*—and one that scarcely resembles Simondon's proposed "mode of existence for technical objects". Indeed, it is my contention that any attempt to review the oddities of the trawl design process will implicitly lead one to restate the Gleasonian dissent in technological terms. For the very aspects of trawl assembly and organization which serve to frustrate the trawl engineer, and which so clearly differentiate trawl nets from the quasi-organismic objects of Simondonian philosophy, are also the aspects of assembly and organization which were highlighted by Gleason and Whittaker to show that plant communities are not the quasi-organismic objects of Clementsian landscape ecology. In other words, we can and indeed must say that trawl nets are different from concretized technical objects in precisely the same way that Gleason and Whittaker's biological communities are different from Clementsian vegetation units. We are therefore left with little choice but to diverge from Simondon's *Mode of Existence* along the pre-established lines of the Gleasonian dissent.

The highly unusual technical structures that come into better focus through this dissent do not currently have a name. But since biological communities, the fractionally coherent objects of Gleason's individualistic ecology, are also known as *biocenoses*, it would make sense to call the fractionally coherent technical objects of this study *technocenoses*. An individual trawl net can therefore be referred to as a technocenosis. Irrefutably a technical object, it nevertheless has a profoundly ecological structure and a profoundly ecological manner of assembly.

The notion of a technocenosis is not meant to be startling. It simply identifies, in the dark zone between technical and ecological systems, common ontological ground whose existence has not previously been charted. In doing so it takes seriously the notion of *technonatures* (a term that, by contrast, was deliberately coined to be provocative), even as it significantly reworks and expands upon that notion. For, a technocenosis is not merely an ecological system so infused with technical objects, nor a technical system so infused with natural bodies, that the two realms become inextricable. It is more than just an extraordinarily close and therefore illicit intermingling between the two domains, an unholy mixture of supposed opposites. No, a technocenosis is wholly technical, even as it is wholly ecological. Or more correctly, the dynamics of its assembly are wholly *cenotic*, just as those of ecological communities are wholly *cenotic*. The two structures are merely different physical manifestations of a shared set of basic morphogenetic processes.

Indeed, to call them physically different is something of an understatement, for a trawl net bears absolutely no physical resemblance to a plant community. We would never think to relate the two based on their appearances alone. They are composed of such radically different materials and take on such radically distinct final forms that their similarities might seem, to the reader, to be at best metaphorical. But to call a trawl net a technocenosis is not simply to draw an analogy between trawl nets and biological communities. The relationship I wish to highlight between the two kinds of cenosis is not analogical or metaphorical in the slightest (such metaphorical reasoning, as Deleuze points out, constitutes a step back into Cartesian dualisms and representational thought). Rather, what I am pointing out is an absolutely strict correspondence between the long-term physical tendencies of two dynamic systems of assembly.

To decipher this form of systemic and dynamical equivalence, let us turn to DeLanda's (2002) lucid analysis of Deleuzian multiplicities and, more particularly, of mathematical *manifolds*. A manifold, DeLanda explains, is a multidimensional phase space that is used to model a dynamical physical system. It usually describes a dissipative, or energetically open, physical system that, because it is not closed, can have numerous locally optimal solutions. A manifold features a separate axis or dimension for each degree of freedom within the physical system: in other words, a separate dimension of the modeling space describes each way in which its component parts can change. The entire manifold therefore represents every possible state of the system. Within this overall space of possibilities, the instantaneous state of the system can be represented by a single point, while its change of state over time can be represented by a curve. Mapping the trajectory of any physical system or object through its manifold allows for a rigorous description of the way the entire system changes over time.

When complex physical systems first underwent this kind of mathematical simplification and visualization, DeLanda notes, it suddenly became possible to observe *recurrent behaviors* between very different systems. In other words, by directly comparing the trajectories that different systems took through their manifolds, it became clear that very dissimilar physical systems often behaved in precisely the same mathematical ways. Investigating this peculiar phenomenon, the 19th-century mathematician Henri Poincaré discovered the existence of *singularities*, peculiar topological features of manifolds that "have a large influence in the behavior of the trajectories, and since the latter represent the actual series of states of a physical system, a large influence on the behavior of the physical system itself" (DeLanda 2002, p. 14). Such singularities could take a number of forms, including a single point, a closed loop, or even a chaotic and non-repeating orbit around a particular area of the manifold. Regardless of their form, Poincaré showed that singularities could act as attractors, drawing towards themselves any physical system whose trajectory brought it within their sphere of influence (or basin of attraction). Accordingly, different iterations of a single physical system might be drawn together into a tight cluster around the attractor, even if they started off in very different initial states (i.e. were located at different points in the manifold). They would tend toward similar states over time. However, such trajectories could only approach the attractor asymptotically. This meant that the various iterations of the system were never perfectly identical to the attractor or, for that matter, to each other. They might cluster, but they would not merge. Furthermore, because a single manifold might be mathematically structured by multiple attractors at once, two physical systems within a single manifold might potentially fall into the orbit of different singularities and end up quite different from one another (i.e. in different clusters).

What matters most about such attractors, DeLanda writes, "is that they are *recurrent topological features*, which means that different sets of equations, representing quite different physical systems, may possess a similar distribution of attractors and hence, similar long-term behavior" (DeLanda 2002, p. 15). In other words, the mathematical singularities that guided the behavior of physical systems were not dependent on the precise physical mechanisms characterizing any particular system. They were mechanism independent. Accordingly, a certain distribution of singularities might structure the manifolds of numerous physical systems that did not otherwise resemble each other in the slightest. The dynamics of those systems would be mathematically similar, even as their appearances remained wholly dissimilar.

As an example, DeLanda points to two very dissimilar physical objects: a soap bubble and a salt crystal. One is spherical, the other cubical. One is pliable and supremely fragile, the other is rigid and extremely durable. They are made out of completely different substances, have completely different molecular structures, and are characterized by completely different behaviors in the world. Yet each of them, as a physical system, is characterized by an identical tendency to seek a point of minimum intermolecular bonding energy. A salt crystal minimizes the energy required to hold its molecules together by relaxing into its characteristic crystalline lattice, while a soap bubble minimizes its surface tension by assuming a spherical shape. Neither their materials nor their forms resemble one another in the slightest, but their assemblies are nonetheless structured by the same attractor in phase space. Their shared pattern of attractors gives form to similar morphogenetic processes, not similar end products of those processes.

Pondering the way in which the logic of biological evolution has been recapitulated in the 'genetic algorithm' used in computerized systems of artificial intelligence, DeLanda (1994) notes that within an open dynamical system,

> the coupling of any kind of spontaneous variation to any kind of selection pressure results in a sort of 'searching device'. This 'device' spontaneously explores a space of possibilities (i.e. possible combinations of traits), and is capable of finding, over many generations, more or less stable combinations of features, more or less stable solutions to problems posed by the environment. This 'device' has today been implemented in populations that are not biological. (DeLanda 1994, p. 265)

We could potentially identify many different kinds of physical system that function as "searching devices" in this sense. For example, technical progress is characterized by the successive redesign of technical objects over time toward solutions (or configurations) that are at least locally and temporarily optimal. Biological communities, too, quite clearly "search out", in their own blind manner, those locally optimal combinations of species that will prove to be stable over time. My contention about trawl nets and biological communities can therefore be clarified even further: bio- and technocenoses are dynamical systems that constitute a single kind of searching device. In each system, spontaneous variations arise and are either selected or deselected. In each system, the operative selection pressures are the same. And in each system the object that results is therefore characterized by the same fractional coherence.

The final products that are assembled by this peculiar searching device do not look similar or behave in the same way. But that is precisely what we should expect. Trawl nets and biological communities are, after all, related to each other on the level of their manifolds, in precisely the same manner as DeLanda's soap bubble and salt crystal. A biocenosis resembles a technocenosis not in a visual or material sense, but in a morphogenetic sense. The formative selection pressures that steer their assemblies, the underlying dynamics of their organization and genesis, are the same. If a technocenosis looks nothing like a biocenosis, it is simply because there exists no reason that two similarly structured dynamical systems should produce similarly structured physical results. It is the processes of assembly, themselves, that are the same, not their respective products.

From this vantage point, it is clear that the great physical dissimilarity of trawl nets and biological communities does not, in fact, force us to downgrade their relationship to something metaphorical. This is a crucial point, for metaphors are inherently descriptive rather than explanatory. On a purely descriptive level, we might say that a trawl net resembles any number of things: that it shudders and shakes against a strong current like a old drunkard moving down a crowded sidewalk; that it plows its way blindly across the ocean floor like an unmanned bulldozer; that it slowly unfurls like a brilliant and deadly flower as it sinks behind the trawl vessel. None of these descriptions can be wrong, for they are not attempting to explain anything. They are one person's impressions and nothing more. By contrast, the comparison I have drawn between biological communities and trawl nets is explanatory. It *accounts for* the unusual vagaries and specificities of trawl net structure by identifying certain processes, pressures, and dynamics of assembly that trawl nets share with biological communities.

In making this claim, I am not simply hijacking the findings of community ecologists and then steering them, by force, toward my own desired destination. I am not foisting my own set of mathematical and philosophical ideas onto them and calling it their own. We have already seen that Robert Whittaker, champion of plant individualism and pioneer of ecological ordination, treated biological communities as points or trajectories within a manifold. He recognized that communities were objects or systems that could be accurately studied as points within a multidimensional phase space, whose axes defined the system's degrees of freedom. His revolutionary alternative to ecological classification was to plot (or *ordinate*) sampled communities within this multidimensional space and show how they tended to cluster in certain areas of mathematical space. This approach made it possible for Whittaker to discuss and analyze the evident similarities between different communities while removing from himself the typological burdens of ecological classification. Indeed, he turned to ordination precisely because he saw traditional classification, with its search for formal criteria that might somehow define membership in sharply delineated typological categories, as an impossible task. By ordinating biocenoses, Whittaker could show that a group of them might show striking similarities even when there did not exist any single set of shared traits between them.

In all of this, it is clear that Whittaker's work shares both its mathematical underpinnings and its primary impulse (an intense aversion to classificatory thinking) with Deleuzian philosophy. So my recourse to the logic of manifolds, through DeLanda and thence Deleuze, is not simply an arbitrary shortcut helping me to relate two very dissimilar kinds of object. In a very real sense, the argument that we should understand cenoses in terms of multidimensional phase spaces in which different iterations of a complex physical system will cluster around some set of characteristic attractors is not primarily mine, but Whittaker's. In coining the term *technocenosis* I am building on an understanding of biological communities that already exists.

Indeed, the peculiar structural paradox that is embodied by trawl nets, and that has formed the focal point of this chapter, is also the formative problem of community ecology as a whole. It is that discipline's very reason for existence—and cannot be considered as some random offshoot explored only by Gleason and Whittaker. We might even say that community ecology is nothing more than the study of the fractionally coherent structures formed by interacting populations. Putnam (1994), echoing two of the world's preeminent community ecologists, Joan Roughgarden and Jared Diamond, writes that "the central problem of community ecology as a whole" is to search for "explanation of the 'limited membership' of ecological communities" (Putnam 1994, p. 89). In other words, what community ecologists seek to explain is why any given area is settled not by all of the species that might potentially survive there, based on their highly overlapping individual physiological tolerances to the environment, but only by some limited subset or specific combination of those species. It is the search for specific "assembly rules" that explain why certain groups of species occur together more or less frequently than one might expect if their establishment were wholly random (Cody and Diamond, 1975).

Although such patterned groupings often become quite readily apparent to ecologists in the field, the search for any broadly applicable rules of assembly steering their formation is hindered by "the tremendous complexity of interrelationship within the community" (Putnam 1994, p. 4). This complexity of partial relations makes it difficult to tell what actually constitutes the structural pattern of any particular community and what, exactly, separates one patterned grouping of species from another. As Putnam (1994, p. 4) puts it,

> While it may prove possible to offer a detailed description of the composition and operation of a well-studied individual system…, the very intricacy of such description … itself confounds attempts to draw out any less specific principles of organization or functional applicable to communities in general.

The main problem around which the discipline of community ecology revolves is therefore—as I have already argued through the work of Gleason and Whittaker—precisely the same one that obstructs the study of trawl net design. As soon as an ecologist (even a more contemporary one) intensively studies the make-up of any one community, any general patterns of assembly that initially seemed clear become opaque, indistinct, and overly complex. There is no easy way to link the individual cenotic object with the overall pattern it seems to embody. The work of community ecologists is precisely to tease out and explain the ineffable patterns of community structure that emerge from the various interrelationships between individual species within a given geographical area. As a discipline, community ecology seeks to prove that such patterns exist between communities and that, rather than being wholly coincidental, they are the logical (if fractional) result of "the type, strength, and direction" of "the various interrelationships linking together the individual species" in an area (Putnam 1994, p. 2). So my conclusions are not warped by an overreliance on the work of Gleason and Whittaker. Indeed, the problems with which the two men grappled still constitute the central focus of community ecology as a discipline.

Another objection might still be raised, however. In positing that trawl nets are technocenoses, and that they share the dynamics of their assembly with biological communities, am I not in a sense eliding the role of humans in the construction and ongoing progress of trawl nets? After all, biological communities are assembled (and their structures continue to evolve) in a fairly automatic and involuntary manner. Their assembly occurs in a way that is brainless and unseeing. The assembly of a biological community gropes blindly along on its own, like a creature in a cave: uncoordinated, unaware, uncaring. Surely I am not equating this impoverished form of assembly with that of trawl nets, in which humans mobilize vast networks of additional intermediaries—technical, social, institutional, financial, etc.— in order to develop, test, and successfully market the latest innovations in net design? More to the point, aren't all decisions about the final structure of a trawl net carefully considered by a thinking, seeing, remembering *designer*? Isn't this human mediator absolutely indispensible in the assembly of any trawl? Surely he or she has no equivalent in the assembly of biological communities. Surely, my blithe suggestion that the two systems of assembly are "the same" is therefore well off the mark.

