Exploring the 'As Yet Unknown' in Historical Epistemology, Experimental Systems and
Contemporary Nutrition

by

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Abstract

This work concerns the discovery of vitamins and the emergence of the vitamin concept. Specifically, I am concerned with the ontological and epistemological conditions for the emergence of the vitamins as new scientific objects within experimental systems. The theme of the 'as yet unknown' runs throughout the history of the vitamins and, I suggest, highlights the role of temporality in processes of knowledge production and the emergence of new objects. I point to aspects of the ontology and the epistemology of both a knower and a known immersed in time. I argue that the ontology of the unknown is characteristically virtual, blurred, transformative and productive of new knowledge and new objects. Thus, I have taken the opportunity of the apparent disruption in the smooth flow of time, of the 'as yet unknown' to consider the ontology of the encounter of the realization of scientific objects and I have pointed to instances in which the virtual, transformative and productive character of the encounter are highlighted. I outline the technical apparatus by which the scientists encountered the 'as yet unknown', both unwittingly and on purpose. I outline the modes by which the object was known and in so doing outline the object known through its increasing realization. I detail the problem as it developed and the transformations in the field of bio-chemistry necessitated and enabled by the emergence of this problem. In the final chapter I consider the bio-politics of nutrition necessitated and enabled by the emergence of the vitamins.

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And everything would wash away were it not for my love, D.

Table of Contents

Abstract	i
Acknowledgements	ii
Introduction	1
Chapter One	30
Accessory Food Factors: Understanding the Catalytic Function	
Chapter Two	57
The 'As Yet Unknown' and the Configuration of the Problem	
Chapter Three	89
A History of Biochemistry and the Becoming Rat of Early Vitamin Research	
Chapter Four	116
Encountering the Unknown in Animal Feeding Experiments	
Chapter Five	156
Disputes without Discovery	
Chapter Six	181
Grasping Life's Potential: The Bio-Politics of Nutrition	
Conclusion	212

I hear tell there's been flying saucers out here on the lake, and I'm hoping maybe one will pick me up. These aliens the government is trying to hush up, they got no stomachs, nourish themselves from photosynthesis – so you can see why the scum on top of us want to hack that up. The whole fucking planet is built on eating, and if a surgical intervention could remove all stomachs, the whole shithouse would come crashing down and then we could look at Bush and that tight-assed bitch Thatcher and Mohamad Mahathir, that Malaysian bastard hangs people for smoking pot ... would be flopping around like displaced catfish, only I wouldn't feel a thing for them, just stand there and watch them die. It would be my pleasure.

- William S. Burroughs, *My Education: A Book of Dreams*

Introduction

This work concerns the discovery of vitamins and the emergence of the vitamin concept. Specifically, I am concerned with the ontological and epistemological conditions for the emergence of the vitamins as new scientific objects within experimental systems. There are two of points from the history of the discovery of vitamins which make these conditions particularly compelling points of investigation. In 1906, Cornelius A. Pikelharing gave a speech to the Dutch Medical Society, which concerned artificial protein powders, popular at the time, that consumers could take to supplement their regular diets. Pikelharing reviewed the accumulated knowledge concerning protein metabolism and reviewed the relative quality of various products. He concluded that while one or two of the commercial protein powders were beneficial, such benefits could not be derived from protein and suggested that therefore, the benefits of the product lay in some 'as yet unknown' factor. Later, in 1911, Thomas B. Osborne at the Connecticut Agricultural Experiment Station and Lafayette B. Mendel of Yale University, while performing feeding experiments to determine the nutritive quality of isolated amino acids, reported, without reference to Pikelharing's earlier work, that there was some 'as yet unknown' dietary factor in milk which was essential to maintenance and growth in mammals. Without investigating it further, Osborne and Mendel integrated this 'as yet unknown' factor into the basic diet of their lab rats in order to bring their protein feeding experiments to successful conclusion. Several years after these vexed musings on 'as yet unknown' food factors, other researchers in the field of biochemistry took up the problem, and began feeding experiments with rats, stating explicitly that their investigation was for Osborne and Mendel's 'as yet unknown' factor and by 1913, this 'as yet unknown' factor had a name and a concept – that is, vitamin.

These explicit statements of the ‘as yet unknown’ are striking and important because they characterize encounters between the scientist and scientific objects as encounters between a knower and a known that are each immersed in time – *as yet* unknown. Thus, for this work, I have inquired into the significance of the ‘as yet unknown’ for sociological accounts of scientific practice and objects. However, I have not asked after the significance of the ‘as yet unknown’ in order to consider or theorize time and temporality. Rather, mine is an investigation of the ontology and the epistemology of encounters between scientists and phenomena, immersed in time as they are. I argue that encounters between the ‘immersed in time’ scientists and phenomena are characteristically virtual, transformative and productive. I point to aspects of these encounters between the knower and the known, such as the press of the world outside experience, the virtual, problems and affect, and suggest that these aspects of the encounter allow and enable its transformative and productive character. Further, I show how these aspects, the press of the as yet unknown world, the virtual, problems and affect are created by and within the materiality of experimental systems and the state of the scientific field at the time. I will define and elaborate the concepts of press of the world, virtual, problem and affect, throughout the course of this Introduction.

Because this work deals specifically with the ontological and epistemological conditions for the production of new knowledge and new scientific objects in experimental systems, it is distinct in significant ways from much work that has been undertaken in science studies to date. For example, this is not a sociological account of the standardization of the vitamins, their integration or ‘translation’ into larger social, political and cultural frameworks (Law 1999: 2; 3). Nor is it a work concerned with the “complex social process whereby variations in the form and legitimacy of scientific knowledge claims are gradually eliminated” (Woolgar 1988: 22). My work does not concern the regulation by national and

international scientific and governmental agencies of the vitamins. Nor do I consider the ways in which any one nation's population is governed in and through its vitamin consumption. I will not suggest that the government of vitamins curtails or enhances our various freedoms. Such a move would be valid; it is simply not the work I have undertaken here. The difference I have described might be characterized as the difference between the study of the context of justification and the context of discovery, with my work falling within the context of discovery category. Allow me to distinguish my work within that category.

Following Georges Canguilhem, in this work I am concerned with the specificity of science. Therefore, despite "the widespread rejection of demarcationism in current social studies of science" (Lynch and Cole 2005: 279; Woolgar 1988), this is not an analysis of the ways in which science is like other cultural practices, for example, in the moral economy of trust and the manifestation of that economy in power relations within vitamin researchers' lab practices (Shapin 1994: 355-407). I do not consider "science as a cultural phenomenon" (Woolgar 1988: 12). I do not want to deny that science, in many ways, is like many other cultural and social practices and that the ways in which science is a cultural practice are essential to sustaining it in its specificity. Science, for example, must also rely on language and narrative and must also develop social and economic relations. Science studies scholars have sought to de-mystify science by showing the ways in which science is in fact constituted in ordinary practices. Thus, these accounts of science focus on the reproduction of the sensible, the current order of knowledge within scientific practice. However, it has also been argued that the function of science is to stop "thought from just turning in self-satisfied circles" (Stengers in Greco 2005: 9). That is, science cannot only reproduce current orders of knowledge and remain science as such. Science therefore is both culturally and socially

bound as well as specific in its practices. Hence, my concern is with the phenomenology of those aspects of science which are not ordinary, the phenomenology of the specificity of science and its practices.

For Canguilhem, the specificity of science can be found in the development of scientific concepts:

The history of science can of course accommodate various kinds of objects within the specific theoretical domain that it constitutes: there are always documents to be classified, instruments and techniques to be described, methods and questions to be interpreted, and concepts to be analyzed and criticized. Only the last of these tasks confers the dignity of history of science upon the others (Canguilhem 2000: 30).

Because Canguilhem's historical epistemology is singularly concerned with moments of conceptual innovation in science, he is not concerned with the contribution of what was already present to the emergence of scientific concepts and objects. I maintain that if scientific knowledge and objects are emergent in time, their production cannot be reduced to their already actual determinants and, therefore, I explore the epistemological and ontological conditions of new knowledge production beyond the already actual determinant conditions. In an attempt to elucidate the work that I have undertaken, I will work through some of the abstract considerations arising from the philosophies upon which I have relied for the dissertation. I will also continue to review the work of science studies to date¹, particularly social constructivist accounts of the emergence of scientific objects, to consider how that work both contributes to my work here as well as to consider how my work differs from it. Finally, I will conclude with a review of the upcoming chapters.

¹ I will return at relevant points throughout the dissertation to expand upon points of difference between my work and various approaches to science studies. I have attempted, in making these references, to avoid caricaturing the positions I refer to (Livingston 2006:7). My attempt has been rather to discuss theories and ideas which have been part of the theoretical landscape in the social sciences and humanities for some time and to which various readers should have reference.

Understanding Canguilhem as a Philosopher of Problems

Canguilhem's place within the field of contemporary science studies can be described as liminal, at best. Many admire his work, and see his contribution to the history of science and to philosophy more generally as fundamentally important. And yet, it is also apparently difficult to know what to do with Canguilhem, how to use him. Scholars think there is something profound to his work, that it must be taken seriously, but no one speaks its name. His work sits at a difficult intersection of philosophy, history and the life sciences – disciplines which frequently claim to miss the relevance of the others for themselves. Of particular difficulty for contemporary science studies scholars is Canguilhem's claim that the life sciences are instances of life examining itself. The significance of this claim is lost to contemporary science studies scholars partly through attempts to avoid a naturalized epistemology but also through an attempt to avoid a vitalism, not least to avoid a biological reductionism. However, I suggest that the epistemological and ontological significance of this claim is what makes Canguilhem's work productive.

I characterize Canguilhem's work as having three strands: his concern with historical epistemology, his concern with normativity, the principle whereby organisms are healthy insofar as they can establish new norms and continually meet changing environments with success, and his concern to understand medicine and the life sciences as 'vital needs', that is, as attempts by life to understand life (Canguilhem 2000: 339). At first glance, the three strands have nothing to do with one another or require the long reach of Canguilhem's vitalism to make the connections. Although these connections do appear to be a long reach, it is not by making leaps that they constitute a consistent epistemology, but by embedding them, one within the other. The full productivity and relevance of each of these strands can

be found only when the claims of each of the threads are carried through to the claims within the other two.

To begin, I suggest that the relevance of normativity, characterized above, to historical epistemology must be drawn through understanding the life sciences as life's attempts to understand life. That is, the relevance, to science studies, of the observation that the life sciences are vital needs can be established by moving the concept of normativity to the heart of historical epistemology. If the life sciences are vital needs and life is lived in normativity, then the life sciences have some basis in normativity, in normative processes. The matter of normativity, therefore, is of central importance to historical epistemology. "[N]ormativity [is] essential to human consciousness" (Canguilhem 2000: 339). Historical epistemology can take seriously the notion that the life sciences are vital needs, by asking after the processes of normativity in knowledge production.

Now, Canguilhem has been called "a philosopher of the problem" by Gilles Deleuze, who cited for this claim, The Normal and the Pathological. Significantly, in The Normal and the Pathological, "problems" are not a problem. Normativity is, however. I propose that when normativity is moved to the heart of thought and epistemology, as it is when Canguilhem makes the life sciences a vital need, 'problems' become essential to the working through of historical epistemology. Healthy life is normative, according to Canguilhem, because it can survive and flourish in changing and problematic environments. "There is no biological indifference, and consequently we can speak of biological normativity" (Canguilhem 2000: 342). Deleuze goes further and argues that it is precisely this encounter with a changing and problematic, indeed unknown, world that forces us to think (Deleuze 1994: 139). The inception of thought is this moment which cannot itself be characterized as thought because it is an encounter with the as yet unknown and so can only be properly

characterized as sensed. In raising itself to that which it can only sense, life forces itself to pose a problem.² Canguilhem helps us to bear in mind that this encounter takes place between a knower and a phenomenon which are each immersed in time, because he sees normativity as an aspect of our immersion in the world and therefore as an aspect of the history of the world. “Problems do not exist only in our heads but occur here and there in the production of an actual historical world” (Deleuze 1994: 190). Hence, as I said above, problems are one aspect of the transformative and productive encounter between scientists and phenomena, immersed in time. Thus my work offers insight into different epistemic dynamics than those of the dissemination of standardized knowledge.

The Centrality of Problems to the Emergence of New Scientific Objects

The importance of the problem to historical epistemology or science studies more generally is given in that “the objects on which sciences work –the phenomena they are concerned with – are not immediately given to the senses” (Rheinberger 2005: 319). By extension, there is a break between “sensual knowledge” of the immediate world and scientific knowledge. It is in this epistemological rupture that “something that had been taken as given [transforms] into a problem” (Rheinberger 2005: 320). Thus the object being encountered in the world now lies outside experience and can only be “represented in a problematic form” (Deleuze 1994: 169). The problem is that which, at the limit of knowledge, provokes the phenomenon. “The phenomenon is provoked as a problem at the knowledge horizon and may itself require new concepts in order to be accommodated” (Rheinberger 2005: 320). Problems, therefore, are this site at which phenomena are provoked. Because science functions as such through problematization, a scientific object is

² The problem is posed, or formulated as the way in which the organism will proceed through the environment. In this way, Deleuze can suggest that it is the role of philosophers to pose problems, that is, to suggest problems, the ends to which life might be lived.

a phenomenon that has been drawn into the cyclical problematization of science. Hence, material and epistemological problems become central to the history of scientific objects.

Because they are the sites at which phenomena are provoked, problems have unique characteristics. Deleuze suggests that problems exist on the order of “a fundamental encounter” that can be grasped not by recognition, but “in a range of affective tones . . . [they] can only be sensed” (Deleuze 1994: 139). The genesis of thought lies in this moment of extra-propositional encounter (Deleuze 1994: 157). This is an encounter without quality than cannot be grasped by “empirical sensibility” (Deleuze 1994: 144). Therefore, to explore the provocation by the problem within thought requires an analysis of the dynamics of the “unthinkable or the inability to think at the empirical level” as part of the inventiveness of thought and the ingenuous power of thought.³

However, given that the moment of encounter with a problem occurs outside that which can be thought, it follows that, just as the scientific object is not given immediately, the scientific knower is “not immediately given either” and develops through processes of mediation as well (Rheinberger 2005: 320). The scientific object emerges with cyclical problematization and the scientific knower emerges through “a cycle of instruction” (Rheinberger 2005: 320). The emergence of scientific objects from problems therefore occurs in a double, productive, movement of interiorizing and exteriorizing the relation between the knower and the known or, better, the learner and the increasingly realized.

Following Hans-Jorg Rheinberger in his reading of Bachelard, I suggest that this double movement occurs within scientific instrumentation and experimentation (Rheinberger 2005). Scientific instruments, the technical apparatus of scientific experiments,

³ This description of the dynamic of a problem is reminiscent of an organism’s encounter with its environment, as it is evoked by Canguilhem’s idea of an organism’s vital normativity. That is, the dynamic emerges “from the imperatives of adventure” (Deleuze 1994: 197).

are the material instantiations and manifestations of a body of scientific knowledge. Experimental systems are made with instruments that are the material accretion of scientific knowledge. In analyzing the place of technical apparatuses in science, science studies scholars argue that the accretion of knowledge in these material systems means that through scientific experiments, scientists effectively commute their social relations into the material constitution of the world. However, I suggest that while the relative age of the technical apparatus of an experimental system is significant, this significance does not derive from the fact that the accretion of knowledge in experimental systems exhausts the process of new knowledge production. Indeed, it is the differential reproduction of the experimental system, which marks its productivity.

Experimental systems, as material instantiations of current scientific knowledge are the 'knowledge horizon' at which problems and phenomena are provoked. Science's cyclical re-problematization of phenomena requires the differential reproduction of experimental systems over time, through which problems are progressively determined and phenomena increasingly actualized (Deleuze 1994: 199, 211). Within experimental systems are provoked encounters with that which does not exist, yet. At the point of the problem, "phenomenon and instrument, object and scientific spirit, concept and method are all joined in a process of mutual instruction" (Rheinberger 2005: 320). There is established, through scientific experimentation, a "bond of profound complicity between nature and mind" (Deleuze 1994: 165). Therefore, in exploring problems it is necessary also to explore their conditions. Thus, experimental systems must be analysed to explore the conditions of problems and to analyse, thereby, how the problem evaluates "what is important and what is not ... in the conditions of the problem" (Deleuze 1994: 189). Thus, problems and their conditions are at the heart of my account of the emergence of vitamins as new scientific objects.

Science Studies and the Unknown

I suggest that scientists develop experimental systems that actively create changing and problematic environments that will force them to pose problems and think. That is, in their experimental systems scientists create, for themselves, encounters with the as yet unknown. While other cultural and social practices, say science fiction or comic books for example, might also cultivate encounters with the as yet unknown, I suggest that the material practices of experimental sciences are distinct from the materiality of other cultural and social practices because of the ways experimental systems engage and implicate the material world. Hence, much of the focus of this work is on the ways in which specific experimental practices engage the material world and generate an encounter between scientists and the as yet unknown. My focus in the chapters concerning experimental systems is not on the processes by which the technical components of experimental systems are developed or the ways in which the technical apparatus with which experiments were performed became increasingly stable. While I do address some of the processes of the standardization of the tools of experimental practice, the site of my investigation is not “things ... that are relatively stable and stay in place” (Law 1999: 4). I am interested rather in instability generated in spite of, but reliant upon, the stability of the technical conditions of the experiment. I consider how this moment of instability in knowledge and in the material world is orchestrated within experimental systems.

While a focus on moments of orchestrated instability within science might appear to deviate from the work of much contemporary science studies, as I see it, the question is begged by these very science studies. If we are to take seriously all that science studies has taught us, we cannot consider that scientific objects exist already formed, simply waiting to be recognized and named. “Sociological studies of knowledge and science have always

laboured under the burden of a widespread assumption that science represents pure and ‘objective’ knowledge ... the uncovering of the ‘true’ nature of the physical world” (Woolgar 1988: 7; 55). Science studies have effectively critiqued notions of “fixed, self-identical essence or substance of being” (Keller 2002: 56). Scientific objects are not there in the world, nor do we have the intellectual apparatus to “recognize” such objects, even if they were there. Hence, we find revealed in much sociology of science, “the constant unhappiness of epistemologists” (Latour 2004: 211). De-bunking the notion that “discrete objects exist independent of our perception of them” has been a hallmark of contemporary science studies (Woolgar 1988: 30). “The point, rather, is to open up the knowledge-game and to disconnect it from any kind of generalist, ‘view-from-nowhere’ model of authority” (Stengers 2002: 250).

Science studies scholars have argued further that the social context of science and scientific practices are not only “passive tools for revealing the character of these [discrete objects] for what they actually are” (Woolgar 1988: 57). Rather, science studies have asserted that scientific objects emerge through a “fluid nexus of mutually constitutive events, always already perspectival” (Keller 2002: 56). Therefore, I suggest, a major contribution of contemporary science studies to the history and philosophy of science has been to move epistemology away from a model of recognition wherein the work undertaken by scientists is to clear away ignorance and to reveal the scientific object in its true form – a philosophy wherein truth is characterized as proper recognition. “[A] critical appraisal of the idea of science must challenge the very idea of representation” (Woolgar 1988: 36). Science studies works to dispute the form of authority and account of objectivity given in this recognition model of science. Such work has been effective in revealing the power relations at play in developments of scientific knowledge. “[T]he sociological task was to discern what ... social

processes are involved in the construction, assessment and evaluation of knowledge ... and thereby to open the black box of scientific knowledge construction” (Woolgar 1988: 41).

Further, science studies has revealed and detailed the labour by which scientific objects are stabilized, standardized and disseminated, all labour that goes into the recognition of scientific objects as such and all the labour that goes into erasing that labour in the final ‘objective’ product (Shapin 1994: 359). Through analysis of this labour we have seen that “[t]he whole of human invention, imagination, intentionality and freely engaged passion is mobilized in order to establish that there is one interpretation only, the ‘objective one,’ owing nothing to invention, imagination and passion” (Stengers 2002: 251). So, for example, constructivism in science studies highlights the paradox by which scientists can claim the political need for funding of their labs and equipment, while claiming that the phenomena accessed by such labs and equipment are part of an ‘objective’ world independent of such labs and equipment (Latour 2002: 75).

Science studies have made it possible to consider the experimental systems of sciences as the body of science becoming capable of being affected by the world in specific ways and learning how best to be affected by the world. The “local, material and artificial setting” of experimental systems therefore enables scientists to become articulate about the ways the body of science is affected by the world (Latour 2004: 210). From these studies we can conclude, “reality and artificiality are synonyms” (Latour 2004: 213).

The social constructivism of science studies has been significantly mis-apprehended by scientists and even sometimes by science studies scholars themselves. Briefly, therefore, I want to consider the error of an over-zealous social constructivism though I don’t suggest such an error characterizes science studies writ large. An over-zealous social constructivism asks that productive social relations be the whole of the matter of scientific knowledge and

objects. “[T]he social network constitutes the object ... the representation gives rise to the object” (Woolgar 1988: 65). Insofar as social networks constitute the emergent scientific object, science studies make the same mistakes being criticized. This is a compelling symmetry in error between science and science studies. What they argue over is in fact a “wrongheaded agreement” (Livingston 2006: 8). That is, if the entire account of the emergence of scientific objects is taken up in an account of the social relations that weave through their emergence, as in, “the organization of discourse *is* the object” (Woolgar 1988: 73), this is to re-write a world that is at the disposal of human thought. Yet again the world encountered by scientists is simply waiting to be recognized and named. “[S]urface signs (documents, appearances) give rise to [objects]” (Woolgar 1988: 99). I suggest that while we cannot conceive of knowledge as a growing list of recognized objects, neither can knowledge be a growing list of recognizable social relations. To argue that the accretion or reification of social relations exhausts the production of scientific objects is to say that we recognize these objects as a specific conglomeration of already existent social relations. It is to fall back into the same ways of knowing that science studies would like to criticize. This is still a recognition model of science, but it is no longer scientists making the mistake, it is science studies scholars.

There is a second error inherent in writing social relations as determining nature. Such an account fails to see that it is writing nature as the raw material of culture. And in writing nature as the raw material of culture, this approach asks for a nature that is, ultimately, free of the productive webs of cultural and social relations (Haraway 1989). Such an account is asking for the same world asked for by the scientists whose work and epistemology science studies set out to criticize. I will return to the double bind of writing

nature as the raw material of culture in Chapter Four when I review sociological accounts of the place of rats as technical objects in scientific experiments.

The Good Will Politics of Social Constructivism

Let me return to some of my concerns with social constructivist accounts of the emergence of scientific objects in which science and scientific objects are simply different cut ups of already existent social relations. Isabelle Stengers has suggested that arguing that science has no specific means entails “important problems of political power” in constructivist accounts of science (Stengers 2005: 154). The work wherein we tell the sciences the story of how social relations determine their epistemology and findings suggests that all that is needed in order to resist the problems of these epistemologies and findings is to recognize them. Such a politics is a politics of good will, almost Habermasian in its import. Politics is a matter of clearing up confusion. Such is still a politics afraid that “‘reality’ is ... compromised by the pervasiveness of narrative” (Lopez 2006: 10). Once we have all recognized what the problem is, this philosophy suggests, we can move on with goodwill. Stenger goes further and argues that in asking the sciences to see that their work is nothing other than the contemporary state of affairs we ask, with policy makers everywhere “that [scientists] deserve the money they get” (Stengers 2005: 155). We function like the best neo-liberal government when we ask that science “forget about the distinction between scientific matters of fact and states of affairs” (Stengers 2005: 155). In failing to see the difference between the power to create and the power to manipulate, we reduce science to its techno-scientific capacity⁴, just as Monsanto would have it, and despite ourselves,

⁴ Indeed a recent article by Michael Lynch and Simon Cole deals explicitly and specifically with “repeated calls for [STS] research that intervenes in public controversies about science and technology” (Lynch and Cole 2005: 269). They argue that if “some kind of boundary work is a necessary precondition for making policy decisions...STS scholars have a duty to help legal and regulatory actors improve their art” (Lynch and Cole 2005: 297). Simon Cole is an historian of fingerprinting and the piece reviews some of Cole’s experiences as an expert witness in court cases testifying against the validity of fingerprinting. The article consists of court

perpetuate “technoscientific hegemony” (Lynch and Cole 2005: 296). We take up the model of recognition ultimately at our peril; political creativity and resistance will be better served by understanding creation. The creation of any new social norms, for example, will not need the power of “converging recognition” (Stengers 2005: 165). We need rather the means to call forth, pragmatically, that which is not already ours and Stengers suggests that we might in fact be better off appreciating science for its well-established capability in this regard (Stengers 2005: 165-166). Thus, an ontology that can account for the transformative and productive aspects of encounters with the ‘as yet unknown’ is of political import.

The Dimensions of the As Yet Unknown

The contribution of the world beyond our social relations to the production of scientific objects must be understood as a more subtle contribution than that of clay that simply yields itself to the press of contemporary social relations. It is important to begin to conceive of the ways in which the world, beyond what is already known and beyond that which already exists in social relations, contributes to the production of new scientific objects. The dimensions of this zone of the world that is beyond what is already known and beyond already existent social relations, do not match the system or the structure of the actual, real and recognizable world. This approach, which takes seriously the world beyond our knowledge and recognition, is distinct from a determinist model of the emergence of scientific knowledge, where knowledge is born of a filiative relation with some structure that is external to the complex system of the experiment. “For structure is the place where

transcripts and the transcript of an interview between Lynch and Cole where they consider the possibility that “[SSK] would enable science to gain scientific knowledge of itself” (Lynch and Cole 2005: 285). The authors state that the piece is an attempt to consider for the STS community some of the dilemmas and contradictions involved in “an actual effort to engage in a public controversy about science and technology” (Lynch and Cole 2005: 275). Ultimately their consideration is a word of caution (295) as Lynch and Cole reach the conclusion that the court case under consideration, where Cole’s testimony was disallowed, “certainly is damaging” to STS and SSK (Lynch and Cole 2005: 292).

nothing ever happens, that explanatory theory heaven in which all eventual permutations are prefigured in a self-consistent set of invariant generative rules” (Massumi 2002: 27).

The story of the discovery of vitamins provides a moment to pause over this question of the space of the unknown because, as I mentioned briefly above, the ‘as yet unknown’ becomes incorporated into the technical conditions of Osborne and Mendel’s experimental systems. When the ‘as yet unknown’ is integrated into the technical conditions of their experimental systems, it stands out as distinct from the known entities that constitute the remainder of the technical apparatus. There is a list of standardized, stabilized knowledge and objects and then, in the middle of them, appears this hole, the ‘as yet unknown’ factor, where the apparatus is not made up of past knowledge and expertise and neither has its future arrived. The familiar “action-reaction circuits” are suspended and narrative sequences do not move forward along a linear time line. Rather, the linear time of known narratives is suspended over a temporal sink, a hole in time. It is an opening of the seething potentiality of the world and sits in stark contrast to the known elements of the system. There is no actual present to this thing, there is the past knowledge, in which the object is unknown and there is the future to come, as indicated by the ‘as yet’. These are the dimensions of the emergent.

I argue, following Bergson, Deleuze and Massumi, that we can conceive of these dimensions as a “continuous but highly differentiated field that is ‘out of phase’ with formed entities” (Massumi, 2002: 34). The logic, therefore, of that which cannot be assimilated to known narratives must also be of a different order than the logic of the known and that which can be represented.

The importance of the as yet unknown for historians of science is that we find in the as yet unknown the beginnings of thought. And here we can remember Canguilhem. If, as

Deleuze argues, it is life's encounter with changing and problematic environments that forces us to think, we will require an ontology of the as yet unknown to render a full account of the emergence of scientific objects. The moment of life's encounter with the as yet unknown is clearly an aspect of the thought process and so introduces an entire terrain that cannot be negotiated with the model of recognition. Because this is an encounter with that which is in excess of what we know and recognize, we find in the beginnings of thought an encounter without an image. In apprehending that the beginnings of thought are without representation, we present an account of the emergence of scientific objects that is distinct from discourses of representation and therefore from social constructionist thought (Deleuze 1994: 130). If the forcing house of thought is this encounter with the unknown, then the social relations of the knower do not exhaust the whole of the thought process. Inscription is no longer a sufficient account of knowledge production when it is the world without representation forcing us to think.

Ours needs to be an account of a strangeness that awakens thought, which claws thought from its stupor, that makes it jump (Deleuze 1994: 139). This is why, as we saw above, there is introduced an interval of time suspension. Thought is shocked, cannot simply proceed or, simply cannot proceed. There are two aspects to this encounter: the problematic environment and the knower who encounters the problematic environment. To separate them like this is a bit artificial as the moment of the problem is a relation of intensity, rather than only two entities meeting. Nevertheless, there are aspects of the relation that are readily understood by discussing it as though the two entities were distinct. First, then, I consider the knower before considering the unknown that provokes thought.