This objection is essentially a question about the nature of mutation and selection in a searching device. It boils down to whether or not humans play an essential role in both the development of trawl net design innovations—mutations—and the selection of design elements during the net's assembly. In terms of the second concern, selection, I have already shown that humans do, of course, play an absolutely indispensible role in selecting trawl components. Indeed, the first part of this chapter was dedicated precisely to describing the numerous actions performed by the engineer and the fisherman in order to select various trawl components during the assembly of any trawl net. But this quite obvious human participation does not invalidate the parallels I have drawn between trawl nets and biological communities. Quite the opposite is true: it is precisely the mediation of humans in the process of selection that endows trawl assembly with its most characteristically *cenotic* features! On one hand, the series of evaluations performed by the trawl engineer leads him or her into a quandary of niche overlap and functional under-determination. On the other hand, unpredictable inputs from the individual fisherman make design elements stochastically available or unavailable to the engineer. So niche overlap and stochastic availability, the two operative features of any cenosis, emerge directly from the participation of humans, not in spite of it. In fact, the precise mechanisms through which these features arise are irrelevant, for the recurrent features of different manifolds are always mechanism independent.

In terms of design innovations, or what DeLanda calls "spontaneous variations", the precise mechanism by which these arise is also unimportant. We do not have to take DeLanda's word on this. It is simply clear that a searching device is a system of assembly whose behavior relies primarily on the selection of components. Even if there were no design mutations at all—*i.e.*, even if all the available design elements had existed in their current form for quite some time—the selection pressures of the system would continue to act on any available elements. It is these selection pressures that in every case determines which elements are chosen for inclusion in the final object. Mutations or spontaneous variations are not necessary for the system to continue to assemble things;

variations are only necessary if the system is to evolve over time toward locally optimal states of relative stability.

Even if spontaneous variations do exist, however, what matters is simply that they emerge into the system of selection. It does not matter what their provenance is. The collection of processes and mechanisms that produced the variation are unimportant, for these have no effect on the behavior of the evolving system itself. We could better illustrate this by looking at a more familiar kind of searching device: in biological evolution it is irrelevant what precise errors in chemical self-replication led to the emergence of a particular chromosomal mutation. It matters, of course, that such errors emerge with some regularity. But the actual mechanisms responsible for that spontaneous variation have no effect on the behavior of the evolving system itself. Biological evolution is a system defined by natural selection and sexual selection, regardless of the precise mechanisms that lead to faulty chromosomal replication. This, of course, is not to say that trawl assembly is *like* biological evolution. It is simply to point out that what matters, in any dynamical system that functions as a searching device, is the existence of spontaneous variations, rather than the particular details of their creation. To suggest that trawl nets are cenotic is not, therefore, to unduly pass over the role of humans in the process of trawl assembly.

Finally, In case it is not obvious, I should point out that a *technocenosis* is a kind of technical assemblage. It fulfills all of the criteria of DeLanda's "assemblage theory" that were discussed at the outset of this study. A trawl net is characterized by non-linear causal mechanisms such as catalysis and thresholds. Its components are linked by relations of exteriority, in the sense that they could just as well be doing something else if they were inserted into another object. The parts of a trawl net are therefore highly substitutable for one another. Since they can be substituted to no ill effect, their incorporation together in a single object is not so much logically necessary as it is contingently obligatory—their form, in other words, owes a great deal to historical contingencies. Accordingly, they do not embody some structural form that is fixed in advance: their structural patterns can only emerge over time, through repeated iterations of assembly. They are population effects, and as such they are inherently statistical in nature. All of these traits clearly mark technocenoses as assemblages. However, I am not suggesting that the opposite is also true. In other words, the observation that technocenoses are technical assemblages does not mean that all technical assemblages are necessarily technocenoses. Other kinds of technical assemblage might certainly exist. This study simply shows that one specific kind of technical assemblage is possible.

In showing this possibility, we have finally bridged the theoretical lacuna to which I referred in Chapter 1. The pragmatic question of trawl net source identification forced us up against a number of rather immediate obstacles at the outset of this study. Resolving these difficulties forced us to closely scrutinize the way in which we conceive of technical progress, technical function, and technical assembly. In turn, this increased scrutiny has resulted in a remarkable reformulation of how we understand the organization of trawl nets. Having cleared our path of its initial obstacles, we may therefore return to the initial problem addressed by this study: Is it feasible to identify the sources of derelict netting fragments in the Northwestern Hawaiian Islands by examining the design features of trawl nets? Should we understand the shortcomings of NOAA's net identification database as a temporary setback, which might still be overcome by dedicating further resources to the collection and/or analysis of additional data? Or do the problems with the identification database run deeper, and therefore constitute a terminal flaw for the entire enterprise? It is to these questions that we return in the following, final chapter.

Chapter 6 . Conclusion: Trawl Nets and Traceability

In the last chapter we saw that trawl nets can be characterized as *technocenoses*: complex objects comprised of various populations of functionally interchangeable technical components connected in an overall web of energy distribution. We concluded that biological communities and trawl nets are exemplars of a single kind of object in an *ontogenetic* sense—in other words, that biocenoses and technocenoses share a single mode of coming-into-existence. The processes through which they are assembled, their very emergence as objects, are, although materially quite divergent, structured by a common set of dynamic impulses and constraints.

This ontological conclusion is crucially important to the overall goal of this study—assessing the traceability of derelict trawl nets in the NWHI—given that ontology always predetermines or delimits a certain set of epistemological possibilities. In other words, the ways in which we conceive of things' existence in the world necessarily preconditions what we can know about those things and how we can know it. (The reverse is also true: our act of gathering knowledge about things also influences how we conceive of their existence.) If, hearkening back to Chapter 2, we take things to exist in the mode of a Sudoku puzzle, then there are specific things that we can ultimately know about them (*e.g.* the numeral occupying each square of the puzzle) and specific procedures that we can follow in order to gain that knowledge. Likewise, if we take trawl nets to be fractionally coherent objects whose structural tendencies can only emerge iteratively, this quite clearly has epistemological ramifications for how we can study them and what we can learn about them.

6.1 Are derelict trawl nets traceable?

Over the past fifty years, no group has dedicated more effort to exploring the epistemological ramifications of cenotic structure than community ecologists. In this sense, they have already done much of the intellectual heavy lifting for any trawl design researcher. Their methodological work, in particular, constitutes a valuable epistemological resource for the study of other ontologically cenotic objects, including derelict trawl nets. This is not to say that trawl researchers can simply adopt, wholesale, the methodological toolbox developed by community ecologists. For, as we have already seen, biocenoses and technocenoses are materially quite different from one another. The methods developed to grapple with the material structure of biocenoses will therefore tend not to be useful for the study of (materially and structurally quite distinct) technocenoses.

For example, in a biocenosis the component populations are more or less evenly interspersed with one another spatially. They are mixed or intermingled. Plant cover, as Gleason delighted in pointing out, may never be entirely homogeneous. But it is often very nearly so. For this reason, ecologists employ *sampling* methods in which small areas of vegetation are assumed to faithfully represent the overall structure of the larger community. By contrast, the component populations of trawl nets almost always occur in homogenous, spatially discrete blocks called netting panels. The individual mesh cells of different species are not randomly interspersed with each other across the surface of the trawl net (if they were, the net would indeed begin to look something like a plant community!). Accordingly, no one portion of the trawl can be assumed to structurally represent the whole, and the sampling methods developed by community ecologists cannot be employed in the study of trawls.

What trawl nets and biological communities have in common is, instead, the basic dynamical impulses or pressures of their assembly. It is therefore the methods developed by ecologists to analyze the dynamics of community assembly that will prove more useful and more readily adoptable to trawl net researchers. For example, ecologists have developed many different similarity coefficients over the years—various algorithms that can be used to calculate the numerical degree of likeness between different communities (see Legendre and Legendre, 1998; Sneath and Sokal, 1973). The most straightforward of these similarity coefficients—the simple matching coefficient (S_M) and Jaccard's coefficient (S_J)—are, not surprisingly, also the most commonly used. But the study of trawl net designs would quite clearly benefit from the use of Gower's coefficient (S_G) instead. This is a more complicated alternative that offers a number of important advantages over the previous two coefficients (Gower, 1971). Firstly, unlike most coefficients, Gower's permits the simultaneous analysis of all types of descriptive data, whether these are binary characters (e.g. twisted vs. braided twine), multistate characters (e.g. kind of polymer used), qualitative characters (e.g. color of twine), quantitative characters (e.g. the measured mesh size), or semi-quantitative characters (properties that are simply assigned a number value by the researcher). This property is particularly useful for analyzing trawl netting, since at the very minimum that netting is described by characters that are qualitative (e.g. green), quantitative (e.g. mesh size of 23.5 cm), and binary (e.g. presence or absence of knots). Secondly, Gower's coefficient allows for the comparison of two specimens even when there is no available data for a given character on one of them. In other words, it would permit the comparison of two nets even if one of them contained significant amounts of Ultra Cross knotless netting and the other one did not contain any. This flexibility is useful for the study of trawl nets, since their designs are so highly variable. It is highly unlikely that the nets used in any one fishery all contain the same kinds of netting (simply in different quantities), and Gower's coefficient allows them to be compared despite this. Thirdly, because it is not affected by the absence of data, Gower's coefficient also allows for the analysis of hierarchic or dependent characters. An example of a hierarchic character might be the kind of knot used within a panel of netting (character B), a potentially multistate datum which will not be recorded for all trawl nets since it depends first on whether the netting itself is knotted or braided (character A). Finally, Gower's coefficient does all of this while retaining the primary strengths of the two aforementioned coefficients: It is equivalent to the simple matching coefficient when using only multi-state characters, and to Jaccard's coefficient when only using binary characters. The key point is that this powerful epistemological tool for studying cenoses already exists. It does not have to be developed from scratch by trawl net researchers, for it has already been created, used, and evaluated by ecologists for nearly forty years. It is in this sense that ecological methods comprise a valuable resource for trawl researchers.

However, the work of community ecologists also serves a more negative, cautionary purpose, highlighting the major ontological and epistemological obstacles that will impede any study of trawl nets. This is especially true when it comes to the challenges posed by trawl nets' fractionally coherent, iteratively emergent, and therefore inherently *statistical* nature. There are a number of basic statistical constraints with which community ecologists have long familiarized themselves, and which should provide the trawl researcher with a useful glimpse of what he or she probably can never know about the structural patterns of trawl net designs. In particular, we might point to three such obstacles. These, it should be clear, do not comprise any sort of exhaustive accounting of ecological multivariate statistics—indeed, they are extremely basic observations about the fundamental limitations of multivariate clustering. Yet despite or perhaps because of their baseness, they constitute significant and even insurmountable obstacles to the identification and traceability of derelict trawl nets. What are these constraints?

Firstly, because clustering techniques such as ordination are statistical, their explanatory power is directionally limited by the *ecological fallacy*. In other words, ordination uses the characteristics of individual nets to build cumulative knowledge about a larger population of nets. The information it draws from each individual object pertains only fractionally to the overall cluster of objects. Like any other statistical exercise, ordination facilitates the movement from individual-level information to population-level information. But the reverse movement, from population back to individual, is not possible. The emergent properties of the entire cluster cannot be used to deduce anything at all about the structure of an individual net. So even if we know, through clustering, that within a certain population of trawl nets the presence of netting types A and B is strongly associated with netting types C and D, we cannot use the presence of A and B in any particular net fragment to conclude that it also originally contained C and D. The properties of an individual net cannot be reconstructed from the prevailing statistical characteristics of the group. As we shall see momentarily, we might at the very most be able to state that there is a certain percentage likelihood that C and D were also present in the originating net, and conversely a certain percentage likelihood that they were not. But that relationship can never have the quality of a proven fact or a smoking gun. It is never more than a probabilistic speculation.

Secondly, because the clusters of points depicted in an ordination result from the pairwise comparison of every single net to every other net, a new net cannot be introduced into the analysis without changing the size, shape, and positioning of those clusters. Put another way, it is the data points themselves that collectively structure the space of an ordination. There is no transcendent ordering logic to the ordination space beyond the collective values of all the data points themselves. The taxonomic space is emphatically not divided up into overarching *regions* that each contain a cluster of nets; it does not consist of a grid of containers into which each newly analyzed net may simply be placed. Rather, each time that a new net is introduced into the analysis, the entire iterative comparison between every possible pair of nets must be conducted anew. This makes the ongoing identification of nets very difficult indeed, for the classification of nets will itself change every time that a new net is studied. This change might simply affect the shape and number of clusters, but it might also conceivably alter which dimensions of inter-object variability are deemed to be the most important, thereby changing the very axes of the graphical space itself. If, following Ernst Mayr (1982), we envision classification as the creation of a set of taxonomic cubbyholes, and identification as the placing of objects into those cubbyholes, then the simple identification of trawl nets begins to seem very nearly impossible. For by even trying to insert any new object into the cubbyholes, we are changing their size, their shape, their number, and even possibly their logic. Mayr argued that the act of identification is necessarily preceded by the act of classification. But when nets are morphologically associated with one another through clustering, as indeed they must be, the act of classification can itself never be finished.

Thirdly, because the partial associations between netting components are not purely related to their functional requirements, but are always haunted by the specter of other, more stochastic factors affecting trawl net assembly, it can never be clear in advance what accounts for the multivariate clustering of nets. In other words, even if the data points representing trawl nets separate into obvious clusters when ordinated, there is no *a priori* way of knowing what those clusters indicate or what accounts for their appearance. Indeed, the very purpose of ordination is to try to clarify *which* of the many possible factors might best account for the variability of the dataset. An ordination is conducted precisely because the reason for clustering can never be wholly known in advance. And if the clustering exhibited by a group of nets cannot be automatically attributed to their provenance from a common fishery, and a common set of particular functional parameters, then the link between net identification and net traceability becomes somewhat dubious in itself. This is not to say that the clustering of nets cannot provide information on their origins, for it certainly may. But we cannot simply assume this in advance. Even as we meticulously gather data on nets, exhaustively compare those data sets through pairwise comparison matrices, and then conduct ordination based on their similarity (or rather, dissimilarity) coefficients, there is no guarantee that the resultant clusters will tell us anything of value to the would be tracer of trawl nets. Clusters might conceivably result from patterns in the continent-scale availability of certain netting materials, or from the overall financial security of the fishing vessel's crew, or from the short-term trend in market prices for fish products at the time of the net's purchase. Indeed, clusters may not seem to result from any identifiable causes at all.