In considering the knower as an aspect of the transformative and productive encounter with the as yet unknown, I will elucidate, as promised above, the concept of

affect. Knowing is much more mucky when knowers do not have only to recognize things or impose their names upon things, when knowing is a process driven by our immersion in the world - a world which is not preformed, not simply current social relations waiting to be manifest yet again. Bergson has characterized this encounter with that which moves beyond our experience and perception as an affective state. Following the line of tradition from Bergson to Deleuze and Massumi, affect and affective state are not taken here to “denote a personal feeling” (Massumi 1987: xvi). Rather, affect is the name given to the bodily experience of the ‘as yet’ in encountering the as yet unknown. “It is a prepersonal intensity corresponding to the passage from one experiential state of the body to another” (Massumi 1987: xvi). Affect is a concept which captures the way we experience movement between states from what was known to that which is not yet known. Affective states are unique experiences and are constituted by unique capacities, “an augmentation or diminution in that body’s capacity to act” (Massumi 1987: xvi). Affective states are characterized by our capacity to form new and different connections, materially and epistemologically. Affect is the experience of this state.

To move consideration of this affective state to the centre of accounts of the emergence of scientific objects is to take even more seriously the claim of science studies that our senses require education. Hence Latour can write, “[c]losing a door would appear to be a simple enough piece of know-how . . . but considering the amount of work, innovations, sign-posts, rectriminations that go on endlessly everywhere to keep them closed . . ., it seems to be rather poorly disseminated” (Latour 1988: 300). This education of our senses must be developed. That is, only in an encounter with a world too huge for our cognitive capacities do our senses need to be educated. If scientific objects emerge from the constellation of our social relations, the senses of the knowers are not being educated, but

rather they are being reminded of what they already knew. Therefore, because affective states are the ontological instantiation of pre-learned encounters with the world, an account of affective states is essential to an account of the emergence of scientific objects that is sufficiently robust to fulfill what science studies would like it to. In Chapter Four, I will consider the production of affective states within the rat feeding experiments undertaken by scientists investigating the vitamin problem and argue that the production of these states is essential to the scientists' encounter with and formulation of the problem of the vitamins.

In light of what I have just said about affect and affective states, it follows that the object of the encounter which initiates thought, "is not the given but that by which the given is given" (Deleuze 1994: 140). It makes a kind of intuitive sense to state that figures for which we already have an account, that is, the known world, that for which our senses are prepared, will not force our faculty of thought to the limits of its capacity. Rather, only an "element of intensity", an encounter without an image, will provide the demon that will drive us to think, will precipitate thought. If it is this unknown object which moves us to thought, Deleuze calls that which shocks thought a precursor, and because it derives from beyond the known. Deleuze refers to it as a "dark precursor" (Deleuze 1994: 145). Deleuze represents both the strangeness of the object and the violence of the encounter by saying that, "the dark precursor is not a friend" (Deleuze 1994: 145). Deleuze's dark precursor is reminiscent of an instance in the history of vitamins when E.V. McCollum was consigned to work in the basement of the Wisconsin Experimental Station because his lab animal of preference, the rat, was considered by his superiors to be "the ... worst enemy" of an agricultural institute. McCollum's story provides a moment in which to explore in detail encounters with the as yet unknown as an intensity that is transformative and productive at once and I will explore these aspects in Chapter Three.

Henri Bergson and Gilles Deleuze have both attempted to develop ontologies and epistemologies of the dark and shocking unknown, which they call the virtual. The virtual, Deleuze tells us, is not distinct from the 'real' because it is part of the real world, but is distinct from the 'actual'. Both the virtual and the actual are real and this means that the virtual exists simultaneously with all that is actual. That the virtual is real and exists simultaneously with the actual seems apparent given the discussion above, particularly the example of Thomas B. Osborne and Lafayette B. Mendel's experimental system that incorporated the 'as yet unknown' as a technical condition. Within the virtual can be found bundles of potential functions, the beginnings of tendencies. Incipient tendencies constitute the field of the virtual. The incipient forms within the virtual cannot be understood as structure. Rather, it is better to understand this zone of incipient forms as a 'soup of regions of potential' that develop only to disappear again while others arise.

It is this aspect of the material world that means that scientists in their research do not merely reproduce the social and technical world within which they work. The 'as yet unknown' does not pre-figure some already existent set of relations. Instead it signals precisely the collapse of the invariants of accumulated knowledge that enables the new to emerge. Hence, there is no need or capacity for an account of a causal origin (Rheinberger 1992). This new object is not merely a different cut of the same old fabric of the world; rather, a new world emerges. If there is no need for an account of causal origins of knowledge or new scientific objects, there is instead the possibility of an account of the processes by which they become actualized, or realized.⁵ Given that the unknown exists in a realm beyond what can be named or fully experienced presently, the process of actualization

⁵ Actualized is a Deleuzian term meant to suggest the movement of the phenomenon from the virtual to the actual. Realized is a Bachelardian term, which he used in order to connote both the material and the conceptual development of the object (Rheinberger 2005: 321).

is thus, a gradual cutting away of potential. “The truth is that this analysis is effected by a series of attempts at synthesis”, as Bergson puts it (1988: 102).

Having reviewed separately the knower and the phenomenon as distinct aspects of the encounter with the ‘as yet unknown’, we can return to understanding the encounter as a blur of the two, as the intensity created in the two-way process of interiorizing and exteriorizing, problematization and instruction of the encounter. We know from science studies that what is significant about the emergence of scientific objects is not the discrete objects themselves but that the ways in which subject and object come together “cannot be determined in advance” of the object’s emergence (Massumi, 2002: 231). The relation between the knower and the ‘as yet unknown’, then, is one of potential and the beings involved are not solidified in their identities, but are becoming. To call the entities involved in processes of actualization *becomings*, allows us to move fully away from an understanding of either the world as always already entirely existent or the world being made through various cuts of the same cloth. It is to take time, history, seriously and to see the world and the subject embedded together and working themselves out together, elaborating themselves through time, together.

This intensity of this encounter, the aspects of which I have just reviewed, is the site of investigation for most of the dissertation. Hence, I reviewed here the concepts I deploy throughout the coming chapter. These are all concepts meant to characterize the ‘in betweenness’ of movement through time of both the knower and the phenomenon. They are meant to allow an account of the shaping of the world in its emergence, to account for movement through time as transformative and productive and therefore to account for the states of the world immersed in time as contributors to, players in, the development of the world. I emphasize movement through time as an agent in the realization of the world so

that our account of the emergence of scientific objects is not only of the already formed world insisting upon its identity and recognition by scientists and neither is ours an account of the world constituted by the impression of scientists' social relations upon it. Throughout, I will characterize this in betweenness variously. In Chapter Two, for example, I concentrate on what I have called, 'the press of the world' beyond experience and beyond the narrative sequences of the already known. I suggest that this press of the world is experienced as affect and allows for the statement of a problem where previously there had been no problem. In Chapter Three I highlight the transformative capacity of encounters with the as yet unknown and characterize the intensity of this transformative capacity. Chapter Four reviews the material production of encounters with the as yet unknown by scientists within experimental systems. This chapter analyses experimental systems for the ways in which they produce virtual and affective states. Chapter Five returns to the affective and productive capacity of the problem in thought. Thus, each of these chapters relies on concepts which have been elaborated in this Introduction.

The Players: Biochemical

The dissertation also traces the vitamin concept as it was increasingly elaborated through time and in conjunction with the animal feeding experiments, most of which were performed between the years of 1906 and 1913 in American Agricultural Experiment Stations. While I am mostly concerned throughout with the material and epistemological conditions for the emergence of the vitamins, it is the elaboration of the vitamin concept that sets the limits of the investigation and the work concludes in 1913 with experiments that, in the historical literature, are commonly referred to as those in which vitamins were discovered. However, it is not because I consider that vitamins were discovered at this point that my work stops here. Rather, I stop with the 1913 experiments because at that point the

skeleton of the vitamin concept was in place. There are three key components to this conceptual framework.

First, vitamins must be attained for the organism's nutrition from the external environment. Second, vitamins are catalysts and so are needed only in trace quantities. Third, vitamins are organic; that is, they perform a regulatory function in metabolism. They stimulate processes of maintaining the organism's organization. At various points throughout the dissertation I will consider the emergent vitamin concept for one or some of these aspects. In the first chapter, for example, I consider the two-fold significance of the notion that a catalyst nourishes our entire organism and that this catalyst is attained from the external environment. In the second chapter I will consider the development of the idea that vitamins are needed only in trace quantities and in the fifth chapter I will consider the conceptualization of their organic nature.

The Players: Human

Work on an emergent vitamin theory was undertaken in both America and the U.K. throughout the early part of the twentieth century. The majority of historical accounts concern strictly or primarily the work performed by British researchers. Frederick Gowland Hopkins (1861-1947) at Cambridge University made an early contribution to the development of the vitamin concept when he proposed in 1906 that there existed 'accessory food factors' which he suggested performed a catalytic function within animal metabolism. After Hopkins' early contribution to the field, however, research into accessory food factors did not take place in the U.K. in a sustained way until the outbreak of WWI. Despite this, historical accounts of vitamin research on both sides of the Atlantic render a common account in which Cambridge bio-chemistry is the jewel of the crown, while American research work appears as the country cousin - a hard-worker with mud on her heels. My

concern is not to criticize particular accounts of the discovery of accessory food factors and the subsequent emergence of vitamins, or even to suggest that that history might be better written otherwise. Rather, I suggest that the American research stations are under-studied and provide a useful site to consider the methods by which propositions regarding vitamins were rendered.

Within American agricultural science in the four decades surrounding the turn of the century, scientists were concerned to identify the food values of various amino acids. At the University of Wisconsin's College of Agriculture, studies revealed that, even though experimental animals were fed 'scientifically balanced' rations, only animals fed on a corn diet would remain healthy. The experimental station therefore called in an organic chemist, Ernest V. McCollum (1879-1967), to undertake chemical analyses of the cattle's feed. McCollum had obtained his PhD one year earlier from Yale and had spent one year in a post-doctoral fellowship at the Connecticut Agricultural Experimental Station with Thomas Osborne (1859-1929) and Lafayette Mendel (1872-1935), a research team that was also actively pursuing studies of qualitative differences in purified amino acids (Rafter 1987: 528). McCollum's work at the Wisconsin Agricultural College was to consider the nutritional equivalencies of various cattle feeds. McCollum quickly came to a two-fold conclusion. First, not all foodstuffs could be considered of equivalent quality. And second, cows were too large and their metabolism too slow for chemical isolation and qualitative analysis of their feed to yield positive results.

At the same time, work at the Connecticut Agricultural Experimental Station was concerned less with animal feeding experiments and more with analysis of plant proteins. As in the experiments that prompted Hopkins to propose accessory food factors, Thomas Osborne established feeding experiments to determine the nutritive quality of various amino

acid diets. For this work Osborne called upon his colleague Lafayette Mendel, a physiological chemist at Yale (Ihde 1970: 29). As in the work performed by Hopkins, Osborne found that “continued studies over the next several years led to indifferent success in the creation of satisfactory basal diets” (Ihde 1970: 29). In 1913, the labs of McCollum and Davis in Wisconsin and, three weeks later, Osborne and Mendel in Connecticut each published results with conclusive findings that no diet of purified food substances is satisfactory and both proposed not only that there are accessory food factors, the qualitative value of which is necessary to animal life, but that these accessory food factors are to be found in butter fat (Ihde 1970: 29).

Methods

Despite having investigated the archives of both Lafayette B. Mendel at Yale and Elmer V. McCollum at Johns Hopkins University, the majority of the historical data for the dissertation derives from scientific publications and from historical accounts that rely on archival material that I did not access. The Journal of Biological Chemistry has archived past issues on-line and I relied heavily, though not exclusively, on this archive as a source for published material. After scientific publications, the dissertation relies most heavily on letters between the scientists. I did not collect these letters from archives myself, but found them transcribed in Stanley H. Becker’s 1966 dissertation, “The Emergence of a Trace Nutrient Concept through Animal Feeding Experiments”, which he wrote for his PhD in the History of Science at the University of Wisconsin. While Becker’s dissertation is a gold mine of transcribed archival material, his referencing leaves something to be desired and while he cites every letter with the sender, the recipient and the date, he only includes the archival source for these letters in a single citation! Thus, I can only speculate that all the letters he has transcribed are in the same archive he cites once, which is the archive of the Department

of Biochemistry at the University of Wisconsin, which is a source I did not access. Becker's dissertation is also the only source I know of for a translation of an important speech, by Cornelius Pekelharing, from Dutch into English, and I have relied upon this translation in Chapter Two. However, in Chapter Three I have relied on a tape recording of a speech given by Elmer V. McCollum, which I found in his archives at Johns Hopkins University. McCollum's archives are, or were in summer 2004, in rather a state of disarray and the tape has no date on it and was in a box that did not have any reference to the dates contained in the box.

For the most part, therefore, the material I collected in my own archival search has served as colour and flavour, rather than as the meat of the data.

The Chapters

In my first chapter I undertake a Canguilhem-style historical epistemology of the concept of 'accessory food factors' as F.G. Hopkins formulated it in 1906. This account is of the difference of the accessory food factor concept within physiology at the time. To undertake this analysis, I consider relevant aspects of the conceptual framework of the period, which was informed by the work of Claude Bernard. I review aspects of Bernard's physiology and philosophy of experimentation. To show how the Bernardian framework prevented researchers from conceiving of an accessory food factor as essential to nutrition, I review the work of a Dutch bio-chemist, Christian Eijkman. Eijkman is an interesting case because he shared the Nobel Prize for the 'discovery of vitamins' with Hopkins, yet he worked without a vitamin concept and never isolated a substance to which he attributed any of the qualities vitamins came to have. Having elaborated what prevented the concept of accessory food factor from developing within a Bernardian framework, I then review the conceptual work Hopkins performed with the results of his research. Hopkins did not

pursue his research into what he called 'accessory food factors' at the time and did not attempt to formulate his accessory food factors as a problem that should be taken up within physiology. Not yet, anyway.

In the second chapter I begin to consider the shape of the world beyond that which is known and to consider how its press can be usefully incorporated into accounts of knowledge production and the emergence of scientific objects. To this end, I explore three particular instances of scientists encountering the 'as yet unknown' and make two arguments. First, I argue for an ontology that allows for the world to present itself to us in new and unforeseen ways. Second, I argue that the formulation of a problem introduces a qualitative difference to the research field and such differences are important to note if we do not want our scientific objects to be simply accretions of the world, as it already exists.