The statistical issues described above make it difficult to envision how the identification and tracing of derelict trawl netting might ever proceed. Because trawl nets are technocenoses, the structural patterns that gradually emerge across many nets can never be used to forensically reconstruct the absent portions of an individual fragment of netting. Because trawl nets are technocenoses, the act of trawl net identification will always remain hopelessly tangled up with the act of trawl net classification, and no newly found derelict net will be identifiable without the simultaneous re-identification of every other net as well. Because trawl nets are technocenoses, whatever structural patterns do appear to emerge (if, indeed, the data points cluster at all) might potentially be related to any number of non-functional parameters that would not allow us to link them to any particular fishery. Each of these limitations illustrates the fundamental importance of recognizing, in advance, that trawl nets are technical communities rather than technical objects in the functionally integrated and structurally over-determined Simondonian sense. They make it clear that the study of communities, while potentially leading us to any number of insights about their assembly, can never help us to forensically reconstruct them with any degree of certainty, and can never help us to clearly identify their functional purpose.

It is crucial to note that these obstacles to traceability are not merely epistemological, but ontological. They arise from the fractionally coherent mode of existence of any cenosis. For this reason, they cannot be overcome simply by devoting greater financial resources or greater amounts of intellectual effort to solving the problem. They are not the kind of temporary difficulties experienced when one, having mastered the procedure of solving intermediate-level Sudoku puzzles, moves on to tackle more difficult puzzles. Newer methods of solving the derelict trawl net puzzle—intellectual procedures of greater nuance, difficulty, and effectiveness—quite simply cannot be learned or discovered. The peculiar ontological condition of trawl nets dictates that no set of epistemological procedures can ever exist that would suddenly make the origins of individual trawl net fragments knowable from their designs. Rather, our unyielding conclusion must be that *it will never be feasible to forensically reconstruct fragments of derelict trawl netting and identify their sources through the analysis of their physical design characteristics.* The ontology of trawl nets, their mode of existence as technocenoses, relegates such an operation to the realm of sheer epistemological impossibility. Dedicating any additional resources to the problem is, in this sense, simply to throw good money after bad.

With the recognition that trawl nets are technocenoses, the outright failure of NOAA's net identification database to identify sources of derelict netting in the Northwestern Hawaiian Islands, even after the collection of many thousands of netting samples, begins to seem not only comprehensible but inevitable. It also becomes clear why the very small degree of traceability achieved for some of those derelict net samples is related not to their functional parameters (i.e. their source fishery) but to the regional availability of their component nettings. For example, the database's conclusion that some netting fragments originated in "Asia" is based not on some common feature of Asian fisheries, but rather on the presence of a braided twine material that is generally not imported by net manufacturers in North American and Europe. This kind of result, functionally ambiguous and related to the stochastic availability of components at a given time, is exactly what we would expect from the study of cenoses. Such failures of identification and tracing will never be remedied, no matter how many tens of thousands of nets are added to the net identification database, for trawl nets, like any other cenoses, simply do not avail themselves to individual identification and traceability. No amount of federal funding, and no investment of intellectual resources, will ever change this. Accordingly, there appears to be little justification for the continued funding of design-based trawl net identification efforts in the NWHI or anywhere else. The futility of such efforts is wholly assured in advance.

In order to skirt the problems that fractional coherence poses to identification—to avoid, in other words, the seemingly paradoxical dissolution of any apparent structural pattern as soon as it is studied in earnest—ecologists sometimes forgo the wholesale description of a community and instead attempt to identify it by the occurrence in their samples of some indicator species, whose presence suggests the community is fulfilling a particular functional role. For example, the presence of a particular plant within the ecologist's sampling quadrat might be enough to indicate that a given patch of land is adequately performing the ecological functions of a wetland (Robertson, 2006). Can such species-based indicator methods be used to functionally identify fragments of derelict netting? Conceivably, yes. But success in this endeavor would require an extraordinary combination of hard work and good luck and would occur only sporadically at best. The most widely used form of indicator analysis, a deceptively simple method of determining the indicator value of any individual species or assemblage of species within a group of ecological samples, is that developed by Dufrene and Legendre (1997). Their method uses both the frequency with which a single species or species-assemblage occurs across a group of samples, as well as its relative abundance within those samples, to determine its overall value as an indicator of the sampled group.

This procedure could clearly be applied to trawl net samples. If the numerous trawl nets employed within any given fishery were analyzed in detail, it would theoretically be possible to state that a certain type of netting occurs in some percentage of all the nets, in some typical amount (measured by the number of meshes), and from those figures to calculate the netting's value as an indicator of that fishery. However, this method does not overcome the statistical hindrances I have already discussed. Rather, it allows the researcher to state that, in the presence of some amount of some type of netting, there is a certain percentage chance that the net in question originated in some particular fishery. If that percentage is relatively low, than this method will obviously not allow for the identification and traceability of trawl fragments. Achieving a suitably elevated indicator value, while not impossible, would likely require that some specific combination of multiple nettings prove characteristic of almost every trawl net within a certain fishery. This, as we have already seen, is highly unlikely. Furthermore, even if some specific grouping of netting components does prove to have a very high indicator value for a specific fishery, that information will only be useful if the exact same grouping of components is present in the fragment of derelict netting that is recovered. Were entire trawl nets lost and then recovered in the NWHI this would not necessarily complicate matters. But as we saw in Chapter 1, derelict trawl netting is almost always recovered in small fragments that are torn from some part of the larger net. It is rare to find large fragments composed of multiple panels, and almost unheard of to recover entire abandoned trawl nets. In sum, productively using Dufrene and Legendre's indicator value analysis would require not only an exhaustive initial study of actively used trawl nets within a given fishery, but also an unusually high degree of trawl design homogeneity within that fishery, as well as the highly fortuitous recovery of large sections of derelict netting. Even if all of these unlikely conditions were met, the researcher might still only be able to conclude that there is a 70% chance of a derelict net originating in a specific fishery. Is such an indicator value sufficiently high to be actionable for policymakers or enforcement agencies? Unfortunately, it probably is not.

All of this is not to say that the study of trawl net designs is necessarily a waste of time and money. It is only so if our specific goal is to trace derelict nets back to their sources. Beyond this aspiration, there is presumably quite a lot that we could learn from such a trawl net research program. In particular, since ordination helps clarify what factors account for most of the structural variance across a group of samples, conducting an "ecological" study of trawl nets would allow us to rigorously test Fridman's (1973, 1986) oft-repeated but unproven claim that trawl net design is steered by three factors: the parameters of the fishing vessel, the characteristics of the target species, and the physical features of the fishing ground. Which of these factors, if any, is most important in a given fishery? And are these perhaps only the factors of most importance to engineers? What other factors, unrelated to trawl engineering, play the most important role in determining the structure of a trawl? Can policymakers manipulate those formative factors and thereby adjust, indirectly, the way in which fishermen build (or order) their trawls? All of these are valid questions that can, in all likelihood, be answered at least in part by the multivariate statistical analysis of trawl designs. But none of this knowledge will make it possible to positively identify an individual technocenosis as one kind of functional object or another. As Robertson (2006) has shown, human institutions can act only with the utmost difficulty and hesitation around such cenotic, functionally indistinct objects. If we wish to identify trawl nets with a degree of confidence that will allow us to take regulatory or policy actions based on their recognition, then we cannot rely on the inherent identifiers that are only fractionally present in their designs. Instead, ensuring the traceability of trawl nets will require the addition of extrinsic identifiers to their designs: ancillary design features such as tags or specifically colored twines that will presumably serve no functional purpose beyond aiding in the nets' identification.

For all of these reasons, this study's assertion that technocenoses exist is of great significance to the would-be tracer of derelict trawl nets. This finding makes it clear that it simply is not possible to use nets' design features as a means of assigning them a functional identity with any kind of assurance. However, beyond the problem of net identification and traceability, how significant is the postulated existence of technocenoses more generally? Are they common in the world? Must we therefore retrace our steps and reconsider technical objects that, in previous studies, were perhaps misconstrued as highly concretized and quasi-organismic entities? In sum, exactly how much does the result of this study change the way in which social scientists—and especially geographers—must study technical objects?

6.2 How common are technocenoses?

While it is certainly possible that other technocenoses exist, the highly unusual set of conditions that is necessary for cenotic patterns of assembly to emerge consigns such objects to extreme rarity. The findings of this study suggest that we might fruitfully look for other technocenoses wherever five basic conditions are met. Firstly, we should search for technical objects that are characterized by a nonlinear form of operative functioning, rather than Simondon's linear and nearly predetermined form of mechanical transduction. Such transductively non-mechanical objects cannot concretize over time into quasi-organismic functional bodies. Secondly, those objects must consist, structurally, of the amalgamated populations of many different component parts. In other words, each component part should form a basic, repeated unit that occurs numerous times within the structure of the individual object. Thirdly, those different parts must have overlapping ranges of functional tolerance, such that each population is highly replaceable by others. Fourthly, there should exist a relatively free market for the component parts, such that the design and assembly of the object is marked by a high degree of combinatorial freedom. Finally, for all of these reasons, we should look for objects that are neither standardized nor mass-produced, but are instead designed and constructed in a highly decentralized manner. Where all of these conditions are simultaneously fulfilled, we can reasonably expect the technical object in question to exhibit the curious fractional coherence of a cenosis.

These traits cannot be taken in a piecemeal fashion. They must all pertain to the object at once. To illustrate this we might productively look to another type of fishing gear: the fishhook. It would not be unreasonable for one to assume that fishhooks, like trawl nets, are technocenoses. After all, fishhooks are designed in a highly decentralized manner by fishermen all over the world, and are built from a diverse range of materials. Like trawls nets, their operative functioning seems to rely on the actions and instincts of the fish itself. Decentralized, highly variable, and incorporating the actions of a wild, living animal, should they not also exhibit the fractional coherence of a technocenosis? No, for two reasons. Firstly, because fishhooks, unlike trawl nets, are typically built as a single piece: a curved shaft with a barb at one end. They are not an amalgam of numerous components, let alone populations of different components. Secondly, the technical work of a fishhook is not behavioral, per se, but physical. Its role is simply to pierce the fish with its barbed tip and prevent its subsequent escape. The fish is not behaviorally stimulated by the hook in any way—at least not until it has already been caught. Rather, the fish is behaviorally attracted by the presence of bait, which serves the additional purpose of hiding the fishhook from the fish's perceptions. Because fishhooks perform mechanical rather than behavioral work, and because they are built as essentially a single component, their structure can in fact be optimized and standardized for the conditions that prevail in any given fishery. For this very reason, fishhooks are both functionally identifiable and geographically traceable to their source fisheries. Indeed, this technical trait of fishhooks is in no small part responsible for the emergence of fisheries science as an academic discipline. The Norwegian father of fisheries science, Georg Ossian Sars, was unable to fully piece together the life cycle of Atlantic cod until he found a distinctive hook embedded in the lip of a fish caught off the coast of Svalbard, and was able to trace that hook back to the cod fisheries of the Lofoten Islands in northwestern Norway (Rollefsen, 1962). Fishhooks are not only functionally identifiable and geographically traceable, but rather famously so.

The example of the fishhook makes it clear that cenotic assembly is not a ubiquitous trait of fishing gears. There is accordingly no compelling reason to suspect that other technocenoses can be discovered simply by examining other types of fish capture technology. But is the emergence of a technocenosis perhaps a size-dependent phenomenon instead? After all, both trawl nets and biological communities tend toward enormity. Does not their required compositional complexity, their elevated number of basic components, and their demographic repetition of individual unit parts restrict them to only the very largest objects? In an era when so much attention is devoted to the development and implications of microscopic bio- and nanotechnologies, is it not possible that the objects of our concern have escaped attention precisely by emerging at only the extreme end of the *macroscopic* scale—where technical objects essentially become too large to observe, rather than too small? If this conjecture were true—and it may well be—it would actually decrease our odds of finding and studying other technocenoses. For it is true that trawl nets are very large. In fact, they are so large (the biggest trawl currently in use has a circumference of 3.6 kilometers at the mouth) that we would be hard pressed to find another technical object of comparable dimensions whose construction did not require the mobilization of a highly centralized technoscientific network. In this sense, trawl nets are clearly something of an anomaly. Size and complexity are simply not properties that are conducive to the decentralized design and production that characterizes such nets. If technocenoses are, in fact, restricted to very large technical objects, we might therefore expect other examples to be nearly nonexistent. Yet, there is nothing about cenoses that inherently leads to gigantism. Indeed, while some ecologists study immense, landscape-scale communities comprised of trees, birds, mammals, and so forth, others study microscopic communities comprised of bacteria and single-celled organisms. Community dynamics, then, can just as easily emerge at the microscopic as at the macroscopic scale. Trawl nets are large because they must be large in order to capture schools of fish, and because the aqueous medium in which they are used allows the netting to remain relatively rigid during the tow while still compressing down to an easily stowable size onboard the vessel. But this does not mean that tiny technocenoses, composed of assembled populations of nano-scale objects, could not also exist.

One might also note that trawls and biocenoses both have at their core the unpredictable actions and behaviors of wild animals. Is cenotic structure therefore a mere side-effect of animality? Is the wildness of technocenotic structure simply an embodiment of the wild animal behavior through which the technocenosis functions? As with the previous conjecture about gigantism, this one, if correct, would make the discovery of other technocenoses quite unlikely. For, technical objects whose operative functioning incorporate the behaviors of undomesticated animals are of the utmost rarity. It is not that such objects cannot be designed and built, nor that they cannot perform their desired functions, but rather that the sheer statistical unpredictability of their operation makes them far less desirable than almost any other alternative. An excellent example of this is the so-called "tempest prognosticator", a device invented by the aptly named George Merryweather in 1850 and shown at the Great Exhibition in London the following year (Merryweather, 1851). The tempest prognosticator was a sort of biological barometer that used living leeches to predict the approach of storms, as well as their severity. It housed twelve leeches within twelve small glass bottles that were arranged in a circle around an ornately decorated bell. The leeches would become agitated by the changing conditions of the atmosphere preceding an electrical storm, and would wriggle their way up into the narrow bottlenecks. There the writhing of their bodies would displace a small piece of whalebone attached, via a thin chain, to a small hammer poised over the bell. The more times the bell rang, the more severe the approaching weather system. Merryweather lobbied the British government to install replicas of his device all along the island's coastline in order to provide a meteorological warning system for residents.

It is notable that Merryweather's device did not rely on the prognostications of only a single leech. Such a design would have been far simpler, smaller, and more portable, and therefore of far greater utility than the twelve-leech design. Yet the behavior of any single leech was characterized by a certain degree of randomness—"…some appeared to be more sensitive and prophetic than others," he wrote, "and some appeared to be absolutely stupid" [Merryweather 1851, p.43])—and individual predictions were therefore of rather dubious accuracy. Instead, the barometer necessarily functioned in a statistical manner, through the average or summated behaviors of all twelve leeches, a "jury of philosophical counselors" whose collective wisdom transcended the "confused and various" movements of its individual members (Merryweather 1851, p.44). The prognosticator's successful functioning consisted of the aggregate behavior of the animals over time, rather than any individual's movements at a given instant.

Although Merryweather's data suggested that the tempest prognosticator performed quite effectively, the British government turned down his proposal and instead chose to widely distribute Robert FitzRoy's storm glass: a sealed glass container, filled with a mixture of water, ethanol, ammonium chloride, potassium nitrate, and camphor, that acted as a chemical predictor of storms. Changes in temperature and pressure would affect the solubility of the liquid and affect the formation of various precipitates within it. The appearance of the liquid at any time—cloudy, filled with flakes, threaded, clear, etc—would therefore offer valuable clues about any impending weather system. Fishermen were not required to monitor twelve such devices simultaneously in order to reach a conclusion about the impending weather. The chemical reactions of the storm glass were so reliable, and so repeatable, that a single glass would do the trick. In this sense, it was clear that the mechanical transduction of chemical precipitation was technically superior to the living transduction of leech movements.

The example of the tempest prognosticator suggests that if technocenoses do emerge only around the technically harnessed behaviors and actions of wild animals, then they are likely to remain almost impossibly rare in the realm of technical objects. The technical effectiveness of such objects, their technical reliability and autonomy, is so thoroughly trumped by that of any viable non-biological alternative, that they are, like the tempest prognosticator, doomed to obscurity very nearly from the moment of their invention. It is conceivable, of course, that technocenoses are not tied to animality at all. In theory, any sufficiently unpredictable and immeasurable technical mechanism could stave off Simondonian concretization and result in the emergence of cenotic technical structure. But it is unclear what such nonlinear mechanisms would consist of, if not the affordances of living animals.

All of the above considerations point toward the extreme unlikelihood of finding other kinds of technocenosis, even should we explicitly go out searching for them. I will not claim that no other such objects exist in the world, but it is difficult to think of a single other example. Compared to other technical objects, trawl nets appear to be very nearly one of a kind. In many ways this exclusivity is problematic. For if trawl nets are so utterly alone, so unrelated to other technical objects, then isn't the very notion of a technocenosis itself rather dubious? Does not the sheer disconnectedness conferred upon trawl nets by my thesis indicate that the latter has strayed from the realm of verisimilitude? What broader utility can we possibly expect from the notion of technocenoses if, by all appearances, that notion can only be applied to a single kind of object? Surely I am not claiming that trawl nets exist in total ontological isolation from every other technical object in the world, am I?

6.3 How are technocenoses related to other kinds of technical object?

Such critiques erroneously mistake rarity for separation, scarcity for isolation. The study of trawl nets does not create ontological divisions between technical objects so much as it gives solid evidence of ontological continuities between them. Far from being theoretically disconnected to other technical objects, technocenoses represent the natural continuation and perhaps the culmination of an ongoing line of *topological* research into technical objects. Topology is the branch of geometry that studies spatial invariance under continuous transformation—in other words, the ways in which particular shapes can be distorted or deformed without altering some set of fundamental properties. As an example, we might say that the exact shape of a network, determined by the distances between all of its various nodes, can be continuously altered without affecting its basic connectivity in the least. As long as the pattern of connected nodes remains the same, we are dealing with one and the same network. Put another way, the network, even as it undergoes spatial transformations, retains a degree of topological equivalence. Depending on which spatial properties we deem it necessary to preserve, an object can potentially undergo various degrees of deformation while maintaining some form of basic topological equivalence.

Topological notions were introduced into the study of technical objects by Bruno Latour and other early actor-network theorists (Latour 1987, 1993, 1999). Latour envisioned technical objects not as things in themselves, but as networks of external relations or interactions with other things. This was not to say that a technical object was not a physical object, for it obviously was. But a technical object was effective—able to carry out its intended effects—because it sat at the nexus of all of these relations, tying together numerous agendas, arguments, projects, systems, and other physical objects. It served as a sort of keystone or condensation point for a dense network of active relations around itself. Latour showed that the power of a technical object depended largely on the stability of its external relations; its ability to assemble and maintain a specific network of social relations over time. The most powerful objects were those that were able to preserve a single identical web of relations wherever and whenever they were used. He called these objects *immutable mobiles*, since they could be transported from one place to another without becoming relationally unstable or structurally malleable. The relational stability of immutable mobiles allowed them to transport information infallibly from one location to another without its deterioration along the way. They were therefore conducive to the centralization of information and, with it, power.

Since the mid-1990s, however, a number of scholars have argued that technical objects can exhibit other forms of relational constancy beyond the rigidly crystallized relational networks of immutable mobiles. These critiques have deployed the notion of topology—albeit in a somewhat metaphorical way—to describe a more flexible and less brittle configuration of technical relations. Annemarie Mol, in particular, has spearheaded this effort with her contention that some things are *fluid* objects: that they become widespread not because their relations remain perfectly fixed and stable, but because those relations can gradually change, adapting at their margins to different conditions they encounter. Much of the work on fluid relations has focused on objects that are not technical per se, but rather objects of knowledge (e.g. Mol and Law, 1994; Law and Mol 2001; Mol, 2002). A wonderful exception, which insightfully explores the fluidity of technical relations, is De Laet and Mol's (2000) examination of the Zimbabwe bush pump.

The bush pump, a simple and durable device meant to provide communities with clean drinking water, is a technical object that has become extraordinarily widespread in Zimbabwe. Yet it has achieved its success largely because it is able to adapt to local conditions wherever it is installed. For example, the bush pump's bore hole can be drilled and its concrete apron poured in any number of ways, depending on the local characteristics of the soil and the labor force. When the pump's metal parts become fatigued they can be replaced with newer, factory-built parts, or with locally crafted components, or they may simply be removed and not replaced at all. In addition, the pump's ongoing maintenance might be assured, and its use regulated, either by a highly centralized group of village elders or by members of the entire community. And so on. Only by adapting is the pump able to consistently perform its function in different communities. If it were rigid and unadaptable, the pump would be a failure. De Laet and Mol refer to the bush pump as a *mutable mobile*. It moves and spreads precisely because it is relationally flexible.

But for all that malleability the bush pump remains to some degree invariant: it is still clearly identifiable as a Zimbabwe bush pump, even as its relations deform. This introduces what Mol calls the *problem of difference* into the study of technical objects. Insofar as it exists in many different versions of itself, the bush pump is clearly multiple. Yet all of its different versions just as clearly remain examples of a single thing. The object is marked by difference from itself. This raises a necessary question: How much change can an object undergo and still remain the same? De Laet and Mol's answer is that a fluid object is marked by the gradual and incremental nature of its relational changes. A piece is changed on the pump handle one year, and then a replacement seal for the plunger is improvised the following year. The deformations are continuous, since they occur slowly and only at the margins of the object. The core of the object, the stunningly effective design of its main pumping mechanism, is not altered in the slightest. The pump is not suddenly torn halfway apart and then rebuilt with new materials. The designs of two different pumps can only diverge gradually, accumulating minor differences incrementally. The continuity of these deformations shows that the pump is relationally topological, that its invariance persists through spatial transformations that are continuous.

De Laet and Mol's analysis also draws attention to three additional features of fluid objects. (1) On one hand, it is clear that the move from comparatively rigid network objects to fluid objects involves a significant decentralization of design and production. The bush pump retains a significant degree of centralization insofar as it is still mass produced in a factory using a single standardized (if occasionally updated) design. But the design process becomes decentralized over time, as each community installs, maintains, and repairs its pump independently of the manufacturer. It both remains centralized and takes an important step toward decentralization. In this, the study of more relationally fluid objects represents a movement away from actor-network theorists' traditional reliance on large, spectacular, highly centralized technical projects about which a single narrative can revolve. (2) On the other hand, as the physical structures of such objects become mutable, it becomes increasingly difficult to *gauge the success* of their functioning. The success of an immutable mobile is easy to judge: it either stabilizes its network or it fails to do so. But a mutable mobile has shifting relations. It flows into local conditions and might therefore fulfill a variety of functional roles. For this reason there is no firm baseline against which to judge its performance: its technical success is less than fully intelligible. However, for de Laet and Mol this unintelligibility does not go very deep. One can tell more or less how well a bush pump is working in any instance—it is simply that some of the standards of success (for instance the permissible level of *E. coli* in the water) change from place to place. At the end of the day one criterion of technical success remains perfectly clear: either the internal pumping mechanism is bringing water to the surface, or it is not. It either pumps, or it doesn't. So the bush pump's unintelligibility does not extend to its innermost technical core, does not cloud the basic hydraulic principles upon which its operation relies. Still, it is difficult to ever determine exactly how well or how poorly the pump is working. (3) Finally, Mol and Law note that the composition of fluid objects begins to approach Wittgenstein's notion of family resemblance, though it does not quite get there (Mol and Law, 1994; Law and Mol, 2001). In other words, the many different exemplars of a fluid object are not all necessarily similar or distinct in the same way. The features that any one exemplar shares with a second exemplar are not necessarily the same features that either of them shares with a third exemplar. They have numerous overlapping similarities and differences, even as certain core elements of the object's design remain absolutely identical in all exemplars.

Is there something beyond fluidity, some other topological mode of equivalence that would allow technical objects to remain invariant even as they underwent relatively extreme (i.e. less marginal and less gradual) forms of structural transformation? Can Mol's problem of difference be pushed beyond fluid technical objects and toward its extremes? John Law has attempted to extend Mol's topological insights by positing the existence of what he calls *fire objects* (Law and Mol, 2001; Law, 2002; Law and Singleton, 2005). These objects would be characterized not merely by their continuous deformations, but by a certain level of discontinuity as well. In other words, fire objects would remain singular even as their designs became fractured and marked by marked by abrupt differences. Law suggests that these, the topological extensions of Mol's fluid objects, would therefore be characterized by their fractional coherence. In other words, the study of fire objects, because these are marked both by extreme difference and by sameness, would be "about drawing [multiple] things together without centering them", such that the object would be correctly revealed as "more than one but less than many" (Law 2002, pp. 2-3). Fire objects, even when present, would be marked by the absence of their other, drastically different versions: they would in some way be present and not entirely present at the same time. Law incorporates all of these desired features into his formulation by asserting that fire objects are those that, even when tangibly present, endlessly refer to or rely upon other objects that are necessarily absent. They are, he writes, like flames: distinctively transformative objects marked by their constant *flickering*—their rapid oscillations back and forth between a present, centered singularity and an absent, decentered multiplicity arrayed in the form of a star around it.

Law's attempt to extend the topological inroads made by Mol suffers from a number of unfortunate flaws. Firstly, we might note that his notion of fire objects, while pushing beyond the fluid notion of difference as something gradual and continuous, does not adequately build upon the two other noteworthy traits of fluid technologies: namely, the decentralization of their design and production, and the growing unintelligibility of their technical successes. Instead of continuing the step taken by de Laet and Mol toward the democratization of the design process, its decentralization, Law returns in his (2002) study of fire objects to the kind of highly centralized technoscientific projects favored by classical actor-network theorists (in this case, a proposed new military aircraft). This constitutes a rather confusing step backward. Furthermore, Law does little to address the increasing difficulty, so heavily emphasized in de Laet and Mol (2000), of gauging a technical object's successful functioning as it becomes less topologically rigid and more relationally fluid. For a theoretical formulation that is explicitly attempting to push the bounds of topological thinking beyond Mol's fluid relations, these are significant and unexplained shortcomings.

Secondly, the notion of fire objects is in several respects highly arbitrary. For example, while the problem of difference, if taken to its extremes, will clearly intertwine multiplicity with singularity, and therefore make a thing's presence in the immediate here-and-now less than total, there is no reason why the dual interplay of multiplicity/singularity and presence/absence must take the form of an uncontrollable "flickering" between two extremes. Law neither makes it clear why this spastic jumping back and forth between poles is necessary, nor explains the supposed mechanism for such unstoppable movement. Even if we can agree that an object of great topological laxity must also be fractionally coherent, somewhere between one and many, the object does not for that reason need to somehow be zinging back and forth between those two extremes. In a similar manner, Law asserts somewhat mysteriously that the structure of a fire object will necessarily be star-like: the tangibly present, singular version of the object will serve as a center point, around which will be arrayed the absent multiplicity in a burst of radial linkages. Why the structure of the object must exist in this particular geometric form is unclear. What such a structure accomplishes for us analytically is equally uncertain. It is, like the object's unexplained oscillatory flickering, a seemingly unnecessary detail that serves only to complicate our analysis. Both seem only to be distractions, extraneous features that should be sliced away by Occam's razor.