If Hopkins has received a lot of recognition for his work on vitamins (including the Nobel Prize), E.V. McCollum is surely the second most frequently cited 'discoverer of vitamins'. In the third chapter, I undertake to re-tell the story, which is infamous in the secondary literature, of McCollum's desire to begin feeding experiments with rats, his superior's refusal to allow such work, and McCollum's subsequent research work in the basement of the Wisconsin Agricultural Experiment Station. I preface the story with an historical over-view of bio-chemistry and nutrition science to the turn of the century as well as a brief history of the establishment of the Experiment Station system in America. I use these histories in an attempt to set the scene for the horror McCollum's superiors felt toward his research subjects and to elaborate the significance of his work to the history of bio-chemistry. I recount the story as an instance of normativity in thought, just as I have outlined it above and point to the role of affect in interaction with historical, political and

social forces in the production of new intellectual norms. I also use the story to highlight the character of affect as the felt intensity of transformation.

Chapter Four shifts the focus of the dissertation from the epistemological conditions of the emergence of the vitamins to the material conditions for their emergence. In this chapter, I consider experimental systems as productive of new knowledge. To this end, I review the rise of the standardized lab rat and argue that it is significant that within the standardized apparatus of experimental systems, new knowledge emerges. I then consider the experimental systems used by researchers in pursuit of an encounter with the ‘as yet unknown’ accessory food factor; I suggest ways in which these systems enabled Osborne, Mendel and McCollum to encounter the unknown, and also how these systems enabled the scientists to begin to actualize the vitamins.

The fifth chapter returns to famous moments in the discovery of the vitamins, specifically two disputes between different scientists that took place in 1912 and 1913. The first dispute took place between F.G. Hopkins and Osborne and Mendel when Osborne and Mendel published results that stated explicitly that they were not interested in the ‘as yet unknown’ food factor as a problem. Hopkins disciplined Osborne and Mendel publicly for their irreverence and I suggest that Osborne and Mendel were effectively disciplined not for a mistake in their research or results, but specifically for their sense of the field and its problems. Osborne and Mendel responded to Hopkins’ disciplining by saying that though they had not to date published their results, the problem of the as yet unknown was in fact of central concern. I argue that this dispute points to the centrality of researchers’ ‘sense of the problem’ in constituting the limits of a disciplinary field.

I then review a much more famous ‘priority dispute’ between McCollum and Osborne and Mendel. Traditionally, within the historical literature it is argued that this was a

fight over the discovery of vitamins, that it is a priority dispute. In this chapter, however, I suggest that the public argument between the two labs is what precipitated the 'discovery' rather than the other way around. I argue that this is the case because the two articles over which the scientists argue do nothing different, in fact, than all the other papers that had come before. In each paper, evidence is presented to show that the as yet unknown factor is an organic catalyst. However, the chemical structure of the factor is still not known and neither lab had managed to isolate the substance. These papers therefore are re-formulations of the problem, similar to the re-formulation of the problems reviewed in Chapter Three. I conclude this chapter with a consideration of the significance to sociology of the problem of the accessory food factors as it is elaborated in the papers of Osborne and Mendel and McCollum.

In the last chapter, having become entirely caught up in the pre-conditions of the emergence of the vitamins, I endeavour to suggest the sociological significance of the vitamins as they were eventually conceptualized. The body of empirical work upon which I draw is schematic, but sufficiently suggestive for my purposes. The chapter is speculative, insofar as it is adventurous and takes a few risks, and suggests a terrain to be explored further. I suggest that in understanding the physiological function of the vitamins, we can begin to see how they affected social, economic and political relations upon their integration into international nutrition policy by the League of Nations in 1935-37. This chapter is a consideration of how the vitamins effect intense politicization of human nutrition and well-being, indeed vitality.

Chapter One

Accessory Food Factors: Understanding the Catalytic Function

Despite the practical knowledge throughout the 19th century that citrus fruit cured scurvy, and that rickets and beriberi were diseases caused by poor diet, it was not until 1901 that animal feeding experiments led one investigator to propose the existence of 'accessory food factors', a lack of which was determined to be the cause of some illnesses (Hopkins 1949). I argue that researchers studying scurvy, rickets and beri-beri were prevented from attributing the cause of these illnesses to a lack of some positive factor because such a conception was systematically unavailable to them within the conceptual framework in which they were working.

Research regarding these diseases was performed within a conceptual framework of physiology and the life sciences, which was informed largely by the theories and methods of Claude Bernard, a physiologist with an active career from 1843 until the time of his death in 1878 (Kahane 1966: 1). According to Bernard, order, organization and the capacities by which these are maintained and re-established, are the most salient features of living beings. Therefore, Bernard argued, the important work for the biological sciences is to investigate the processes of regulation by which this organization is maintained. According to Bernard, the organization of living beings is attained most perfectly when processes of self-regulation in the organism's bodily fluids function as an 'internal environment' of relative constancy for the tissues of the organism. Bernard argued that this 'internal environment' rendered organisms independent of the external environment and Bernard considered them examples of 'free or constant' life. To account for the emergence of accessory food factors and the difference that they make to the conceptual structure of the life sciences, physiology and biochemistry, I will sketch briefly those aspects of Bernard's understanding of 'free life' and the

internal environment that were fundamental to the emergence of accessory food factors and those that had to be overthrown for and with their emergence.

I will then review the work of Christian Eijkman, a Dutch bio-chemist working on the problem of beri-beri in the Dutch East Indies in the late 1800s. Eijkman shared the Nobel Prize for the 'discovery of vitamins', despite the fact that he worked without a vitamin concept. I will outline the conceptual framework within which Eijkman worked and highlight those aspects of the conceptual structure that rendered a vitamin concept systematically inaccessible to him. In this manner, I will show how it is possible that Eijkman did not 'discover vitamins' despite the apparent likeness of his experimental procedures and results with the experiments which did yield a vitamin concept.

I will then review the work of Frederick Gowland Hopkins, the bio-chemist who shared the Nobel Prize with Eijkman for the discovery of vitamins. I do not review his work simply as a counterpart to Eijkman's; rather, I am concerned to account for the work Hopkins performed which led to his conception of 'accessory food factors'. I argue that Hopkins was engaged in research to investigate the character and processes of a Bernardian internal environment when he proposed the existence of 'accessory food factors' and I account for the conceptual work that Hopkins was able to perform as a result of his research. Although Hopkins was investigating the character of the internal environment, I argue that his conception of accessory food factors rends so completely the Bernardian conceptual structure that the concept of internal environment is no longer an adequate account of the physiology of higher organisms. Specifically, the vitamin concept⁶ sundered

⁶ In fact, what emerges out of investigations within a Bernardian framework is the 'accessory food factor' concept. However, because I have yet to introduce the concept and will not do so until the section on F.G. Hopkins, to avoid confusion and for consistency, I shall refer here to the 'vitamin' concept. Also, although F.G. Hopkins proposed the existence of accessory food factors in 1906, vitamins were not widely known or even accepted for another decade. Therefore, it is arguable that while accessory food factors break with a Bernardian framework, the vitamin concept continued the break in their development

Bernard's conception of 'free life' as characterized by regulatory processes that function to isolate the organism's organization from the external environment.⁷ Although Hopkins does specify that the 'accessory food factors' he proposed were 'as yet unknown', my focus in this chapter is not on the role of the 'as yet unknown'. Rather, my focus in this chapter is to elaborate the set of conceptual problems Hopkins was working within and to establish in this way a Canguilhem-style historical epistemology.

Claude Bernard and Bernardian Physiology

The emergence of the vitamin concept meant that physiology required, for the first time, a conception of organisms that are dependent upon the external environment for the functions of regulatory processes maintaining their organization. I will review here aspects of Bernard's physiology that are important to understanding the difference made by the emergence of vitamins. It was not only a Bernardian conceptual structure that was important to the discovery of vitamins, but also Bernard's contribution to the practices of experimental physiology. Insofar as he demonstrated the principles of organic function through experimental practice, Bernard's work "vindicated" the use of the experimental method (Halberg 1967: 193).

The French physiologist Claude Bernard (1813-1878) was active in a physiology laboratory throughout the 1840s and 1850s and continued to teach and write until the time of his death. Bernard, Robert Koch (1843-1910) and Louis Pasteur (1822-1895) were contemporaries. Koch and Pasteur, through their different work as a medical researcher and as a chemist respectively, were each central to the discovery that parasitic organisms could act as agents of disease and each contributed greatly to further research in medical

⁷ While the vitamin concept is key to the dissolution of the Bernardian framework, it was not the only concept that emerged to undo the logic and relevance of Bernard's physiology. I am thinking in particular of Bayliss and Starling's 1905 naming of 'hormones', which I will review briefly later in this chapter.

bacteriology. Bernard maintained that the significance of their work to his own was to provide microscopic explanation for activities of life that, until the advent of microscope studies, had been attributed to 'vital force' (Bernard [1878] 1974: 3).

Bernard was involved in several major debates and points of contention within physiology at the time. Of significance here is his involvement in the vitalist/materialist debate, as well as his place in the debate regarding experimental practice in physiology and medicine. I will review his position in each briefly. The vitalist/materialist debate is significant to his understandings of organic function and problems of regulation. The debate between vitalism and materialism sought a definition of life's principles and emerged, according to Bernard, from the definition of the object of physiology, "[p]hysiology being the science of life's phenomena, it was thought that this definition implied another – that of life" (Bernard [1878] 1974: 11). Bernard's contributed to the debate by effectively skirting the issue. He argued that a definition of life was not necessary to the biological sciences. Indeed he argued that it was antithetical to the spirit of experimental science to deduce answers concerning its object from a definition (Bernard [1878] 1974: 11). That is, experimental science should investigate experimentally what constitutes life, rather than rely on a axiom or first principle.

The position that Bernard took in the vitalist/materialist debate points a way into the nuances of his understanding of organic function and problems of regulation.

One cannot make clear these basic notions by anything simpler; in a definition one has always to use the very word one wishes to define. . . . Nothing is definable in natural sciences; all attempts at definition produce only a simple hypothesis. . . . It is quite sufficient to have a common understanding of the word 'life', to be able to use the term, but at the same time, one must know, that it is illusory, vain, even contrary to the spirit of science to seek for it an absolute definition. What one should try however, is to pinpoint its characteristics in their natural order of significance (Bernard [1878] 1974: 12).

For Bernard processes of “death, disease and the capacity for recovery” distinguished life from the “mere existence” of, for example, a rock or a grain (Canguilhem [1977] 1988: 132). Living things could, characteristically, re-organize themselves; forms with the capacity to re-assert their organization, “re-establish their form” could be said to have life (Canguilhem [1977] 1988: 133). Because this capacity distinguished living things from non-living things, these processes of organization were the proper object of biological study. “Hence [for Bernard] in the animate body there is an arrangement, a sort of ordering, that should not be overlooked, for it is truly the most salient trait of living beings” (Canguilhem [1977] 1988: 133). Organization therefore is characteristic of vital phenomena and, according to Bernard, the proper objects for biological study are the various processes by which living things acquire this organization. “Vital force directs the phenomena, which it does not produce. Physical agents produce the phenomena, which they do not direct” (Bernard [1878] 1974: 22).

According to Bernard, the ordering that characterizes living beings is attained and maintained by “a regularity” (Canguilhem [1977] 1988: 139). This regularity is responsible for the development of an organism, its persistence and, if needed, its recovery. However, to re-iterate, “[t]he fact is, that vital force can do nothing without physico-chemical conditions, it stays absolutely inert; vital phenomena appear only when physico-chemical conditions decisive for its manifestations are present” (Bernard [1878] 1974: 21). Physico-chemical processes of regulation are the means by which living systems acquire and maintain any particular level of organization. Therefore, because all manifestations of life come from physico-chemical conditions, we do not need to consider the nature of the vital force. Rather, “[w]e shall seek to determine, as exactly as possible, the conditions of the manifestations of life in order to master them in the same ways as a physicist or a chemist

masters the phenomena of inorganic nature” (Bernard [1878] 1974: 20). Because “[t]his determinism reveals to us the conditions by which we can reach the phenomena, produce them, suppress them or else modify them” (Bernard [1878] 1974: 26), the field of physiology is rendered as “the physico-chemical determinism corresponding to vital manifestations” (Bernard [1878] 1974: 23).

Bernard’s fascination with processes of regulation was at home in the physiology of the time when the relative constancy of the fluid matrix of the blood was just being understood (Holmes 1967: 180). “Blood impressed him with its independence of its properties from the effect of external influences acting on it” (Holmes 1967: 180). For Bernard, the significance of the blood lay in its nutritive function, which he understood as one of the processes of regulation defining living things.⁸ Bernard understood nutrition as a process of “molecular renovation” and, thus, an essential trait of living beings (Bernard [1878] 1974: 16). Blood circulates and is, therefore, the nutritive environment within which the living tissues find themselves. Of particular significance is blood provision to the tissues of a steady state internal environment. As Bernard conceived of it, the internal environment engages with the external environment and is characterized by processes of regulation in order to achieve a constant, i.e. unvarying, nutritive environment for the tissues (Halberg 1967: 198). When organisms have attained this condition, they exhibit a form of life that Bernard refers to as ‘free’, ‘constant’ or ‘continuous’.

The existence of the being is lived in the liquid internal milieu formed by circulating organic fluids which envelope and bathe all anatomical elements of the tissues; it is the lymph or plasma, the liquid part of the blood, which in higher animals, permeates the tissues and pools all interstitial fluids

⁸ In his 1879 Textbook of Physiology, Michael Foster wrote, “In one or two instances only can we localize ... metabolic actions and speak of distinct metabolic tissues. In the majority of cases we can only trace out or infer chemical changes without being able to say more than that they do take place somewhere; and in consequence, perhaps somewhat loosely, speak of them as taking place in the blood” (Foster 1879: 382).

nourishing local regions, being thus the source and the confluent of all elementary exchanges (Bernard [1878] 1974: 48).

Blood particularly and body fluids more generally are the internal environment, which provides the “conditions of existence” for cells in higher animals (Holmes 1967: 179). This conception of the internal environment is Bernard’s most obvious and oft-cited contribution to physiology.

[T]he inner environment enveloping the organs, tissues and elements of tissues does not change. The atmospheric variations do not penetrate, so that it is true to say, that for a higher animal physical conditions of the milieu stay constant; the environment surrounding it is invariable, giving it a sort of atmosphere of its own in the forever changing cosmic milieu. . . . Perpetual changes of the cosmic environment do not affect it; it is not tied to them, but free and independent (Bernard [1878] 1974: 48)

These life forms are ‘free’ or ‘continuous’ insofar as they have the capacity to regulate themselves and require from the external environment only sustenance, a supply of energy. Bernard argued that the internal environment provides to the living molecule nutrition, sustenance, but he also conceived of the internal environment as a shield between the cells of the tissues and the vagaries of the external environment. Thus, there is a two-fold character to the internal environment. It is both a nutritive and sustaining agent and a protective system.