Thirdly, Law's formulation of fire objects is entirely undiscerning. That is to say, there is no implicit reason that the characteristics of a fire topology must be recognized in some objects and not others. Discussing fire objects, Law and Singleton (2005, p. 8) state that, "we can't understand objects unless we also think of them as sets of present dynamics generated in, and generative of, realities that are necessarily absent." Yet the problem with this statement is that it quite conceivably pertains to all objects. Does not every object, in its immediate and tangible presence, also relate to or depend on or imply the existence of other realities that, while perhaps equally important, are necessarily absent? Is there any object that is not in some way referent to and predicated upon other objects or processes, other realities? Is this not merely another way of making the rather banal statement that not everything that is responsible for an object can be simultaneously present within the observer's field of view? In this regard, there is nothing distinguishing about fire objects; no reason that we must analyze one object as a fire object instead of another. The same lack of selectivity characterizes Law's declaration that fire objects are somehow distinctively transformative in nature. After all, what technical object is *not* transformative? All technical objects, as we have already seen, perform at their core some kind of unique transductive process. All of them are therefore transformative to some degree. There is no technical object that does not function by turning one thing into another slightly different thing. So this aspect of fire objects, again, cannot be a useful criterion for distinguishing a fire technology from any other kind. Law never quite explains why we should not consider all objects to be fire objects.

For all of these reasons, fire objects seem like a rather dubious topological addition to the relationally rigid network objects and the relationally viscous fluid objects that we have already examined. Indeed, this study has shown that it is possible to extend Mol's powerful topological insights without going through the puzzling contortions of Law's fire topology. For, there exists another form of technical object that also incorporates discontinuous variations to its design, is present but not entirely present, exists in a state between singularity and multiplicity, and is drawn loosely together but decentered: a technocenosis. In other words, a technocenosis accomplishes the same topological work as Law's proposed fire object. However, unlike the latter, it also provides a logical continuation of the other major trends that featured so prominently in de Laet and Mol's classic analysis of fluid technologies. The design and production of trawl nets is *even more* decentralized than that of the Zimbabwe bush pump. The degree of success of a trawl net's operative functioning is *even harder* to gauge than that of the bush pump. The similarities between trawl nets are *even more* demonstrative of Wittgenstein's family resemblances than those of various bush pumps. Nor does the notion of a technocenosis rely upon the existence of an inexplicable flickering mechanism or incorporate unsupported presuppositions about the structure of the object's web of relations. It fully does away with such extraneous and arbitrary features of Law's theory. Finally, this study has laid out a specific set of functional and structural criteria that are absolutely required in order for a technocenosis to emerge. We therefore cannot simply call any object a technocenosis without having very good and very specific reasons for doing so. All of this indicates that technocenoses, and not fire objects, represent the next topological step beyond fluid objects in the relational study of technologies.

Following Michel Serres (1978), we might characterize the objects described by this relational topology as *cloud objects*. Serres, I should clarify, did not coin this term in order to evaluate the structure and relations of technical objects. Rather, he employed it to discuss particular, scarcely coherent social aggregations. Serres borrows the term *cloud* from Norbert Wiener, who wrote that it refers "not to [any] one physical situation but to a distribution of possible situations of which only one case is realized" (Wiener 1948, p. 33). For Serres, a cloud therefore appears as a "composite whole that has the somewhat 'shaky' air of a photographic superimposition," a whole in which "the image trembles [and] the definite has lost its borders" (Serres 1978, pp. 14-15). Shaky, superimposed, composite, clouds can only be perceived as such from afar. They lose their characteristic shape when observed from up close. Yet, at a close range the observer can distinguish a useful level of detail that is quickly lost when he or she backs up. As Serres writes, "The distant observer has little information. The information increases as the observer approaches, [and] … is maximal only when the site of observation blends with the site observed" (ibid, p. 17). Yet at such proximity the observer cannot discern any of the order that might exist at a larger scale. The gatherable information about a cloud is therefore only ever partial. Its patterns can be seen from afar but will seem blurry and lacking in detail; its minutiae, on the other hand, can be observed under close examination, but then they lose all relation to the overall pattern. Technocenoses, of course, suffer from exactly this problem. Perceived structural patterns seem to exist across many nets. Yet when individual nets are examined and their details compared, it quickly becomes impossible to say in what way they embody those larger structural patterns. Clouds represent a sort of topological limit: the point at which relational continuity is very nearly, but not quite, preserved. They hint at continuity from afar but are composed of discontinuous individuals.

So when examining technical objects from a topological perspective, we can distinguish between solid objects, fluid objects, and cloud objects. Yet I do not wish to imply that there exist three separate topological *classes* of technical object. Rather, what I am suggesting is that there exists something like a continuous topological spectrum or gradient of technical objects in which technocenoses clearly have a place. This gradient is not structured or defined in advance by any *a priori* typological divisions (network, fluid, cloud), but is continuous and undivided. So while every technical object falls somewhere on the gradient, it does so as a distinct and singular example of itself, not as the member of one topological class of objects or another. All technical objects are in a sense topologically unique, marked by their innate singularity and difference from one another. Accordingly, no object can be said to occupy an innately better or more highly evolved portion of the spectrum than any other: immutable mobiles have specific technical advantages and disadvantages; technocenoses also have theirs.

Along this topological spectrum, the degree to which a physical object and its network of relations can be transformed while remaining invariant increases. At one extreme are rigidly crystalline network objects. Immutable, they cannot be transformed without also destroying their networks of relations. In the middle portion of the spectrum are Mol's somewhat more mutable fluid objects. These can be gradually transformed while still maintaining their intended webs of relations. At the other extreme are cloud objects, technocenoses, which can undergo seemingly drastic transformations without altering the web of functional relations in which they emerged. As we have already seen, cloud objects, like true immutable mobiles, are rare. Very few objects can undergo such drastic and discontinuous structural changes without also becoming dissociated from their functional relations. Of course, as we have seen, this trait also makes it very hard to use the object's structural details to identify those functional relations.

As we move from one end of the topological gradient to the other, any individual object or exemplar also becomes less and less representative of the object's other possible iterations. At one extreme, the technical objects called immutable mobiles are perfectly uniform and exemplary of each other. Examining one such object would teach the examiner about all the others as well. In the middle of the spectrum, any exemplar of a fluid object will be fairly—but not entirely—representative of that object's various iterations. For instance, the structure of any one Zimbabwe bush pump will be a good but not perfect representative of the others, since the elements at the margins of each pump may have undergone gradual modification. Examining a small number of such pumps would probably suffice to identify the relevant pattern of components that makes the Zimbabwe bush pump so unique. At the other extreme, cloud-like technocenoses are so unrepresentative of each other that great numbers of them must be examined for any structural pattern to emerge.

The move from immutable mobiles to technocenoses is therefore also necessarily a move from individuals to populations. It is a move from the Sudoku logic of organic wholes to the demographic logic of assemblages. Indeed, in the analysis of trawl nets we see that technical matter is not somehow exempt from the rules of assemblage theory (DeLanda, 2006), and that the emergence of properly technical assemblages is, in fact, possible. The notion of technocenoses therefore links the relational work of ANT and post-ANT scholars to that of more explicitly Deleuzian scholars such as DeLanda. They provide a crucial linkage between literatures rather than serving as some kind of theoretical cul-de-sac. Technocenoses may be rare, then, but that does not mean they are unrelated to other kinds of object. To say that trawl nets are technocenoses is not to isolate them from other objects, but rather to link them in rather important ways to the entre topological spectrum of technical objects.

Summary of the Argument

Chapter 1 introduces the basic problem of derelict fishing gear in the Northwestern Hawaiian Islands (NWHI). Due to prevailing ocean currents, abandoned trawl fishing nets from all across the North Pacific Ocean accumulate in the NWHI. Once there, they harm the islands' marine environment by entangling organisms and physically damaging the region's delicate coral reefs. In order to mitigate such damages, the National Oceanic and Atmospheric Administration (NOAA) has retrieved roughly 1.5 million pounds of derelict netting from the reefs and nearshore waters of the NWHI. Yet this solution is both expensive and short-lived, since additional pieces of derelict netting wash in continuously from the open sea, quickly replacing any netting that was removed. Net-removal addresses the symptoms of net dereliction, while ignoring its causes. It has become evident that long-term solutions must be preventive instead: they must involve the implementation of measures that are targeted to prevent net abandonment at its sources.

Targeted prevention will only be possible once the primary sources of derelict nets in the NWHI are identified. It is not currently feasible to identify trawl nets by tagging them, so NOAA has instead sought to determine their sources by analyzing the design features of recovered pieces of netting. Indeed, NOAA has created an electronic net identification database specifically for this purpose. Yet, the database has had little success in identifying derelict trawl nets. This is due to the unusual fragmentation of derelict trawls, the inherent complexity of trawl nets themselves, and the multitude of possible source fisheries in the North Pacific. These factors transform the task of identification to one of forensic reconstruction: Trawl nets can be identified only insofar as they are mentally reconstructed from a small handful of their pieces. The purpose of this dissertation is to assess the feasibility of forensically reconstructing trawl nets in this manner, and to therefore evaluate whether the development of a net identification database merits further investments of time, effort, and money.

The possibility of forensic reconstruction relies on the precise nature of the relationship between the parts of a trawl and the whole. In order to evaluate whether NOAA's source identification method is possible, it will therefore be necessary to scrutinize that part-whole relationship. There are basically two forms this relationship might take: trawl nets might be organized in much the same manner as an organism, or they might be organized as an assemblage. Organismal thinking is clearly on the decline in the social sciences, and assemblage thinking is clearly on the ascendency. Yet assemblage theory has never before been successfully applied to technical objects in themselves. In addressing the problem of source identification for derelict trawl nets it will therefore be necessary to address this theoretical lacuna. Indeed, it is only by examining the empirical problem of trawl net source identification that this theoretical gap can be bridged; in turn, confronting the theoretical issue will help make better sense of the empirical problem of source identification. The dissertation therefore seeks to resolve both sets of problems together.

Chapter 2 begins by asking whether the forensic reconstruction of complex functional objects is possible, and if so, what kinds of object are amenable to such a task. It addresses this question through a textual analysis of three authors: the early-19th century comparative anatomist Georges Cuvier; the early-20th century landscape ecologist Frederic Clements; and the mid-20th century philosopher of technical objects, Gilbert Simondon. The chapter begins by showing that Cuvier's famous reconstruction of fossil skeletons relied on three basic principles of structure and function. These principles can collectively referred to as *enclosed multifunctional reticulation*, or EMR. An analysis of Clementsian ecology then shows that Clements incorporated the tenets of EMR into his own theory of botanical "super-organisms". However, he also modified Cuvier's system by incorporating into it a mechanism of dynamic change called autogenic succession. This modified version of EMR is referred to as *progressively enclosed multifunctional reticulation*, or PEMR. Finally, an analysis of Simondon's *Mode of Existence of Technical Objects* shows that Simondon's notion of technical progress, called "concretization" is essentially a recapitulation of PEMR. Accordingly, if trawl nets undergo Simondon's concretizing progress, it is reasonable to expect that their forensic reconstruction will be possible through the tenets of EMR.

Chapter 3 therefore asks whether trawl nets undergo the Simondonian process of concretization. It shows that only the minor and purely mechanical aspects of trawl nets concretize, while the biological aspects of trawl design, those that function exclusively in the presence of fish, categorically fail to concretize. Nothing in *The Mode of Existence* accounts for this failure. However, a closer examination of Simondon's text shows that concretization is possible only for technical objects that function in a specific manner called *mechanical transduction*. Mechanical transduction is characterized, above all else, by the linearity and repeatability of its causes and effects. The chapter concludes that if trawl nets do not concretize, it is perhaps because they function through something other than mechanical transduction.

Chapter 4 takes up this suggestion by examining how it is that trawl nets function; how, in other words, they catch marketable fish (those of the desired size and species) while releasing unmarketable fish. Drawing on an exhaustive review of the fisheries literature on trawl nets, this chapter shows that trawl nets do not just mechanically filter fish out of the water column. Rather, trawl nets are behavioral devices that function by manipulating fishes' ingrained responses to large, moving objects. The operative flows of energy and information in a trawl net pass not through one of Simondon's mechanical transducers, but through living transducers in the form of fish bodies and minds. The functioning of trawl nets must therefore be understood in terms of James Gibson's animal *affordances* rather than through Simondon's system of discrete signals and separate energetic impulses. The chapter illustrates four different ways in which affordances cause trawl nets to function in a non-linear manner. It concludes by demonstrating that this functional non-linearity makes it impossible for trawl nets to concretize.

Chapter 5 asks how trawl nets are assembled and organized, if not through the Simondonian process of concretization. It first points out that design of trawl nets is characterized by a kind of paradox. Different *types* of trawl net are clearly manufactured, but it is never possible to say which of these types an individual net belongs to. In order to see why this is so, Chapter 5 examines the process of trawl engineering. It shows that trawl engineering is primarily a process of selection, in which different preexisting parts are chosen for inclusion within a given trawl. The engineer selects not only a particular kind of mesh cell to include in each part of the trawl, but also the number of those meshes to be included within each netting panel. He or she assembles populations of meshes. This process of selection is functionally under-determined, since many different kinds of mesh and many different sizes of netting panel may be functionally equivalent for a given role within the net. There are accordingly no purely functional criteria for selecting one trawl component over another. Selection is therefore determined in large part by unpredictable or stochastic events that make certain parts available or unavailable to the engineer at any given time. As a result, trawl net designs are extremely variable but not entirely so. The parts are functionally related to each other, but so loosely that any resultant structural patterns can emerge only by observing a very large number of nets.

The chapter then pivots to examine the work of Henry Gleason, whose "individualistic dissent" eventually overturned Clementsian landscape ecology and became the basis for modern ecology. Gleason's dissent was based on his observation of the same structural paradox that it apparent in trawl designs. To explain this paradox he noted that various plant species were functionally equivalent in a given environment: their environmental tolerances or capabilities frequently overlap. Therefore, the establishment in a community of one species, rather than another functionally equivalent one, depended largely on stochastic processes that made each species available or unavailable at the correct time. Community structure was therefore inherently variable. Gleason's intellectual successor, the ecologist Robert Whittaker, showed that this variability did not mean that community structure was wholly random. Rather, since different species were only partially associated with one another, patterns of community structure would only emerge over time, through the observation of many different individual examples. In order to capture this complex notion of structural similarities, Whittaker pioneered the use of clustering and ordination techniques, in which different communities were plotted in a multidimensional phase space whose axes described the many ways in which a community's structure could change.

These two sections make it clear that any explanation of how trawl nets differ from Simondon's organismic technical objects will necessarily restate the Gleasonian dissent in technical terms. Trawl nets and biological communities can therefore be seen as a single kind of object. Both are dynamical systems in which various underdetermined and coexisting populations are assembled together in order to distribute the system's energy. Both depend for their assembly on the same pair of primary selection pressures (functional under-determination & stochastic availability). Both are therefore fractionally coherent objects, meaning that their structural patterns can only emerge across many iterations of assembly. If a biological community is also known as a biocenosis, then we can call the corresponding technical community a technocenosis. A trawl net is a technocenosis.

The comparison between the two kinds of object is not metaphorical. It relies on Manuel DeLanda's observation that when physical systems are modeled as multidimensional phase spaces (manifolds), it becomes clear that very different systems can behave in precisely the same manner. Their manifolds have recurrent features, or common attractors, that cause the different physical systems in question to follow the same trajectories. A salt crystal and a soap bubble, for instance, are materially quite different from one another, and yet their assembly is characterized by a single set of attractors (the minimization of molecular bonding energy). In particular, DeLanda points out that any dynamical system of assembly characterized by selection pressures and the emergence of spontaneous variations will form a kind of "searching device" that evolves over time toward locally optimal solutions. Trawl nets and biological communities are in this sense two different searching devices that are structured by a common set of selection pressures. The material results of each system are quite different, but a shared set of formative pressures guides the assembly of each one. They are the same kind of assemblage.

Having clarified the nature of the relationship between an entire trawl and its component parts, the dissertation returns in **Chapter 6** to the basic empirical question it asked in Chapter 1. Can the design-based analysis of recovered netting fragments be used to identify the sources of derelict trawl nets in the NWHI? The answer is no. Because trawl nets are technocenoses, their structures can only be analyzed with the multivariate statistical methods of clustering and ordination. Yet these methods are prevented, by a number of basic epistemological obstacles, from being used for the identification of individual objects. Because no amount of funding or effort will solve these problems, continued attempts to fix or improve NOAA's net identification database are inadvisable. Net identification should proceed, instead, through other means such as improved methods of tagging.

The dissertation concludes by considering the broader implications of this research for the geographic study of technical objects. It suggests that technocenoses, for a number of reasons, are probably extraordinarily rare. However, this does not mean that they are unrelated to other kinds of technical object. Indeed, an examination of work by Bruno Latour, Annemarie Mol, John Law, and Michel Serres suggests quite the opposite: that the notion of technocenosis effectively unites our current topological understandings of technical objects and sketches out unforeseen continuities between Latourian and Deleuzian strands of relational thought.

Bibliography

Adey, J.M., I.P. Smith, R.J.A. Atkinson, I.D. Tuck, and A.C. Taylor. 2008. 'Ghost fishing' of target and non-target species by Norway lobster *Nephrops norvegicus* creels. *Marine Ecology Progress Series* 366: 119-127.

Al-Masroori, H.S., H. Al-Oufi, J.L. McIlwain, and E. McLean. 2004. Catches of lost fish traps (ghost fishing) from fishing grounds near Muscat, Sultanate of Oman. *Fisheries Research* 69: 407-414.

Andrady, A. L., ed. 2003. *Plastics and the Environment*. New York: John Wiley & Sons.

———2000. *Plastics and their impacts in the marine environment*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 137-143.

———1990. *Environmental degradation of plastics under land and marine exposure conditions*. Paper presented at: Proceedings of the Second International Conference on Marine Debris, 2-7 April, 1989, Honolulu, Hawai'i: U.S. Department of Commerce, NOAA Technical Memorandum, NMFS, NOAA-TM-NMFS-SWFC-154, pp. 848-869.

Anonymous. 2006. *Northwestern Hawaiian Islands Marine National Monument: A Citizen's Guide*. Honolulu, Hawai'i: National Oceanic and Atmospheric Association, United States Fish and Wildlife Service, and State of Hawaii.

Appel, T.A. 1987. *The Cuvier-Geoffroy Debate: French biology in the decades before Darwin*. New York and Oxford: Oxford University Press.

Arnott, S.A., D.M. Neil, and A.D. Ansell. 1999. Escape trajectories of the brown shrimp *Crangon crangon*, and a theoretical consideration of initial escape angles from predators. *The Journal of Experimental Biology* 202: 193-209.

Ayaz, A., D. Acarli, U. Altinagac, U. Ozekinci, A. Kara, and O. Ozen. 2006. Ghost fishing by monofilament and multifilament gillnets in Izmir Bay, Turkey. *Fisheries Research* 79: 267-271.

Baeta, F., M.J. Costa, and H. Cabral. 2009. Trammel nets' ghost fishing off the Portuguese central coast. *Fisheries Research* 98: 33-39.

Barnes, T.J. 2008. Geography's underworld: The military-industrial complex, mathematical modeling and the quantitative revolution. *Geoforum* 39: 3-16.

Barry, A. 2005. Pharmaceutical matters: the invention of informed materials. *Theory, Culture & Society* 22: 51-69.

Beutel, D., L. Skrobe, K. Castro, P. Ruhle Sr., P. Ruhle Jr., J. O'Grady, and J. Knight. 2008. Bycatch reduction in the Northeast USA directed haddock bottom trawl fishery. *Fisheries Research* 94: 190-198.

Bickerstaff, K., and J. Agyeman. 2009. Assembling justice spaces: The scalar politics of environmental justice in North-east England. *Antipode* 41: 781-806.

Bloomfield, B.P., Y. Latham, and T. Vurdubakis. 2010. Bodies, technologies and action possibilities: When is an affordance? *Sociology* 44: 415-433.

Boland, R.C. 1997. *A preliminary survey of the underwater accumulation of derelict nets at French Frigate Shoal*. Honolulu: Honolulu Laboratory, Southwest Fisheries Science Center, National Marine Fisheries Service, Administrative Report H-97-13.

Boland, R.C., and M.J. Donohue. 2003. Marine debris accumulation in the nearshore marine habitat of the endangered Hawaiian monk seal, Monachus schauinslandi 1999-2001 *Marine Pollution Bulletin* 46: 1385-1394.

Brainard, R.E., D.G. Foley, and M.J. Donohue. 2000. *Origins, types, distribution, and magnitude of derelict fishing gear*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 24-39.

Bridge, G. 2003. The next new thing? Biotechnology and its discontents. *Geoforum* 34: 165-174.

Broadhurst, M.K. 2000. Modifications to reduce bycatch in prawn trawls: A review and framework for development. *Reviews in Fish Biology and Fisheries* 10: 27-60.

Brown, J., and G. Macfadyen. 2007. Ghost fishing in European waters: Impacts and management responses. *Marine Policy* 31: 488-504.

Brown, J., G. Macfadyen, T. Huntington, J. Magnus, and J. Tumilty. 2005. *Ghost fishing by lost fishing gear*. Institute for European Environmental Policy/Poseidon Aquatic Resource Management Ltd Joint Report, Final report to DG Fisheries and Maritime Affairs of the European Commission. Fish/2004/20.

Bublitz, C.G. 1996. Quantitative evaluation of flatfish behavior during capture by trawl gear. *Fisheries Research* 25: 293-304.

Bullimore, B.A., P.B. Newman, M.J. Kaiser, S.E. Gilbert, and K.M. Lock. 2001. A study of catches in a fleet of 'ghost-fishing' pots. Statistical data included. *Fishery Bulletin* 99: 247-253.

Campbell, M.J., and W.D. Sumpton. 2009. Ghost fishing in the pot fishery for blue swimmer crabs *Portunus pelagicus* in Queensland, Australia. *Fisheries Research* 95: 246-253.

Cappiello, D. 2008. Marine monument languishes. *Honolulu Star Bulletin*. Available online at http://archives.starbulletin.com/2008/08/08/news/story07.html (last accessed 1/14 2010).

Carr, A., and J. Harris. 1997. Ghost-fishing gear: Have fishing practices during the past few years reduced the impact? In *Marine Debris: Sources, Impacts and Solutions,* ed. J. M. Coe and A. B. Rogers, pp. 141-151. New York: Springer-Verlag.

Carroll, P.S. 2000. A U.S. Perspective on MARPOL V: Compliance, Enforcement, and Implementation. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp.77-84

Catchpole, T.L., and A.S. Revill. 2008. Gear technology in *Nephrops* trawl fisheries. *Reviews in Fish Biology and Fisheries* 18: 17-31.

Chance, M.R.A., and W.M.S. Russell. 1959. Protean displays: a form of allaesthetic behaviour. *Proceedings of the Zoological Society of London* 132: 65-70.

Chosid, D., M. Pol, M. Szymanski, L. Ribas, and T. Moth-Poulsen. 2008. *Further testing of cod-avoiding trawl net designs*. National Oceanic and Atmospheric Association, NOAA/NMFS Saltonstall-Kennedy Program Completion Report. Grant no. NA03NMF4270139 (02-NER-040).

Clapp, R.B., and W.O. Wirtz II. 1975. The natural history of Lisianski Island, Northwestern Hawaiian Islands. *Atoll Research Bulletin* 186: 1-196.

Clements, F.E. 1936. Nature and structure of the climax. *The Journal of Ecology* 24: 252-284.

———1916. *Plant Succession: An Analysis of the Development of Vegetation*. Washington, D.C.: The Carnegie Institution of Washington.

Cody, M.L., and J.M. Diamond, eds. 1975. *Ecology and Evolution of Communities*. Cambridge, Massachusetts: Harvard University Press.

Coleman, W. 1964. *Georges Cuvier, Zoologist: A Study in the History of Evolution Theory*. Cambridge, Massachusetts: Harvard University Press.

Constantino, M.A., and M. Salmon. 2003. Role of chemical and visual cues in food recognition in leatherback posthatchlings (*Dermochelys coriacea* L). *Zoology* 106: 173-181.

Cronon, W. 1995. The trouble with wilderness. In *Uncommon Ground: Rethinking the Human Place in Nature,* ed. W. Cronon, pp. 69-90. New York: W.W. Norton & Co.

Cuvier, G. 1980 (1812). *Memoirs on Fossil Elephants and on Reconstruction of the Genera* Paleotherium *and* Anoplotherium. New York: Arno Press.

———1978 (1827). *Essay on the Theory of the Earth*. New York: Arno Press.

———1835. *Researches on Fossil Bones: in which are established the characters of various animals whose species have been destroyed by the revolutions of the globe*. 4th English edition. London: G. Henderson.

———1831. *Animal Kingdom: arranged in conformity with its organization*. Translated by H. M'Murtrie. London: Orr and Smith.

———1802. *Lectures on Comparative Anatomy*. Translated by James Macartney and William Ross. London: T.N. Longman and O. Rees.

———1798. Extract from a memoir on an animal of which the bones are found in plaster stone around Paris, and which appears no longer to exist alive today. Paper read at the public session of the National Institute on 6 October 1798. Manuscript MS 628, Bibliothèque Central, Muséum National d'Histoire Naturelle: Paris.

———n.d. Essay on zoological analogies. Unpublished manuscript. Item 94, ff.382-388, in the *Catalogue des manuscrits du fonds Cuvier* (H. Dehérain, 1908). Bibliothèque de l'Institut de France: Paris.

Dameron, O.J., M. Parke, M.A. Albins, and R. Brainard. 2007. Marine debris accumulation in the Northwestern Hawaiian Islands: An examination of rates and processes. *Marine Pollution Bulletin* 54: 423-433.

Dant, T. 2004. The driver-car. *Theory, Culture & Society* 21: 61-79.

De Boever, A., A. Murray, and J. Roffe. 2009. "Technical mentality" revisited: Brian Massumi on Gilbert Simondon. *Parrhesia* 7: 36-45.

de Laet, M., and A. Mol. 2000. The Zimbabwe bush pump: mechanics of a fluid technology. *Social Studies of Science* 30: 225-263.

DeLanda, M. 2006. *A New Philosophy of Society: Assemblage Theory and Social Complexity*. New York: Continuum.

———2003. Virtual environments and the emergence of synthetic reason. In *Flame Wars: The Discourse of Cyberculture,* ed. M. Dery, pp. 793-815. Durham: Duke University Press.

———2002. *Intensive Science and Virtual Philosophy*. London and New York: Continuum.

———1994. Virtual environments and the emergence of synthetic reason. In *Flame Wars: The Discourse of Cyberculture*, ed. M. Dery, pp. 263-286. Durham: Duke University Press.

Deleuze, G., and F. Guattari. 1987. *A Thousand Plateaus: Capitalism and Schizophrenia*. Minneapolis: University of Minnesota Press.

DeMartini, E.E., and A.M. Friedlander. 2004. Spatial patterns of endemism in shallow-water reef fish populations of the Northwestern Hawaiian Islands. *Marine Ecology Progress Series* 271: 281-296.

Donohue, M.J. 2005. Eastern Pacific Ocean source of North-western Hawaiian Islands marine debris supported by errant fish aggregating device. *Marine Pollution Bulletin* 50: 886-888.

———2000. *Derelict fishing gear monitoring and removal*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 85-90.

Donohue, M.J., R.C. Boland, C. Sramek, and G. Antonelis. 2000. *Derelict fishing gear in the Northwestern Hawaiian Islands: Diving surveys and debris removal at two atolls confirms threat to coral reef ecosystems*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, p. 287.

Donohue, M.J., R.C. Boland, C.M. Sramek, and G.A. Antonelis. 2001. Derelict fishing gear in the Northwestern Hawaiian Islands: Diving surveys and debris removal in 1999 confirm threat to coral reef ecosystems. *Marine Pollution Bulletin* 42: 1301-1312.

Donohue, M.J., R.E. Brainard, M. Parke, and D. Foley. 2000. *Mitigation of environmental impacts of derelict fishing gear through debris removal and environmental monitoring*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 383-402.

Driver, P.M., and D.A. Humphries. 1970. Protean defense by prey animals. *Oecologia* 5: 285-302.

Dufrene, M., and P. Legendre. 1997. Species assemblages and indicator species: the need for a flexible asymmetrical approach. *Ecological Monographs* 67: 345-366.

Eayrs, S. 2007. *A Guide to Bycatch Reduction in Tropical Shrimp-Trawl Fisheries*. Rome: Food and Agriculture Organization of the United Nations.