That sort of independence which the organism has in the external environment derives from the fact that in the living being the tissues are in fact withdrawn from direct external influences, and are protected by a veritable internal environment, which is constituted in particular by the fluids circulating in the body (qtd in Holmes 1967: 186).

Scientific investigation within a Bernardian framework interprets “body function” within a principle of constancy (Halberg 1967: 208).

Blood acted as a safeguard for the tissues and their basic constancy within the organism. The dynamism of processes of regulation is not fundamental to higher organisms within this framework. Rather, dynamism is of secondary importance, performing a supporting role to a fundamental constancy. The internal environment engages with the

external environment and is characterized by processes of regulation in order to achieve a basic constancy, which is the “physico-chemical conditions for the manifestation of life” (Halberg 1967: 198).

[W]e shall see that the principle of action in living matter is not internal. It could not be separated or isolated from the atmospheric or cosmic (External) conditions and there are no phenomena that could be attributed to it exclusively. The spontaneity of vital manifestations is only a false appearance, easily refutable by the study of facts. There are external agents, and outside stimuli which constantly provoke manifestations of properties in matter, in itself inert. In higher organisms these stimuli do, in fact, reside in what we call the inner milieu, but this milieu although deep situated is still external to the elementary organised part, which is the only part that is really alive (Bernard 1878 (1974): 12).

Hence, while Bernard was interested in the variability of the internal environment, this is only insofar as such variation resulted from processes which achieved this principle and fundamental constancy.

A shielding by basic constancy should not be confused with the view of basic rhythmic physiological interactions underlying a superficial constancy or, rather, a limited variability. In the former view, rhythms remain secondary or even trivial considerations as to both interpretation of body function and experimental method (Halberg 1967: 208).

The self-maintenance of this ordering is the mark of higher organisms. The internal environment is a nutritive function only insofar as it is also an isolating agent for the tissues. The order that characterized free life was a closed system.

This point moves us in two directions at once, back to Bernard’s position in the vitalist/materialist debate and forward to his arguments concerning the experimental method in physiology. Bernard argued that the effective isolation of the living molecule in higher organisms had contributed to vitalist conceptions of life and to the development of a vitalism in the biological sciences. “In constant life the organism appears free, independent of external cosmic occurrences[,] and vital manifestations then seem due solely to internal conditions” (Bernard [1878] 1974: 151). And it is because the explanation of vital

phenomena rests in the study of the delicate internal conditions of the organism that the experimental method is both difficult and necessary for physiology.

The spontaneity enjoyed by beings endowed with life has been one of the principal objections urged against the use of experimentation in the biological studies. Every living being indeed appears to us provided with a kind of inner force, which resides over manifestations of life more and more independent of general cosmic influence in proportion as the being rises higher in the scale of organization. In the higher animals and in man, for instance, this vital force seems to result in withdrawing the living being from general physico-chemical influences and thus making the experimental approach very difficult (Bernard 1949: 59).

Just as his thinking concerning the vitalist/materialist debate informed his understanding of organic function and problems of regulation, so too do both his thinking on the vitalist/materialist debate and his understanding of problems of regulation, point to the logic of his position regarding the experimental method in physiology and medicine.

Bernard's Experimental Method

According to Bernard, only experiment will elucidate the interactions of regulatory processes which characterize the object of the life sciences. "[T]he real explanation of vital phenomena rests on study and knowledge of the extremely tenuous and delicate particles which form the organic units of the body" (Bernard 1949: 63). Because regulatory processes characterize life forms, by extension the site for investigating this object, 'life', is at the level of these regulatory processes. The direction or the plan of the vital phenomena under study is given by the conditions which determine it. Thus, it is imperative to study these conditions. "[I]f vital phenomena differ from those of inorganic bodies in complexity and appearance, this difference obtains only by virtue of determined or determinable conditions proper to themselves" (Bernard 1949: 69)

There is an evident logic to Bernard's lifetime advocacy of the experimental method in physiology and medicine and his advocacy of the uses of vivisection to advance the biological sciences.

With the help of experimental analysis we must transfer physiological functions as much as possible outside the organism; segregation allows us to see and to grasp hidden conditions of the phenomena, so as to follow them later inside the organism and to interpret their vital role (Bernard [1878] 1974: 89).

Bernard argued that experiment must occur at the site of these processes of regulation, where the internal environment mediates the contact between the external environment and the tissues.

Experiment must take place at the site of regulation and these processes of regulation occur at the site of contact between the organism's internal environment and the external environment. "[I]f we wish to find the exact conditions of vital manifestations in man and the higher animals, we must really look, not at the outer cosmic environment, but rather at the inner organic environment" (Bernard [1878] 1974: 98). Further, if the processes of regulation are characteristic of life, they are best studied while the organism is alive.

Bernard contributed an understanding that within the internal environment substances have roles in processes of regulation and so rendered legitimate the investigation of chemical substances and their functions in living organisms. For Bernard, the study of "mechanism, properties and conditions" of life was the field of general physiology and rendered general physiology distinct from descriptive physiology and anatomy (Teich 1992: 491). Bernard's work thus contributed to the links between chemical and biological fields of investigation.

A final point of significance to us in Bernard's conception of the internal environment is that the integrative function for the processes of regulation maintaining the

constancy of the internal environment was performed by the nervous system (Halberg 1967: 199). That is, the nervous system is the mechanism by which processes of regulation to maintain an organism's order are stimulated, or catalysed. "[The] nervous system is the agent of equilibration of all conditions in the internal milieu" (Bernard [1878] 1974: 176). The catalytic functions of processes of regulation are performed solely by the nervous system.

Conditions necessary to the life of elements which must be assembled and kept constant in the internal environment for the functioning of free life, are those we know already; water, oxygen, heat, chemical substances otherwise called reserves. [I]n a perfected animal with independent life, there is a nervous system which regulates the conditions in order that they may work in harmony (Bernard [1878] 1974: 48).

Although Bernard understood that tissues within the body secrete body fluids, he did not attribute a catalytic or integrative function to these body fluids.

Christian Eijkman

Bernard's thought was profoundly influential for the physiological research and theory of his time and the discovery of and conceptualization of vitamins occurred within a Bernardian conceptual framework of organic functions, processes of regulation and stability. However, the emergence of vitamins required a re-conceptualization of organic functions and the functions of processes of regulation in living things. Within Bernardian physiology, processes of regulation in the blood rendered organisms independent from the external environment. Vitamins, however, serve a regulatory function within the organism but are attained from the external environment. Thus, to conceive of the vitamins requires thinking an organism that is not rendered independent from the external environment by its regulatory functions, but is, rather, dependent upon the external environment for these functions. Prior to this re-conceptualization, a good deal of research, even research into what we now understand to be vitamin deficiency diseases, was performed within a Bernardian framework and did not yield the discovery or conceptualization of 'vitamins'. Much of this

research looked exactly the same as the feeding experiments from which vitamins did finally emerge. In his historical account, "The Story of the Vitamins", Ernest McCollum, one of the Americans attributed with discovering vitamins, reviewed 16 feeding experiments performed between 1906 and 1920 in the UK and America, which did not lead to the emergence of vitamins but which were in all respects replicated in the experiments that did (McCollum 1936). One researcher whose work is representative of this type of research that did not isolate a vitamin substance or yield a vitamin concept nonetheless shared the Nobel Prize, in 1929, for his contribution to 'vitamin' research.

Histories of the discovery of vitamins almost invariably begin with or at least include in their accounts the work of Christian Eijkman (1858-1930), a Dutch micro-biologist working in Batavia, on the island of Java, in the Dutch East Indies.⁹ In 1897, Eijkman determined that beri-beri was produced when animals, including humans, are fed a diet of polished rice, but not when the animals are fed a diet of whole grain rice (Eijkman (1929) 1965; Carpenter 1995). Although the 1929 Prize assured Eijkman a place in the history of vitamins, it was only in retrospect that members of the scientific community and the Nobel committee could see Eijkman's work as pointing to the vitamins. With Canguilhem, I make a distinction between the history of a science and the historical epistemology of a science. History of science is the memory of a science, a repository of its knowledge. But, according to Canguilhem, the history of science asks that epistemology develop "a set of criteria for judging which moves within the vast expanse of the past are legitimate" within any one science (Canguilhem [1977] 1988: 4). A history of bio-chemistry can be "a record of everything ever said about the subject", from which it is easy to chart a line of progress from any one point to any other later in time (Canguilhem [1977] 1988: 4). It is the work of

⁹ Batavia is now Jakarta, the largest city and capital of Indonesia.

historical epistemology to ensure that this “space-time” of progress is not “imaginary” (Canguilhem [1977] 1988: 5).

Eijkman’s work certainly concerned food, nutrients and nutrition and the relations between food and illness. However, Eijkman was working without the word or the concept, ‘vitamin’. There is a body of work concerned with why it took so long for the vitamins to be discovered, to account for the social relations behind the ‘delayed discovery’. To account for the discovery of the vitamins as a delayed discovery misses the mark. The discovery of vitamins can only be conceived of as delayed from what is our current knowledge. According to Canguilhem, the job of the historical epistemologist is not to write a story of science of logical progression from one discovery to the next. Such an account can only be made retrospectively and is therefore more about what we know now than the science at the time we are investigating. Rather, science works at its own contemporary problems and it is the work of the historian of science to engage with the emergent problems in the science under investigation at the time under investigation.

If Eijkman’s work was not a harbinger of conceptual emergence, neither was it influential amongst the majority of researchers in the field at the time. Most historical accounts of the discovery of vitamins portray Eijkman’s work as little known and inaccessible because published only in Dutch until the 1920s. We will see in the next chapter that there is a connection between Eijkman and Casimir Funk, the scientist who coined the word ‘vitamines’. Funk did in fact read Eijkman in the original Dutch and relied upon Eijkman’s experimental findings to begin his investigations, but Funk used Eijkmans’ results as an example of work that pointed to a poorly formulated problem.

I argue that there are three conceptual differences between Eijkman’s account of the cause of beri-beri, which he understood as a deficiency disease, and the conceptual structure

that yielded a vitamin concept and the attendant understanding of deficiency disease. The prominence of a Bernardian conceptual framework accounts for two of these differences, and implicates the third. First, processes of regulation as they characterized life, according to Bernard, were processes that enabled independent, 'free' life; their regulatory systems were closed systems (Canguilhem [1977] 1988: 140). Therefore, within this framework, it cannot be conceived that the regulatory processes of organism occur in an open relationship with the environment. It cannot be conceived that an organism might be incapable of maintaining its order and perform necessary restorative functions because of a dependence upon the environment. This conception of an organism's regulatory processes as a closed system, in concert with the implicated and second conceptual difference, the power of the germ theory at the time to argue that bacteria, as positive agents, were the cause of disease, meant that Eijkman could not conceptualize deficiency disease in a manner which would lead to the emergence of a vitamin concept. Third and finally, as we saw, within a Bernardian framework, all stimulating power within the body is a function of the nervous system. We will see that the function vitamins serve in regulatory processes is a catalytic function. That is, they are a stimulating power in metabolism.

Working within a Bernardian framework, Eijkman therefore could not conceive that a chemical might act to catalyse the organization of the cells and tissues of living beings, less could he conceive that a catalytic function would operate at the site of contact between the organism and its environment. Eijkman did not see that an organism might need to be in contact with the environment for the catalysis of its organization or that the organism might lose its recuperative function because this dependency is not met.

Eijkman arrived in Batavia as an assistant to a Dutch commission sent there in 1886 (Eijkman [1929] 1965; Carpenter 1995: 155).¹⁰ Beri-beri was undermining Dutch colonial government efforts by seriously affecting colonial soldiers and ruining native prisons in the Dutch East Indies. The government commissioned Cornelius A. Pekelharing and C. Winkler to determine the cause and conditions of beri-beri and to attempt to deal with its alleviation (Carter 1977: 128).¹¹ “Faced with this serious situation, the Home Government decided in 1886 to send out a commission to investigate the nature of beriberi and its cause” (Eijkman [1929] 1965). At the time of Eijkman’s work, there were two prevalent theories regarding the cause of beri-beri; first, rice poisoning and second, “a deficiency of the rice diet, but not in the same sense as we now understand it”(Eijkman [1929] 1965). Interestingly, because of the power of the germ theory at the time, before the mission to Batavia the Dutch government sent Pekelharing, who led the commission, to study in the laboratory of Robert Koch. It was here that Pekelharing first met Christian Eijkman (Canguilhem [1977] 1988: 115).

After a year’s work in Batavia, Pekelharing and Winkler determined that beri-beri was parasitic in origin and they isolated a coccus from the air of army barracks, which they considered the causal agent of beri-beri. A year later, in 1887, they returned to the Netherlands (Carpenter 1995: 155). “The commission succeeded in isolating a coccus, which it considered [to be the cause of beriberi], from the air in barracks which, from its point of view, could be regarded as infected” (Eijkman [1929] 1965). Eijkman, however, did not

¹⁰ The claims for this date vary, between 1883 and 1886 (Carter, K. C. 1977). However, I will hold with the 1886 date because this is the date that Eijkman recounts in his Nobel Lecture.

¹¹ In his Nobel lecture Eijkman discusses the attempts to improve health and the relative successes that were made with the two prevalent theories, and a lack of a vitamin concept. Attempts to improve the health of populations were only relatively successful but this was due to the tastes within the populations, rather than some conceptual lack in the scientific theory. “In the light of our knowledge today, however, we see that there was a very large grain of truth in Van Leent’s [deficiency] theory, and it can scarcely be doubted that a lasting improvement in the health of the native crews could have been achieved if it were possible to introduce the Dutch navy food and persuade the men to take it” Eijkman, C. ((1929) 1965).

return to the Netherlands with Pekelharing and Winkler but stayed to pursue his research and was assigned the directorship of the laboratory of microbiology.

As the director at the laboratory, Eijkman made the auspicious choice of using chickens as the experimental animal for his beri-beri research. The instance that led to conclusive research was accidental, as Eijkman observed that the chickens had suddenly and spontaneously contracted a polyneuritis that looked much like human beri-beri. Eijkman's investigations found no parasitic agent and found that neither was the disease contagious through exposure. During the course of these investigations, Eijkman learned that the chickens' diets had been changed from low-grade uncooked, unpolished rice to the left over polished and cooked kitchen rice meant for human consumption. Eijkman established for himself that the development of the polyneuritis in the birds was linked causally to the consumption of the kitchen rice and pursued an investigation of the differences between the two kinds of rice. Eijkman found that it was not the cooking of the rice that made the difference but the level of processing each rice went through before reaching its different consumers. Chicken feed rice was not as heavily processed as was rice destined for human consumption and, after feeding experiments with different varieties of rice and different cooking processes, Eijkman concluded that the "difference between the two types of rice seemed to be in the presence or absence of the silverskin layer" (Carpenter 1995: 157).