Engås, A., D. Foster, B.D. Hataway, J.W. Watson, and I. Workman. 1999. The behavioral response of juvenile red snapper (*Lutjanus campechanus*) to shrimp trawls that utilize water flow modifications to induce escapement. *Marine Technology Society Journal* 33: 43-50.

Enger, P.S., H.E. Karlsen, F.R. Knudsen, and O. Sand. 1993. Detection and reaction of fish to infrasound. *ICES Marine Science Symposia: Actes du Symposium* 196: 108-112.

Erzini, K., C.C. Monteiro, J. Ribeiro, M.N. Santos, M. Gaspar, P. Monteiro, and T.C. Borges. 1997. An experimental study of gillnet and trammel net ‘ghost fishing’ off the Algarve (southern Portugal). *Marine Ecology Progress Series* 158: 257-265.

Fridman, A.L. 1986. *Calculations for Fishing Gear Designs*. Farnham, UK: Fishing News Books Limited.

———1973. *Theory and Design of Commercial Fishing Gear*. Jerusalem: Israel Program for Scientific Translations.

Gibson, J.J. 1986. *The Ecological Approach to Visual Perception*. New York: Psychology Press.

Gilardi, K.V.K., D. Carlson-Bremer, J.A. June, K. Antonelis, G. Broadhurst, and T. Cowan. 2010. Marine species mortality in derelict fishing nets in Puget Sound, WA and the cost/benefits of derelict net removal. *Marine Pollution Bulletin* 60: 376-382.

Gleason, H.A. 1939. The individualistic concept of the plant association. *American Midland Naturalist* 21: 92-110.

———1926. The individualistic concept of the plant association. *Bulletin of the Torrey Botanical Club* 53: 7-26.

———1917. The structure and development of the plant association. *Bulletin of the Torrey Botanical Club* 44: 463-481.

Godø, O.R., and V. Wespestad. 1993. Monitoring changes in abundance of gadoids with varying availability to trawl and acoustic surveys. *ICES Journal of Marine Science* 50: 39-51.

Godøy, H., D.M. Furevik, and S. Stiansen. 2003. Unaccounted mortality of red king crab (*Paralithodes camtschaticus*) in deliberately lost pots off Northern Norway. *Fisheries Research* 64: 171-177.

Gower, J.C. 1971. A general coefficient of similarity and some of its properties. *Biometrics* 27: 857-871.

Graham, K.J., M.K. Broadhurst, and R.B. Millar. 2008. Effects of codend circumference and twine diameter on selection in south-eastern Australian fish trawls. *Fisheries Research* 95: 341-349.

Graham, N., E.G. Jones, and D.G. Reid. 2004. Review of technological advances for the study of fish behavior in relation to demersal fishing trawls. *ICES Journal of Marine Science* 61: 1036-1043.

Grimaldo, E., M. Sistiaga, and R.B. Larsen. 2008. Evaluation of codends with sorting grids, exit windows, and diamond meshes: Size selection and fish behavior. *Fisheries Research* 91: 271-280.

Hameed, M.S., and M.R. Boopendranath. 2000. *Modern Fishing Gear Technology*. Delhi: Daya Publishing House.

Handegard, N.O., and D. Tjøstheim. 2009. The sampling volume of trawl and acoustics: estimating availability probabilities from observations of tracked individual fish. *Canadian Journal of Fisheries and Aquatic Sciences* 66: 425-438.

———2005. When fish meet a trawling vessel: examining the behaviour of gadoids using a free-floating buoy and acoustic split-beam tracking. *Canadian Journal of Fisheries and Aquatic Sciences* 62: 2409-2422.

Hanlon, R.T., and J.B. Messenger. 1996. *Cephalopod Behaviour*. Cambridge, UK: Cambridge University Press.

Hannah, R.W., and S.A. Jones. 2007. Effectiveness of bycatch reduction devices (BRDs) in the ocean shrimp (*Pandalus jordani*) trawl fishery. *Fisheries Research* 85: 217-225.

Hannah, R.W., S.A. Jones, and K.M. Matteson. 2007. *Observations of fish and shrimp behavior in ocean shrimp (Pandalus jordani) trawls*. Newport, Oregon: Oregon Department of Fish and Wildlife, Marine Resources Program.

Harden Jones, F.R. 1963. The reaction of fish to moving backgrounds. *Journal of Experimental Biology* 40: 437-446.

Hareide, N.-R., G. Garnes, D. Rihan, M. Mulligan, P. Tyndall, M. Clark, P. Connolly, R. Misund, P. McMullen, D. Furevik, O.B. Humborstad, K. Høydal, and T. Blasdale. 2005. *A preliminary investigation on shelf edge and deep-water fixed net fisheries to the West and North of Great Britain, Ireland, around Rockall and Hatton Bank*. Bord Iascaigh Mhara, Fiskeridirecktoratet, NEAFC, Sea Fish Industry Authority, Joint Nature Conservation Committee, Marine Institute Foras na Mara.

Harris, R.R., and M. Ulmestrand. 2004. Discarding Norway lobster (*Nephrops norvegicus* L.) through low salinity layers – mortality and damage seen in simulation experiments. *ICES Journal of Marine Science* 61: 127-139.

He, P., ed. 2010. *Behavior of Marine Fishes: Capture Processes and Conservation Challenges*. Singapore: Wiley-Blackwell.

Heinemann, D., H. Gillelan, and L. Morgan. 2005. *Bottomfish fishing in the Northwestern Hawaiian Islands: Is it ecologically sustainable?* Washington, D.C.: The Ocean Conservancy and Marine Conservation Biology Institute, .

Henderson, J.R. 2001. A pre- and post-MARPOL Annex V summary of Hawaiian monk seal entanglements and marine debris accumulation in the Northwestern Hawaiian Islands. *Marine Pollution Bulletin* 42: 584-589.

———1990. *Recent entanglements of Hawaiian monk seals in marine debris*. Paper presented at: Proceedings of the Second International Conference on Marine Debris, 2-7 April, 1989, Honolulu, Hawai'i: United States Department of Commerce, NOAA Technical Memorandum, NMFS, NOAA-TM-NMFS-SWFSC-154, pp. 540-553.

Henderson, J.R., and R. Steiner. 2000. *Source identification of derelict fishing gear: Issues and concerns*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 358-363.

Hinchliffe, S. 2008. Reconstituting nature conservation: Towards a careful political ecology. *Geoforum* 39: 88-97.

Humborstad, O.-B., S. Løkkeborg, N.-R. Hareide, and D.M. Furevik. 2003. Catches of Greenland halibut (*Reinhardtius hippoglossoides*) in deepwater ghost-fishing gillnets on the Norwegian continental slope. *Fisheries Research* 64: 163-170.

Hutchby, I. 2001. Technologies, texts and affordances. *Sociology* 35: 441-456.

Hutchinson, G.E. 1957. Concluding remarks. *Cold Spring Harbor Symposia on Quantitative Biology* 22: 415-427.

ICES. 2008. *Report of the ICES-FAO Working Group on Fish Technology and Fish Behavior (WGFTFB), 21-25 April 2008, Tórshavn, Faroe Islands*. Report Number, ICES CM 2008/FTC:02.

INAPESCA. Catálogo de Sistemas de Captura: Capítulo I. Arrastre. Available online at http://www.inapesca.gob.mx/portal/component/content/article/17-publicaciones/77-catalogo-de-sistemas-de-captura (last accessed 7/3 2010).

Ingólfson, O.A., and T. Jørgensen. 2006. Escapement of gadoid fish beneath a commercial bottom trawl: Relevance to the overall trawl selectivity. *Fisheries Research* 79: 303-312.

International Fisheries Exhibition. 1884. *Fisheries Exhibition Literature, Vol. IV*. London: William Clowes and Sons.

Itaya, K., Y. Fujimori, S. Shimizu, T. Komatsu, and T. Miura. 2007. Effect of towing speed and net mouth size on catch efficiency in framed midwater trawls. *Fisheries Science* 73: 1007-1016.

Jones, E.G., K. Summerbell, and F. O'Neill. 2008. The influence of towing speed and fish density on the behaviour of haddock in a trawl cod-end. *Fisheries Research* 94: 166-174.

Kaiser, M.J., B. Bullimore, P. Newman, K. Lock, and S. Gilbert. 1996. Catches in ‘ghost fishing’ set nets. *Marine Ecology Progress Series* 145: 11-16.

Kearnes, M. 2007. (Re)making matter: design and selection. *Area* 39: 143-155.

———2006. Chaos and control: nanotechnology and the politics of emergence. *Paragraph* 29: 57-80.

Kiessling, I. 2007. *Finding Solutions: Derelict fishing gear and other marine debris in northern Australia*. Hobart: Australia National Oceans Office.

Kim, Y.-H., and C.S. Wardle. 2005. Basic modelling of fish behaviour in a towed trawl based on chaos in decision-making. *Fisheries Research* 73: 217-229.

———2003. Optomotor response and erratic response: quantitative analysis of fish reaction to towed fishing gears. *Fisheries Research* 60: 455-470.

———1998. Measuring the brightness contrast of fishing gear, the visual stimulus for fish capture. *Fisheries Research* 34: 151-164.

Kim, Y.-H., C.S. Wardle, and Y.-S. An. 2008. Herding and escaping responses of juvenile roundfish to square mesh window in a trawl cod end. *Fisheries Science* 74: 1-7.

Kostyunin, Y.N. 1971. *Trawls and Trawling*. Jerusalem: Israel Program for Scientific Translations.

Krag, L.A., R. Holst, and N. Madsen. 2009. The vertical separation of fish in the aft end of a demersal trawl. *ICES Journal of Marine Science* 66: 772-777.

Krag, L.A., N. Madsen, and J.D. Karlsen. 2009. A study of fish behavior in the extension of a demersal trawl using a multi-compartment separator frame and SIT camera system. *Fisheries Research* 98: 62-66.

Kubota, M. 1994. A mechanism for the accumulation of floating marine debris north of Hawaii. *Journal of Physical Oceanography* 24: 1059-1064.

Küchler, S. 2008. Technological materiality: beyond the dualist paradigm. *Theory, Culture & Society* 25: 101-120.

Laist, D.W. 1994. Entanglement of Marine Life in Marine Debris. *Draft paper prepared for the Third International Conference on Marine Debris, 8-13 May 1994, Miami, Florida*. Cited in: Macfadyen, G., Huntington, T., and Cappell, R. 2009. Abandoned, lost or otherwise discarded fishing gear. UNEP Regional Seas Reports and Studies, 185: FAO Fisheries and Aquaculture Technical Paper, 523. Rome.

Laist, D.W., and M. Liffman. 2000. *Impacts of marine debris: research and management needs*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 344-357.

Landeau, L., and J. Terborgh. 1986. Oddity and the ‘confusion effect’ in predation. *Animal Behavior* 34: 1372-1380.

Langedal, G., and A. Drivenes. Opprensking av tapte fiskeredskaper i 2009. Bergen: Fiskeridirektoratet. Available online at http://www.fiskeridir.no/content/download/18868/177848/version/1/file/rapport-tapteredskap-2009.pdf (last accessed 7/3 2010).

Latour, B. 2002. Morality and technology: The end of the means. *Theory, Culture & Society* 19: 247-260.

———1999. *Pandora’s Hope: Essays on the Reality of Science Studies*. Cambridge, Massachusetts: Harvard University Press.

———1996. *Aramis, or the Love of Technology*. Cambridge, Massachusetts: Harvard University Press.

———1993. *We Have Never Been Modern*. Cambridge, Massachusetts: Harvard University Press.

———1987. *Science in Action: How to Follow Scientists and Engineers Through Society*. Cambridge, Massachusetts: Harvard University Press.

Law, J. 2002. *Aircraft Stories: Decentering the Object in Technoscience*. Durham: Duke University Press.

Law, J., and A. Mol. 2001. Situating technoscience: an inquiry into spatialities. *Environment and Planning D: Society and Space* 19: 609-621.

Law, J., and V. Singleton. 2005. Object lessons. *Organization* 12: 331-355.

Legendre, P., and L. Legendre. 1998. *Numerical Ecology*. Amsterdam: Elsevier.

Legg, S. 2009. Of scales, networks and assemblages: The League of Nations apparatus and the scalar sovereignty of the Government of India. *Transactions of the Institute of British Geographers* 34: 234-253.

Lovell, H., and S.J. Smith. 2010. Agencement in housing markets: The case of the UK construction industry. *Geoforum* 41: 457-468.

Macfadyen, G., T. Huntington, and R. Cappell. 2009. *Abandoned, lost or otherwise discarded fishing gear*. UNEP Regional Seas Reports and Studies, 185; FAO Fisheries and Aquaculture Technical Paper, 523. Rome: United Nations Environment Programme, Food and Agriculture Organization of the United Nations.

MacFarlane, C. 2009. Translocal assemblages: Space, power and social movements. *Geoforum* 40: 561-567.

Mackenzie, A. 2002. *Transductions: Bodies and Machines at Speed*. London & New York: Continuum.

Madsen, N. 2007. Selectivity of fishing gears used in the Baltic Sea cod fishery. *Reviews in Fish Biology and Fisheries* 17: 517-544.

Madsen, N., R. Skeide, M. Breen, L.A. Krag, I. Huse, and A.V. Soldal. 2008. Selectivity in a trawl codend during haul-back operation—An overlooked phenomenon. *Fisheries Research* 91: 168-174.

Manjarrés, L., L.O. Duarte, J. Altamar, F. Escobar, C. Garcia, and F. Cuello. 2008. Effects of using bycatch reduction devices on the Colombian Caribbean Sea shrimp fishery. *Ciencias Marinas* 34: 223-238.

MARTRAWL, I. 1998. Improved cell design for a trawl system and methods. International patent number WO 98/46070. Accessible online at http://www.wipo.int/pctdb/en/wo.jsp?WO=1998046070.

Matsumura, S., and K. Nasu. 1997. Distribution of floating marine debris in the North Pacific Ocean: Sighting surveys 1986–1991. In *Marine Debris: Sources, Impacts and Solutions,* ed. J. M. Coe and D. B. Roberts, New York: Springer-Verlag.

Matsuoka, T., T. Nakashima, and N. Nagasawa. 2005. A review of ghost fishing: scientific approaches to evaluation and solutions. *Fisheries Science* 71: 691-702.

Mayr, E. 1982. *The Growth of Biological Thought*. Cambridge, Massachusetts: Belknap Press of Harvard University.

McClain, C.R., S.R. Signorini, and J.R. Christian. 2002. Subtropical gyre variability observed by ocean color satellites. NASA Technical Memorandum, NASA/TM—2002–211616. Greenbelt, Maryland: Goddard Space Flight Center.

McIntosh, R.P. 1975. H. A. Gleason—"Individualistic Ecologist" 1882-1975: His contributions to ecological theory. *Bulletin of the Torrey Botanical Club* 102: 253-273.

Merryweather, G. 1851. *An Essay Explanatory of the Tempest Prognosticator in the Building of the Great Exhibition for the Works of Industry of All Nations*. London: John Churchill.

Miller, G. 2000. *The Mating Mind: How Sexual Choice Shaped the Evolution of Human Nature*. New York: Doubleday.

Minton, M. 2000. *Industry considerations and actions*. Paper presented at: Proceedings of the International Marine Debris Conference on Derelict Fishing Gear and the Ocean Environment, 6-11 August, 2000, Honolulu, Hawai'i: National Oceanic and Atmospheric Association and the Hawaiian Islands Humpback Whale National Marine Sanctuary, pp. 364-382.

Mitson, R.B., and H.P. Knudsen. 2003. Causes and effects of underwater noise on fish abundance estimation. *Aquatic Living Resources* 16: 255-263.

Mol, A. 2002. *The Body Multiple: Ontology in Medical Practice*. Durham and London: Duke University Press.

Mol, A., and J. Law. 1994. Regions, networks and fluids: anaemia and social topology. *Social Studies of Science* 24: 641-671.

Morishige, C., M.J. Donohue, E. Flint, C. Swenson, and C. Woolaway. 2007. Factors affecting marine debris deposition at French Frigate Shoals, Northwestern Hawaiian Islands Marine National Monument, 1990-2006. *Marine Pollution Bulletin* 54: 1162-1169.

National Research Council. 2008. *Tackling Marine Debris in the 21st Century (prepublication draft)*. Washington, D.C.: The National Academies Press. Available online at: http://www.nap.edu/catalog/12486.html.

NET Systems. 2009. Mexican owners switching to Ultra Cross. Available online at: http://www.net-sys.com/news.php?id=1 (last accessed 8/25 2010).

Nicolson, M. 1990. Henry Allan Gleason and the individualistic hypothesis: The structure of a botanist's career. *The Botanical Review* 56: 92-161.

NOAA Marine Debris Program. 2010. Marine debris survey and removal from the Papahanaumokuakea (Northwestern Hawaiian Islands) Marine National Monument. Available online at: http://marinedebris.noaa.gov/projects/removal_nwhi.html (last accessed 6/24 2010).

Nomura, M. 1969. Preliminary study of characteristics of trawl net construction in Thailand. *IPFC Current Affairs Bulletin* 54/55: 11-18.

Norman, D.A. 1988. *The Psychology of Everyday Things*. New York: Basic Books.

Ong, A. 2006. Mutations in citizenship. *Theory, Culture & Society* 23: 499-505.

Outram, D. 1986. Uncertain legislator: Georges Cuvier's laws of nature in their intellectual context. *Journal of the History of Biology* 19: 323-368.

Özbilgin, H., R.S.T. Ferro, J.H.B. Robertson, G. Holtrop, and R.J. Kynoch. 2006. Seasonal variation in trawl codend selection of northern North Sea haddock. *ICES Journal of Marine Science* 63: 737-748.

Papahānaumokuākea Marine National Monument. 2008. *Papahānaumokuākea Marine National Monument Management Plan, v. 1-3*. Honolulu: National Oceanic and Atmospheric Association, United States Fish and Wildlife Service, and State of Hawaii.

Parente, J., P. Fonseca, V. Henriques, and A. Campos. 2008. Strategies for improving fuel efficiency in the Portuguese trawl fishery. *Fisheries Research* 93: 117-124.

Parker, C. 2009. Tunnel-bypasses and minarets of capitalism: Amman as neoliberal assemblage. *Political Geography* 28: 110-120.

Pichel, W.G., J.H. Churnside, T.S. Veenstra, D.G. Foley, K.S. Friedman, R.E. Brainard, J.B. Nicoll, Q. Zheng, and P. Clemente-Colón. 2007. Marine debris collects within the North Pacific Subtropical Convergence Zone. *Marine Pollution Bulletin* 54: 1207-1211.

Prudham, S. 2007. The fictions of autonomous invention: accumulation by dispossession, commodification and life patents in Canada. *Antipode* 39: 406-429.

Putnam, R.J. 1994. *Community Ecology*. Dordrecht and Norwell, Massachusetts: Kluwer Academic Publishers.

Queirolo, D., H. DeLouche, and C. Hurtado. 2009. Comparison between dynamic simulation and physical model testing of new trawl design for Chilean crustacean fisheries. *Fisheries Research* 97: 86-94.

Quevedo, J.A.H. 2001. Shrimp trawl design improvements suggested for Mexican fisheries. Final Project, Fisheries Training Program, United Nations University, Reykjavik. Available online at: http://www.unuftp.is/static/fellows/document/joseprf.pdf .

Reid, D.G., V.J. Allen, D.J. Bova, E.G. Jones, R.J. Kynoch, K.J. Peach, P.G. Fernandes, and W.R. Turrell. 2007. Anglerfish catchability for swept-area abundance estimates in a new survey trawl. *ICES Journal of Marine Science* 64: 1503-1511.

Robertson, M.M. 2006. The nature that capital can see: science, state, and market in the commodification of ecosystem services. *Environment and Planning D: Society and Space* 24: 367-387.

Roden, G.I. 1980. On the subtropical frontal zone north of Hawaii during winter. *Journal of Physical Oceanography* 10: 342-362.

———1975. On North Pacific temperature, salinity, sound velocity, and density fronts and their relation to the wind and energy flux fields. *Journal of Physical Oceanography* 5: 557-571.

Rollefsen, G. 1962. Fisken og dens gåter: Norsk fiskeriforskning fram til i dag. In *Havet og våre fisker,* ed. G. Rollefsen, G. Dannevig, H. Mosby, K. Sunnanå and H. Tambs-Lyche, pp. 30-55. Bergen, Norway: J.W. Eides Forlag.

Rose, N. 2001. The politics of life itself. *Theory, Culture & Society* 18: 1-30.

Roughgarden, J. 2009. Is there a general theory of community ecology? *Biology and Philosophy* 24: 521-529.

Rudwick, M.J.S. 1997. *Georges Cuvier, Fossil Bones, and Geological Catastrophes: New Translations and Interpretations of the Primary Texts*. Chicago: University of Chicago Press.

Ryer, C.H. 2008. A review of flatfish behavior relative to trawls. *Fisheries Research* 90: 138-146.

Ryer, C.H., and L.A.K. Barnett. 2006. Influence of illumination and temperature upon flatfish reactivity and herding behavior: Potential implications for trawl capture efficiency. *Fisheries Research* 81: 242-250.

Sainsbury, J.C. 1996. *Commercial Fishing Methods: An Introduction to Vessels and Gears*. Oxford: Fishing News Books.

Sancho, G., E. Puente, A. Bilbao, E. Gomez, and L. Arregi. 2003. Catch rates of monkfish (*Lophius* spp.) by lost tangle nets in the Cantabrian Sea (northern Spain). *Fisheries Research* 64: 129-139.

Santos, M.N., M.B. Gaspar, and C.C. Monteiro. 2009. Ghost fishing on by-catch species from a gill net hake fishery. *Fisheries Management and Ecology* 16: 72-74.

Santos, M.N., H. Saldanha, M. Gaspar, and C. Monteiro. 2003a. Hake (*Merluccius merluccius* L., 1758) ghost fishing by gillnets off the Algarve (southern Portugal). *Fisheries Research* 64: 119-128.

Santos, M.N., H. Saldanha, M.B. Gaspar, and C.C. Monteiro. 2003b. Causes and rates of net loss off the Algarve (southern Portugal). *Fisheries Research* 64: 115-118.

Scandol, J.P., T.J. Underwood, and M.K. Broadhurst. 2006. Experiments in gear configuration to reduce bycatch in an estuarine squid-trawl fishery. *Fishery Bulletin* 104: 533-541.

Schmidt, M. B. 2009. Reactions of vendace (*Coregonus albula*, Linnaeus 1758) towards an approaching pelagic pair-trawl observed by split-beam echosounding. *Fisheries Research* 96: 95-101.

Serres, M., W. Woodhull, and J. Mowitt. 1978. Exact and human. *SubStance* 6: 9-19.

Shaw, I.G.R., P.F. Robbins, and J.P. Jones. 2010. A bug's life and the spatial ontologies of mosquito management. *Annals of the Association of American Geographers* 100: 373-392.

Simondon, G. 2009. Technical mentality. *Parrhesia* 7: 17-27.

———2007 (1958). *El modo de existencia de los objetos técnicos*. Buenos Aires: Prometeo Libros.

Sneath, P.H.A., and R.R. Sokal. 1973. *Numerical Taxonomy: The Principles and Practice of Numerical Classification*. San Francisco: W.H. Freeman and Company.

Starr, F., and K. Martz. 1999. *S. S. Midway Expedition, May 21, 1999 – June 16, 1999: Trip Report*. Honolulu: United States Fish and Wildlife Service.

Starr, F., K. Martz, and L. Loope. 2001. *Botanical Inventory of Kure Atoll*. Honolulu: Hawai'i Department of Land and Natural Resources, Division of Forestry and Wildlife.

Stiegler, B. 1998. *Technics and Time: The Fault of Epimetheus*. Translated by R. Beardsworth and G. Collins. Stanford: Stanford University Press.

Thrift, N. 2005. From born to made: technology, biology and space. *Transactions of the Institute of British Geographers* 30: 463-476.

Tilman, D. 2004. Niche tradeoffs, neutrality, and community structure: A stochastic theory of resource competition, invasion, and community assembly. *PNAS* 101: 10854-10861.

Timmers, M.A., C.A. Kistner, and M.J. Donohue. 2005. *Marine Debris of the Northwestern Hawaiian Islands: Ghost Net Identification*. Sea Grant Publication: UNIHI-SEAGRANT-AR-05-01. Honolulu: University of Hawaii Sea Grant College Program.

Topping, P., D. Morantz, and G. Lang. 1997. Waste disposal practices of fishing vessels: Canada's East Coast 1990-1991. In *Marine Debris: Sources, Impacts and Solutions,* ed. J. M. Coe and A. B. Rogers, pp. 253-262. New York: Springer-Verlag.

Tschernij, V., and P.O. Larsson. 2003. Ghost fishing by lost cod gillnets in the Baltic Sea. *Fisheries Research* 64: 151-162.

Uchida, R.N., and J.H. Uchiyama. 1986. *Fishery Atlas of the Northwestern Hawaiian Islands*. NOAA Technical Report NMFS 38. Washington, D.C.: United States Department of Commerce, National Oceanic and Atmospheric Administration, and National Marine Fisheries Service.

U.S. Commission on Ocean Policy. 2004. *An Ocean Blueprint for the 21st Century*. Final Report. Washington, D.C.

Vabø, R., K. Olsen, and I. Huse. 2002. The effect of vessel avoidance of wintering Norwegian spring spawning herring. *Fisheries Research* 58: 59-77.

Wardle, C.S. 1993. Fish behaviour and fishing gear. In *Behaviour of Teleost Fishes,* ed. T. J. Pitcher, pp. 609-644. London: Chapman & Hall.

Watson, A., and O.H. Huntington. 2008. They're here—I can feel them: The epistemic spaces of Indigenous and Western Knowledges. *Social and Cultural Geography* 9: 257-281.

Watson, R., C. Ravenga, and Y. Kura. 2006. Fishing gear associated with global marine catches – I. Database development. *Fisheries Research* 79: 97-102.

Weinberg, K.L. 2003. Change in the performance of a Bering Sea survey trawl due to varied trawl speed. *Alaska Fishery Research Bulletin* 10: 42-49.

Weinberg, K.L., and S. Kotwicki. 2008. Factors influencing net width and sea floor contact of a survey bottom trawl. *Fisheries Research* 93: 265-279.

Weinberg, K.L., D.A. Somerton, and P.T. Munro. 2002. The effect of trawl speed on the footrope capture efficiency of a survey trawl. *Fisheries Research* 58: 265-279.

White, D.F., and C. Wilbert. 2009. Inhabiting technonatural time/spaces. In *Technonatures: Environments, Technologies, Spaces, and Places in the Twenty-First Century*, ed. D.F. White and C. Wilbert, pp. 1-30. Waterloo, Ontario: Wilfrid Laurier University Press.

White D., K. Cook and C. Hamilton. 2004. *The Net Kit: A Net Identification Guide to Northern Australia*. Sydney: WWF Australia.

Whittaker, R.H. 1962. Classification of natural communities. *Botanical Review* 28: 1-239.

Wiener, N. 1948. *Cybernetics: or Control and Communication in the Animal and the Machine*. Cambridge, Massachusetts: MIT Press.

Wiig, H. 2005. A cost comparison of various methods of retrieving derelict fishing gear. Honolulu: State of Hawaii, Department of Business, Economic Development & Tourism. Available online at http://hawaii.gov/dbedt/info/energy/resource/waste/marinedebris-pacon05.pdf (last accessed 9/18 2010).

Winger, P.D., H. DeLouche, and G. Legge. 2006. Designing and testing new fishing gears: The value of a flume tank. *Marine Technology Society Journal* 40: 44-49.

Wittgenstein, L. 1998. *Philosophical Investigations*. New York: Wiley-Blackwell.

Ye, S., and A.L. Andrady. 1991. Fouling of floating plastic debris under Biscayne Bay exposure conditions. *Marine Pollution Bulletin* 22: 608-613.