Having arrived in Batavia at the behest of a colonial government overwhelmed by the problem of beri-beri, Eijkman had relatively easy access to human populations that would provide an opportunity to determine the relation between rice consumption and beri-beri. "Without assuming that polyneuritis gallinarum was etiologically identical to beri-beri, Eijkman sought to discover whether beri-beri was also correlated with the consumption of polished rice" (Carter 1977: 129). The majority of this research was carried out on prison

populations and the results were profoundly positive and enabled the control of symptoms on a massive scale.¹²

From the results of his most developed feeding experiments, Eijkman proposed “the [rice] envelope contains substances indispensable to life and health that are absent or occur in too low concentrations in the nucleus of the grain” (qtd in Carpenter 1995: 157). The development of feeding experiments brought Eijkman to his understanding of and surety of the correctness of a deficiency in the rice diets, but, “not in the same sense as we now understand it” (Eijkman [1929] 1965). For Eijkman, the cause of the illness remained in poisons and he speculated that a lack of the substance in the rice envelope will cause “inadequate formation of digestive fluids, accompanied with abnormal fermentation processes in the intestinal canal, formation of toxic reaction products, and so on” (qtd in Carpenter 1995: 158).

As beri-beri is a nerve disorder, a Bernardian understanding of the integrative function of the nervous system would resonate with the conception that some poison was hindering the integrative function of the organism’s regulatory processes. “We were most inclined to conceive that the amylaceous feed undergoes chemical changes in the alimentary canal under the influence of micro-organisms and that the resulting product or products act as neurotoxin” (qtd in Carpenter 1995: 160). In this way, Eijkman could conceive that beri-beri was prevented by the presence of some food substance, while not proposing that the missing substance was active in the integrative function of the regulatory processes of higher organisms. “In other words, one did not have to rule out poisoning as the immediate cause of polyneuritis, even though the problem originally stemmed from the lack of something in the diet” (Carpenter 1995: 158). Eijkman conceptualized a food deficiency disease, but one

¹² For a detailed review of Eijkman’s work with prison populations see “Paradigm Postponed: The Tardy Arrival of Vitamins” by Walter Gratzer in his book Terrors of the Table: The Curious History of Nutrition.

distinct from those which attended the vitamin concept. Specifically he considered the missing substance to function within regulation to protect the organism from the germs and toxins of the external environment, rather than conceiving of a substance which was necessary to functioning of the organism's regulatory processes.

Frederick Gowland Hopkins

By mapping the Bernardian framework within which Eijkman pursued his beri-beri research I have explored how and why the logic of Eijkman's inquiry ultimately did not lead to a vitamin concept. I have reviewed Eijkman's work and pointed to those aspects of a Bernardian physiology that would require dissolution for the emergence of a vitamin concept. I turn now to the work of Frederick Gowland Hopkins (1861-1947) to get to the root of "how and why the work was done", which led to the emergence of an accessory food factor concept (Canguilhem 1977 (1988): 117).

In 1906, at Cambridge University, Hopkins was performing feeding experiments from which he determined that there was some small but essential component of animal diets missing in experimental diets of purified starch, carbohydrates and fats. He proposed to call these small, essential components, "accessory food factors". Significantly, while Hopkins' work was informed by Bernardian physiology, he was also working at a time of transformation across several disciplines brought on in part by the discovery of hormones and their function in the internal environment. Hopkins therefore had available a notion that chemicals, and not only the nervous system, served integrative functions within animal organisms. His notion of accessory food factors was an important move in the development of these cross-disciplinary transformations. The concept 'accessory food factors' enabled Hopkins to re-conceive the character of the 'most salient feature of living beings'; that is, their organization. The feeding experiments performed by Hopkins allowed him to conceive

of chemicals, acquired from the external environment as essential to an organism's regulatory processes. Accessory food factors thus enabled a re-conceptualization of the nutritive function and necessitated a shift in understanding of nutrition and nutritional factors.

I will review Hopkins' earliest experimental work, not simply to review the early work of an important scientist, but rather to show the conceptual framework within which 'accessory food factors' emerged, which is distinct from the Bernardian framework in which Eijkman worked. A review of the distinct aspects of this framework will allow me to demonstrate the ways in which accessory food factors changed and contributed to the conceptual framework within which bio-chemical research was undertaken.

Hopkins' training and early research work (1888-1898) was undertaken during a time of transformation in the conceptual structure of physiology as a discipline.

[My] real qualifications . . . were those possessed by an individual who, having been trained for [analytic chemistry], and having acquired some knowledge of its aims and claims, sought later a training in the profession of medicine, and so gained similar knowledge with regard to it (Hopkins 1949: 123).

Hopkins' training in both analytic chemistry and in medicine meant that he had both the conceptual framework and the technical skill to consider these new problems of the dynamics of chemical processes and regulation within the internal environment (Kamminga 1996: 273). Hopkins went to medical school at Guy's Hospital in London and stayed there for two years after graduating (1894) to teach in the Chemical Department of Clinical Research. His early work was in urine analysis as a site for the investigation of metabolic and regulatory function. In this work, Hopkins could investigate "the mutual effects of the [physiological chemical] constituents one upon another, as they exist side by side in solution" (Kamminga 1996: 274). Hopkins' early work in urine analysis is characteristic of the field of

chemical physiology at the turn of the 20th century in its concern with the consideration and analysis of a newly conceived chemically regulated internal environment. It was during this time and due to this interest in the character of the processes of regulation within the internal environment that Hopkins began feeding experiments.

Hopkins left the laboratory at Guy's Hospital to take up a position in the Physiology Department at Cambridge University. Michael Foster, who was the chair of the Department of Physiology at Cambridge at the time, invited Hopkins to this position with the specific and explicit request that Hopkins bring with him for the Department the study of biochemistry.¹³ Foster, significantly, was the biographer of Claude Bernard and was determined to carry out research into the problems of a Bernardian physiology.¹⁴ In Hopkins' earliest work in the Cambridge physiology laboratory, he was assigned research into foundational chemistry of physiology. I showed above that the study of chemical processes was one aspect of Bernard's work that distinguished it from the physiology of his contemporaries and also that the investigation of chemical regulation of the internal environment was new at the time that Hopkins arrived at Cambridge. These two conceptual developments constituted what was, when Hopkins arrived at Cambridge, a bold step in disciplinarity as they established an object and field of investigation unique to biochemistry, one which could not be subsumed by physiology. These conceptual frameworks enabled

¹³ There are several versions of the story of Foster's invitation to Hopkins to come to Cambridge. In most, Foster simply invited Hopkins to the Cambridge Physiology department and Hopkins stayed there until the establishment of his own Department of Biochemistry in 1914 (Kohler 1975; Kohler 1982). In other accounts, however, Foster is involved in agitating for a school of agriculture at Cambridge and part of his program of agitation is to invite Hopkins to bring the study of bio-chemistry to this new school of agriculture (Geison 1978). Still another account has it that Hopkins was invited to the department of Physiology in 1898 and moved to a school of Agriculture within the University in 1912 (Kamminga 1996).

¹⁴ Foster's work concerned the physiology of the heartbeat. Claude Bernard conceived of the sympathetic nervous system as controlling all the involuntary muscle movement in the animal body. Foster, on the other hand, was committed to the notion that the heart contained muscles by which it established and regulated its rhythmic movements for itself. Not only did Foster himself dissent from the Bernardian concept, but for twenty-five years he was the leader of a group of dissenters, mostly based at Cambridge University (Geison, 2001). Of course, eventually these dissenters established that they were correct.

Hopkins to conceive of the importance of the dynamics of biochemistry to organic functions and their principles.

Researchers in physiological chemistry were asking questions and posing problems about the chemical processes of regulation in the internal environment. As Hopkins himself said:

[I]t is easy to recognise that, near the beginning of the present century [1900], a sense of satiety had arisen in connection with [chemical] ... studies carried out for their own sake. . . . In numerous centres, instead of only in a few, quite other aspects of the science were taken up: in particular, the study of the dynamic side of its phenomena. . . . Among the branches which are fated to benefit by this revolution – it is to be hoped in this country as well as others – is the chemistry of the animal. . . . In connection with enzyme catalysis the work done at this time by physiological chemists was in the main of a pioneer character, but it was urgently called for and had most useful applications (Hopkins 1913-14: 215).

This work would reveal the necessity of re-thinking the internal environment.

It is clear that chemical systems capable of so responding to what may be termed specific chemical stimuli must not be neglected by any student of chemical dynamics. The physiologist has for many years been engaged upon careful analyses of the mechanical and electric responses to stimulation. In the phenomena before us we find “responses” which are equally fundamental. If we do not study them exhaustively we shall miss an important opportunity for throwing light upon the nature of animal tissues as chemical systems (Hopkins 1913-14: 217)

Chemical regulation was a newly discovered function of the internal environment. The idea that chemical stimuli might function to regulate the internal environment is significant insofar as it opens new problems and understandings for functional regulation beyond the nervous system.

Although the discovery of internal secretion had led Bernard to the concept of the internal environment, it was not until the turn of the last century that internal secretions were conceived of as having a regulatory function. During this time of re-conceiving the internal environment the regulatory function of internal secretions was re-considered by

Brown-Sequard. "All tissues are modifiers of the blood by an internal secretion in the venous blood . . . We admit that all tissues and, more generally, each cell of the organism produces by itself some products or special ferments, which are poured into the blood and by the mediation of this fluid influence all the other cells which in this way are dependent on each other, by a mechanism different from the nervous system" (Houssay 1967). A little later, but still around the turn of the 20th century, Bayliss and Starling were publishing their results regarding the function of chemical messengers, a functional role performed by what they called hormones. "Bayliss and Starling (1902-1904) and Starling (1905) demonstrated that the duodenal mucosa in contact with acids, releases in the blood a substance, secretin, which stimulates the secretion of pancreatic juice. These specific chemical messengers which go from one organ to stimulate others at a distance, were called hormones, a name given by Starling (1905) suggested to him by Hardy"(Houssay 1967).

Even with the difference of chemical stimulation, or regulation, the concept of the internal environment at this point was still one in which the internal environment was constituted by regulatory processes that shield the tissues of the body from the vagaries of an external environment. The difference is that now both the nervous system and chemical substances perform the integrative function of the internal environment. Hopkins took up the problems of this newly conceived internal environment.

Hopkins' Feeding Experiments

Hopkins' initial feeding experiments were designed to bring to light the presence and organization of amino acids in proteins, such that they caused or inhibited growth.

In 1901, Hopkins and Sydney Cole showed that "tryptophane", until then a substance of obscure chemical identity and physiological function, is an amino acid (which was later renamed tryptophan). The physiological importance of tryptophan was demonstrated in . . . experiments, which showed that if mice are fed zein, a protein which does not contain tryptophan, they stop growing normally and soon die. . . . The implication

that a dietary supply of tryptophan is essential for animal growth and survival suggested that this amino acid . . . cannot be produced from precursors in the normal course of animal metabolism (Kamminga 1996: 274).

This research is significant as it went further in the re-conceptualization of the internal environment by showing that in its engagement with the external environment, the internal environment borrows from it not merely any matter for its sustenance, but a matter of sustenance of some quality. It is the first time the body was viewed as dependent upon the environment for an amount, a quantity of *quality* of sustenance, rather than simply a quantity of sustenance. This work, in concert with the emerging ideas of chemical, rather than only nervous integrative functions of regulation, pushed even further at the limits of the concept of 'free life' as protected from the specificities of the external environment by the blood.

From these experiments Hopkins was prompted to ask questions regarding the role of individual amino acids and their qualitative contribution to the course of animal metabolism. In a further set of experiments designed to determine the presence and organization of amino acids in proteins, animals were fed 'pure' diets, foods that were composed only of the isolated proteins, fats and carbohydrates of regular food. Such isolation was done to control the food at the molecular level so that amino acid sequences might be varied with success. The experimental method for these investigations led Hopkins to insights regarding amino acids and, more significantly for us, led him to insight regarding the nature of animal dietaries more broadly.

In the course of these investigations, Hopkins noted that diets of pure protein (even with an adequate amino acid composition), carbohydrate, fat, minerals and water fail to support animal growth, leading him to suggest the existence in normal diets of tiny quantities of as yet unidentified substances that are essential for animal growth and survival. He called these hypothetical substances 'accessory food factors' and hinted at a possible link between a lack of such nutrients and diseases such as scurvy and rickets (Kamminga 1996: 275).

Significantly, therefore, we see that the results of these feeding experiments convinced Hopkins, *without recourse to a germ theory*, of the existence of “accessory food factors”, without which the health and well-being of the animals was jeopardised and frequently imperilled. Eijkman had already conceived of a substance in food necessary for animal health and well-being, but one which relied upon a germ theory to understand the role of that accessory substance in foods. By developing a notion of an accessory food factor that was not part of a germ theory, Hopkins moved decisively away from a Bernardian conceptual framework. Accessory food factors distinguished themselves as something, the straightforward lack of which caused disease. Hence accessory food factors were distinctive also in a second respect. Accessory food factors were a foodstuff, or were acquired from foodstuffs, but their value in the body, their role in the internal environment, was not measured by the energy that they made available for animal activity, or the matter they contributed to the maintenance of the organism’s structure. The measure of the value of accessory food factors to the health of the animals under study could not be accorded a quantitative value. Rather, accessory food factors were important to animal metabolism because of the *role* they played in the *processes* of metabolism.

It is possible that what is absent from artificial diets and supplied in such addenda as milk and tissue extracts is of the nature of an organic complex (or of complexes) which the animal body cannot synthesise. But the amount which seems sufficient to secure growth is so small that a catalytic or stimulative function seems more likely. It is probable that our conception of stimulating substances . . . may have to be extended (Hopkins 1912: 452).

The accessory food factors conclusion is distinct from the tryptophan conclusion because tryptophan was a quality taken from the external environment for the sustenance of what Bernard called the living molecules, the tissues. Tryptophan was an amino acid of particular quality, but it was still a matter of sustenance for the organism. Tryptophan was a

kind of building material. The accessory food factors, by contrast, are factors of quality borrowed from the external environment as integrative, or stimulating, or catalytic functions within the organism's regulatory processes. That is, as opposed to the quality of sustenance provided by tryptophan, for example, the quality provided to the body by accessory food factors was not that of a substrate in metabolism, but of a function in metabolism. Molecules that perform catalytic functions are not consumed in the course of metabolism. In these feeding experiments, Hopkins has acquired evidence that the functions of chemical equilibrium during metabolism are attained through appeal to and in contact with the external environment.

Accessory food factors were both guests, as they are acquired through appeal to the external environment, and hosts to the chemical regulation of the internal environment. Accessory food factors are conceptualized as stimulating a system in process; they are internal regulators, rather than a foodstuff offering energy to be burned, or matter for bodily structure. A crucial aspect of nutrition therefore is a catalytic function. And it is here that we see the dissolution of the Bernardian concept of the internal environment. The nutritive function of the blood, the internal environment, is made up in part by the catalytic function of the vitamins. No longer does the blood simply bathe the tissues in a stable nutritive sustenance from which they draw their energy and material. Rather, if a substance is nutritive now, it is nutritive because it contains a catalytic agent, which is carried to the tissues. The implication is that a catalytic function is part of the tissue function. Regularity is not a process in which only the blood participates, but rather is constitutive of the organism's organization.

Here Hopkins is close to Bernard insofar as he attributes positive content to the notion of internal regulation. However, he is far from Bernard insofar as Bernard

understood regulation to be the means to achieve the ends of a stable internal environment, whereas Hopkins understood regulation to be constitutive of the organism's organization. Regulation and regulatory processes now occurred within organisms that are open systems. Higher organisms do not any longer have independent regularities, which maintained their organization as 'independent' life. No longer do processes of regulation shield a basic constancy from a variable external environment. The external environment can now account for the decay of the functions maintaining an organism's restorative powers. Hopkins' was a concept of self-regulation maintained through appeal to the external environment, as *constitutive of*, rather than protective of, self-preservation.

Conclusion

My attempt has been to analyse the "propositions which arise out of a certain type of work" (Canguilhem, 2000: 25). I have shown how the work in which Hopkins was engaged, his experimental work and his conceptual work, gave rise to the propositions he made concerning the existence of accessory food factors and their role in human nutrition. Prior to the emergence of accessory food factors, physiology was the study of a Bernardian conception of the internal environment, a process of regulation which occurred in the blood and other body fluids, which was integrated by the nervous system and which served as a nutritive supply to the body's tissues and in this function also served to protect the tissues from the vagaries of the external environment. The objects of study in this conceptual framework are regulation and normality, but these are a regulation and a normality to the ends of a fundamental constancy.

With the emergence of accessory food factors, we see that the nutritive function of the blood and the internal environment is not a function of protecting a fundamental constancy from variation in the environment, but rather one of delivering a catalytic function

to the tissues from the external environment. The living system is no longer closed, but now the study of regulation and normality must account for a system which is dependent upon the external environment for the provision of a catalytic function. This shift in understanding occurs on multiple planes. First, it must be conceived that chemicals can function as internal regulators; second, it must be conceived that these internal regulators can be acquired from the external environment, and finally it must be conceived that these regulators do not function as a barrier between the tissues and the external environment, but rather that these catalytic functions acquired from the environment are a function of nourishment to the living tissue. The accessory food factors point to the possibility of a life and a state of well-being beyond that which is attained through having 'enough food' to a conception of health and vitality attained in the suspension of cause and effect relations between the organism and its environment. Now life's organization, its 'most salient feature', does not develop in a cause and effect relation, but in catalysis at the site of contact between the internal and the external environment. Life is not constituted and sustained in cause and effect, but in excess of catalysis.

Chapter Two

The 'As Yet Unknown' and the Configuration of the Problem

In the previous chapter I argued that the accessory food factor concept, as developed by Hopkins, entailed significant changes to physiological theory and I elaborated the course of critical correction by which this shift was made. No longer was the organization of 'free life' characterized by its independence from the external environment but rather, the organization of even complex organisms, functioned via a dependence upon the external environment for a stimulant to its regulatory processes. The course of critical correction reads like a skipping stone, stopping on one point long enough only to launch to the next. In this chapter I dwell a while longer in those moments of change, in the problems driving the course of critical correction. Specifically, I consider these moments as encounters with parts of the world, which are in excess of what is already actual, known and recognizable. I argue that a key characteristic of this excess is that it is open to formulation as a problem.

Thomas B. Osborne at the Connecticut Agricultural Experiment Station and Lafayette B. Mendel of Yale University had been receiving Carnegie Institution funding for five years when their Carnegie Institution publication, "Feeding Experiments with Isolated Food Substances" appeared in 1911. In this work they were performing feeding experiments to determine the nutritive value of isolated proteins and found that unless they included some small portion of 'protein free milk' in the diet, their animals quickly failed and died, leaving the protein experiments inconclusive. They wrote, "as a maintenance diet our food lacked something other than protein and energy" (Osborne 1911: 60). Osborne and Mendel reported that there was some 'as yet unknown' dietary factor in milk, which was essential to maintenance and growth in mammals. After this, in order to bring their protein feeding experiments to conclusion, Osborne and Mendel simply integrated this unknown factor in

the form of protein free milk into the basic diet of their lab rats. Because they formulated the unknown as the '*as yet* unknown', this instance provides a moment to consider the temporality of both experimental systems and of the emergence of new scientific objects.

I argue, with Bruno Latour and following Michel Serres, that we must understand the emergence of new objects in the world not only as a mixing up of materialities, but also a mixing up of temporal frames. In the Introduction I suggested that theorizing complex temporal flows bears on the ontology and epistemology of accounts of the emergence of scientific objects. For example, Thomas Kuhn's conception of temporal flow led him to the paradoxical situation of arguing that objects always already exist fully formed and can therefore be encountered as anomalies and that scientists re-invent the world with every paradigm shift. Latour suggests that this conception of time as an always already present is characteristic of modernity. Within the temporal frameworks of modernity, Latour argues, contemporary conditions must all belong to the 'same time' (Latour 1993: 73). Latour suggests that this conception is insufficiently robust to account for the complexity of temporal flows. For example, I suggest that encounters between scientists and the '*as yet* unknown' are what Latour refers to as "polytemporal" (Latour 1993: 75). Latour argues that historical time and calendar time must be conceived of differently and that when calendar time informs historical investigations, our understanding of emergence is undermined. "Calendar time may well situate events with respect to a regulated series of dates, but historicity situates the same events with respect to their intensity" (Latour 1993: 68). Thus, Latour's notion of 'polytemporal' flows allows for the intense and transformative character of encounters with the '*as yet* unknown'. Understanding the history of science with calendar time is to conceive of science as a series of logical progressions, a "beautiful order. Latour argues that part of what characterizes modern science and modern approaches to science is

the sense that temporal flow is only a flight toward the future. Within this flight to the future there are no pockets of the past, no temporal lags. Instead there are exceptions, anomalies (Latour 1993: 73). “Fortunately, nothing obliges us to maintain modern temporality with its succession of radical revolutions” (Latour 1993: 74). Therefore, in this chapter I consider the character of scientific encounters with problems given a notion of complex temporal flows in which both the knower and the known are immersed.

First I will review the role of the ‘as yet unknown’, stated as such, in the Carnegie Institution work undertaken by Osborne and Mendel. Despite their explicit statement of the unknown, Osborne and Mendel were uninterested in pursuing the question of the unknown. In fact, they formulated the unknown as explicitly as they did precisely in order to avoid it as a problem. I argue that, despite the reticence of Osborne and Mendel, one of the significant aspects of the unknown is that it is open to formulation as a problem. Recall from the Introduction that the difference between an anomaly and the as yet unknown is that anomalies are always already actual and fully formed in an always existent present. The as yet unknown on the other hand is not always already fully actualized in the present, but is an encounter between scientists and the world, both of which are immersed and emergent in time. The as yet unknown is an encounter therefore that is virtual, transformative and productive. In fact, the formulation of a problem is a key aspect in moving from the unknown and its potential to the actual scientific object. I will review two instances in which scientists encounter the unknown and attempt to formulate it as a problem. In the first of these instances, Cornelius Pekelharing, who we encountered with Eijkman in Batavia, attempted to formulate the problem of the as yet unknown food factor by reviewing the effects claimed for commercial protein powder. Second, Casimir Funk, whose scientific biography I will elaborate, undertook a large-scale review of the literature concerned with

animal diet and disease in an attempt to re-formulate the problem as it had been presented to date.

It is significant to my argument to note that Pekelharing and Funk's encounters with the 'as yet unknown' occurred before Osborne and Mendel's. It is not, therefore, because Osborne and Mendel formulated the 'as yet unknown' that these encounters take place. Neither were Osborne and Mendel informed by the work of either Pekelharing or Funk. Rather, I suggest, these encounters occurred because the scientists were receptive to the press of the world, the press of the unknown. I argue for an ontology that allows the world to present itself in previously unforeseen ways to the scientists, even while it impels the scientists to think about the world. Of course scientists encounter the press of the unknown with consciousness. They move along the lines of the world already in place for themselves and others. My purpose is not to deny that our encounter with the world occurs with a postural schema, conscious knowledge, tacit knowledge and habitus, but to consider the world beyond our grasp as one aspect of the elaboration of new knowledge and new objects. I will consider the materiality, or better, the ontology of the unassimilable, that which is unknown and cannot be articulated, as it was found in these two early encounters with the unknown. I want to ask after the ontology and the epistemology, perhaps I would venture, the phenomenology of the unknown. I want to consider the scientists' encounter with what is there while the vitamins are not, yet.

I review the three encounters with the as yet unknown for two reasons. First, I consider the specificity of the scientists' encounter with the unknown. I will show that the as yet unknown leaves open different approaches, different possible formulations of a problem. Second, I will argue that the formulation of a problem introduces a qualitative difference into the field of research. That is, rather than add some 'fact' to the store of knowledge, the

formulation of a problem is significant for the qualitative difference it makes to the field of research. This difference points to the production of new knowledge and new scientific objects as more than an uncovering of facts to be stockpiled with the rest, but rather to a process by which differences become actualized.

Osborne and Mendel and the Carnegie Institution Publications

The story of Osborne and Mendel's encounter with and incorporation of the 'as yet unknown' lends itself to the apprehension of the unknown as a significant aspect of the emergence of new knowledge and new scientific objects. In 1911, Thomas Osborne and Lafayette Mendel in Connecticut were performing feeding experiments to determine the value of isolated amino acids and were compelled to integrate an "as yet unknown" food factor into the dietary regimes of their rats in order to bring their protein experiments to successful completion. The unknown, the "as yet unknown", is a factor of which, in their published reports, Osborne and Mendel discuss having taken account during their feeding experiments in order that they might get on with the work of amino acid analysis. In the published reports, the unknown is an aspect of the experiment, the research and the reporting of the results. In their report (as distinct from the publication) to the Carnegie Institution for 1911, Osborne and Mendel related that the efforts of bringing their protein feeding experiments to successful completion had so occupied their attention that other work undertaken with Carnegie funds was marginal and indeed that some promised work had not even begun.

The experimental feeding of white rats, which was instituted for the purpose of determining the relative nutritive value of different proteins, has given results of such interest and importance that our chief efforts have been directed to their successful conduct. ... Consequently much time has been devoted to the preparation of suitable materials with which to conduct these feeding experiments, for it has been essential to their success that only protein preparations of the highest purity should be used. The labor thus required has made it necessary to postpone, for the present, some of the

work which in our last report was described as nearing completion (Osborne 1911: 228).

That is to say, their protein feeding experiments were of such import that Osborne and Mendel had spent all of their time (and money, presumably) perfecting the 'as yet unknown' as a technical factor within their experimental systems, even when they had promised to undertake other research with moneys granted.

The problems Osborne and Mendel were facing, and the ones they did not face, are best understood as they were situated in the field of bio-chemistry at the time. Osborne and Mendel's was a particularly current problem, protein synthesis, and in the 1911 Carnegie Institution publication they presented their research question through a review of the 'newer knowledge' of proteins that had accumulated within biochemistry over the previous 10 years. This newer knowledge, as Osborne and Mendel understood it, had called into question the previously taken for granted assumption of physiological equivalencies between and amongst the proteins. Where previously it had been assumed that various proteins were made up of homologous units, no matter what the final protein, increasingly bio-chemical investigation was revealing that in fact the units of proteins, the amino acids, varied qualitatively and had various qualitative effects on animal physiology. "It has been the structural dis-similarity rather than the likeness of the proteins which has aroused physiological comment" (Osborne 1911: 2). By 1911, Osborne and Mendel argued, it could be taken for granted, for example, that the proteins of corn and wheat "show distinctive features in respect to the yield of the different amino acids obtainable from them" (Osborne 1911: 2), and the acceptance of these findings was growing in the physiological community. Osborne and Mendel wondered whether these differently structured proteins provided the same nutritional value. Further, they wondered, if different proteins were of various nutritional values, how was it that animals synthesized them such that animal protein composition was consistent.

Are these different proteins, specific in respect to origin, biologically differentiated and individually unique in chemical make-up, of equal value for the purposes of nutrition? If they are not, how are we to explain the uniformity, apparent or real, which is exhibited in the composition of the individuals of any species living under widely divergent dietetic conditions? (Osborne 1911: 2)

Evidence also suggested that “proteins experience a profound cleavage prior to absorption” and Osborne and Mendel postulated at the time that it was likely that protein material left the alimentary tract as amino acids (Osborne 1911: 3). These two pieces of evidence (amino acid specificity and amino acid absorption) together pointed to the fundamental importance of protein specificity to proper nutrition. Osborne and Mendel demonstrated that “the relative and absolute value of individual proteins must be determined by physiological trials” (Osborne 1911: 4).

Osborne and Mendel’s Carnegie Institution publication was issued in two parts. The first part of the publication in January 1911 was a review of work undertaken “chiefly for the purpose of orientation in respect to future proceedings” (Osborne 1911: 14). It was a publication of efforts to determine experimental conditions. Unfortunately, one of the experimental conditions that Osborne and Mendel reported was that in January 1911 their entire laboratory, rats and all, burned to the ground. It was after the fire that Osborne and Mendel decided to publish the first report on the experimental conditions established to date.¹⁵

¹⁵ Although never stated explicitly, it is fairly easy to conclude, given that their results came out in two parts, the first immediately following the fire, that the fire determined that Osborne and Mendel publish their first report when they did. Presumably the Carnegie Institution did not require the publication of results in two parts. As we will see the results in the second publication are of experiments that went on for much longer than those of the first publication. This suggests that Osborne and Mendel only published the first results when they did because they could not continue the experiments any longer.

The relatively short duration of these experiments¹⁶ became significant to the researchers and Osborne and Mendel reported on this significance in the second half of their Carnegie Institution publication. “[A]s the result of the first year’s experiments [those before the fire], it was found possible to maintain rats in health and apparent nutritive equilibrium over considerable periods of time on a mixture of isolated food-substances containing isolated proteins as the source of nitrogenous intake” (Osborne 1911: 59). In the second publication, without a lab fire to cut short the lives of their experimental subjects, it was found that “a prolongation of the observations has led to a less favorable outcome. A continuation of the experiments over longer periods has shown that in every case, sooner or later, the animal declined; and unless a change in the diet was now instituted within a comparatively short time the animals died” (Osborne 1911: 59).

In the second, longer experiment, Osborne and Mendel apprehend that the rats could not thrive or even survive on the experimental diet with which they had achieved positive results in the shorter experiment. In this second Carnegie Institution publication, Osborne and Mendel reviewed the data of the rats’ decline and referred to the intake records of the rats that died from subsequent malnutrition. With this second batch of rats, whose lives as experimental subjects were not cut short by fire, Mendel and Osborne found that, “[a]side from the proteins, in which our experimental interest has been primarily centered,

¹⁶ Prior to the fire Lafayette B. Mendel and Morris Fine, a PhD student in the Yale biochemistry laboratory undertook to investigate the utilization of various proteins and presented their findings in the Journal of Biochemistry under a series entitled, “Studies in Nutrition”. This series runs numbers one through six and consists of investigations into the utilization of various isolated proteins by rats and dogs and they are chiefly concerned with nitrogen utilization, rather than the protein synthesis that we are concerned with here. The publication of the six numbers of the series takes a year, from Studies in Nutrition I: The Utilization of the Proteins of Wheat which was accepted in September 1911 to Studies in Nutrition VI: The Utilization of the Nitrogen of Extractive-free Meat Power and the origin of fecal nitrogen which was accepted in September 1912. The papers were timely and contributed to the growing biochemical knowledge of the metabolism and catabolism of proteins and amino acids. However, none of the experiments were undertaken for a period of longer than 6 days. Further, six days was a single outlier and most experiments ran 4 to 5 days, with a significant number of only 3 days.

our attention has been drawn more and more to those components of the diet which are not sources of energy, yet fundamentally indispensable” (Osborne 1911: 57). They reported that rats previously declining on diets of isolated proteins could achieve a resumption of growth and well-being when fed a diet of whole foods (Osborne 1911: 60). “This observation is remarkable because of the exceptional duration of the experiment. It is apparent, therefore, that *as a maintenance diet our food lacked something other than protein and energy*” (Osborne 1911: 60). Such an observation is striking, as it exceeds the parameters set by the experimental system - to investigate the qualitative differences between amino acids. In this second, longer, set of experiments Osborne and Mendel were encountering problems other than the ones they had set themselves. The observation is of something which “pushed past the mark” of nutritive effectiveness and is unwilled by the scientists (Massumi, 2002: 211). The different duration of the experiment made apparent a problem that Osborne and Mendel were facing in the technical apparatus of their experimental system.

Despite noting in italics that there was some problem at work in their experimental systems, other than the nutritive quality of isolated proteins, Osborne and Mendel were positively blithe about its significance.

It remains to be shown precisely what the lacking component of our earlier diets is, whether some organic constituent or a peculiar proportion of inorganic ingredients. *In any event* it is evident that our original artificial food mixtures are incapable of supporting life indefinitely (Osborne 1911: 60; italics mine).

Osborne and Mendel here invoke ‘in any event’ and effectively signal to their readers that the question of the ‘lacking component’ is not the focus of their attention but, rather, provides insight into their real concern, their artificial food mixture. Because of these results, Osborne and Mendel spent much effort in their search for an artificial food mixture that would sustain rat growth, not to determine the ‘something other’ required in the diet, but

only insofar as such a diet would bear on the results concerning the nutritive value of the different proteins.

Osborne and Mendel did not attribute significance to the as yet unknown factor except insofar as it enabled them to complete experiments on other problems. The new at the time of its emergence was nothing more than one more preparation to get through in order to get on with the work of experimenting with proteins; it was an irritant and this aspect of irritation points rather well to the character of the new and the emergent (Rheinberger 1997: 417). The new is not what was possible from the experimental system as Osborne and Mendel had set it up. The new emerged from that which was beyond what they knew they had put into the experimental system.

This difference between the new and the possible is fundamental to conceiving the emergence of new scientific objects from a world immersed in polytemporal flows. This is not about accounting for the selection of one possible concept from those, the existence of which is already determined as possible. To rely upon the concept of the possible when considering the emergence of the new, or the unthought in the world is, according to Deleuze, following Bergson, to pose the problem badly. According to Deleuze-Bergson, the concept of the possible requires that which it is possible to think currently, that which is currently real. "The idea of the possible appears when, instead of grasping each existent in its novelty, the whole of existence is related to a pre-formed element, from which everything is supposed to emerge by simple 'realization'" (Deleuze 1988: 20). This is a spatialized conception of the new and life and is in error precisely insofar as it is spatialized. There are, in life, processes of change and unfolding; not everything in the world exists already, simultaneously. The possible is a self-identical image of the real and therefore that which is

real is indifferent to the context of its emergence and the insights of science studies to this point are rendered pointless.

Osborne and Mendel found that rats fed on diets that would not sustain life could be kept through periods of growth as well as pregnancy if only an addition of milk was made to the diet. “The favorable experiences with dried milk powder ... early directed our attention to [dried milk powder]” (Osborne 1911: 79). To prove the effectiveness of the nutritive property of milk powder, Osborne and Mendel kept rats on a diet of milk powder, starch, lard and salt for prolonged periods, with sustained growth and even reproductive capacity. Because milk is a whole food, though, such a diet could not contribute to the experimental feeding of isolated proteins. Previous experimental work, however, helped Osborne and Mendel integrate the significance of the milk powder for these isolated protein experiments.

Remembering that our earlier trials with casein, the chief protein ingredient of the milk powder, and with combinations of casein and other proteins were at best successful only in maintaining nutritive equilibrium – and that not indefinitely – and were never adequate for the manifestation of real growth, we directed our attention to the non-protein constituents of milk (Osborne 1911: 79).

Osborne and Mendel remembered that it was not the protein in milk that had been beneficial to their experimental animals. Therefore, they reasoned, the beneficial aspect of milk must lie outside its protein constituent. “The past remains, therefore, and even returns” (Latour 1993: 69).

At this meeting point between the as yet unknown and Osborne and Mendel’s memory of previous experiments, begins the formulation of a problem. That is, the meeting between the past and the future, presents the problem, which will force thought. Memory, according to Bergson can effect, or produce, various experiences of the ‘sensible qualities’ of the world (Bergson 1988: 34). “Memory augments and thus changes affects” (Hickey-Moody 2006: 194). It is affect, rather than understanding or knowledge, which is changed in this

instance because Osborne and Mendel did not know where they were going with the 'as yet unknown', they were still experiencing it as the potential for connection. They experience the potential for connection of the as yet unknown differently because of their memories of previous feeding experiments. Osborne and Mendel's sense of the matter changed with their memory of previous experiments. At this meeting point between the future and memory, Bergson tells us, there is not exact replication of previous actions because memory produces new experiences of the sensible qualities of the world. Poised between their memory and the as yet unknown, Osborne and Mendel have a new sense of the world. I might just as easily invoke Canguilhem here as Bergson. According to Canguilhem organisms can live in normativity because they are poised between the past and the future in their encounter with a self-insistent world (Massumi, 2002: 211). That is, at the meeting point of memory and the future, problems are constituted. And with the development of a problem comes a new sense of the world, the potential for various experiences of the sensible qualities of the world.

The process undergone by Osborne and Mendel as they reported it in their Carnegie Institution publication was not knowledge acquisition as we usually think of it. Osborne and Mendel do not so much think as they test their capacities in their new experience of the world. In response to their memories, they 'directed their attention'. In their report on 'remembering' and 'we directed our attention to', Osborne and Mendel are reporting on their sense of the experimental system, rather than their knowledge or understanding of it. Osborne and Mendel have no idea what this unknown substance is and yet they continue to work. Learning, at this point, is an embodied process of groping and feeling their way through the world that is beyond their experience and knowledge. The press of the world

beyond their experience is an equal player with Osborne and Mendel as they formulate their problem in light of the as yet unknown factor.

After numerous failures to modify the inorganic and non-protein ingredients of our dietaries by altering the relation of proportions of the various ions as well as the character of the carbohydrates and fats, it occurred to us that the protein-free portion of the milk might give the clue to the successful feeding of proteins which did not appear to be the inefficient factors in our cases of malnutrition (Osborne 1911: 80).

The scientists did not reason their way to the non-protein portion of milk, it occurred to them. We can take this turn of phrase 'occurred to them' in its two-fold meaning; that is, as both a sudden thought which arises and as something which happened to them, something which occurred. This occurrence derived from the insistence of the world beyond that which the scientists could know, experience or articulate. The object of encounter that initiated thought was sensed, but this was a sense of that which was beyond their perception, the as yet unknown.

Further, what occurred to them was not an answer to the question of *protein diets*, or even the *answer* to the question of the non-protein constituent of milk, but rather a *clue* to the successful feeding of isolated proteins. Clues are almost invariably riddles. Not only do they not reveal the secret, they present yet another puzzle of which sense must be made before proceeding. Success therefore (insofar as they are reporting it) is "getting" the clue, the formulation of the problem that lets them move forward. "There is no more a method for learning than there is a method for finding treasures" (Deleuze 1994: 165).

Osborne and Mendel proceeded by following the sense they had of milk and its nutritive constituents and prepared a protein free milk through centrifuge, distillation, filtration, evaporation, dissolving and further centrifuging.

The protein free milk powder thus produced as above described left about 14.5 per cent of inorganic matter on ignition. This includes ... the inorganic

constituents of the milk, although by no means in the combination in which they occur in the mammary secretion (Osborne 1911: 82).

The unknown could now be incorporated into the experimental system materially, as well as conceptually. “The use of this product (which we shall designate as protein-free milk) as an adjuvant to isolated proteins to furnish the inorganic elements of the diet has succeeded beyond our expectations” (Osborne 1911: 82). The incorporation of this unknown constituent, which was known to enable well-being, growth and reproductive capacity, allowed Osborne and Mendel to determine that when the unknown constituent was of no aid in enabling growth on a diet of isolated proteins, then the error in the diet lay strictly with the protein. “Thus at length we have found a method of controlling or furnishing some of the most essential non-protein factors in the diet, so that the value of the individual proteins can be investigated under much more favorable conditions than formerly” (Osborne 1911: 82).

By incorporating the ‘as yet unknown’ as a function of their experimental system, Osborne and Mendel incorporated, in effect, “a state of suspense, potentially of disruption” (Massumi, 2002: 26). In attempting to narrate the linear sequence of events, this ‘as yet unknown’ appears as a “temporal sink, a hole in time” (Massumi, 2002: 26). There is no actual present to this thing, there is the past knowledge, in which the object is unknown and there is the future to come, as indicated by the ‘as yet’. “What are we to do if we can move neither forward nor backward” (Latour 1993: 76)? The expression that the substance is “as yet unknown” resonates again with Canguilhem’s conception of health as life lived in normativity. Canguilhem tells us that with health, or normativity, comes the feeling of assurance that one is the establisher of vital norms. In the confidence of their ‘as yet’, the scientists assure us that they have been taken up in a process, they are surfing the wave of ‘coming to know’. Indeed, this is where they are most comfortable. When Osborne and

Mendel state that the matter is 'as yet unknown' they state their confidence that the problem will be more fully developed and elaborated later. In this way, the scientists signal the positivity of the process of transformation in which they are involved (Doyle, 2003: 131). In a later chapter we will see Osborne and Mendel stating explicitly their confidence that new knowledge will arise for them from that which is immediately entirely obscure. In the 'as yet unknown' we see that the vagueness of the new object is constitutive. The as yet unknown does not yet make specific connections within bio-chemistry, it contains only potential and this is both its vagueness and its productivity. It is only out of the vague, the indeterminate, the unknown, that surprise will emerge, that the new in the world will be constituted. The new is constituted as such precisely because it arises from this zone of indeterminacy, that which is beyond what can be articulated or named in experience.

In the history of the emergence of the vitamins, the processes at play in the zone of the as yet unknown are significant because the as yet unknown is all any of the scientists have for at least two years. It is striking just how long the unknown non-protein constituent of milk remains a function of the experimental investigations of the nutritive quality of various isolated proteins. For example 18 months after this initial formulation of protein-free milk, in their 1912 report, Osborne and Mendel do not even report as one of the "facts we now consider to be established" that some non-protein component of milk is essential to growth, maintenance and well-being of lab rats, let alone speculate on the nature of the non-protein constituent (Osborne 1912: 267).¹⁷ What emerges from scientific research concerning food and food values is an as yet unknown, as unknown, as a problem, and the presentation of this problem is considered to be a contribution to the literature. I argue that

¹⁷ In November 1912, Osborne and Mendel also published an article entitled "Maintenance experiments with isolated proteins" in the *Journal of Biochemistry*. In this article they invoke 'as yet unknown' factors as key to successful nutrition without suggestion that the non-protein constituent of milk might be one of those factors.

in their encounter with the as yet unknown, the scientists develop the problem through which the vitamin concept will be elaborated.

The Press of the World, Affect and the Striking Quality of Problems

The as yet unknown, whether stated explicitly as such or not, opens up many possibilities for research. All thought takes place “in a context of the unthought” (Wolfe 1998: 115) and the streaming of the virtual plethora of possibilities is a productive aspect of knowledge and knowledge production. In this section I consider two investigations and statements concerning the as yet unknown which emerge from examinations of precisely this unthought, the virtual aspect of bio-chemical knowledge at the time. These investigations are unique insofar as they are not primarily laboratory investigations but re-readings of the literature. These re-readings were undertaken first in 1905 by Cornelius Pekelharing (whom we saw supervising Eijkman’s visit to Batavia) and in 1911 by Casimir Funk. Each of them read a volume of literature and suggested, from the evidence presented there, conclusions different from the conclusions drawn within the literature reviewed.

Pekelharing and Funk initially only read other experiments and experimental results, yet they were each able to determine problems beyond the narrative within the texts. It is perhaps ironic to find in what is effectively a content analysis of literature an instance in which the agency of the material world asserts itself. The nature of the material world is not entirely contained by what has already been said by men of science. The material world presses upon these scientists, it has a “self-disclosing” nature. I argued above for the importance of that which is virtual in the process of coming to know. The virtual does not disappear with the acquisition of knowledge, the emergence of the scientific object. With the actualization of the scientific object, that which is virtual drops back and remains as a realm of potential, that past and the future of the objects and knowledge of the world.

Actualization changes the shape of the virtual, but the virtual persists. It is as though the surplus effect of previous explanations, for example, those reliant upon the germ theory, go in to making the excess that compels the further groping and re-readings by the scientists. All the sensory data that would have been experienced as affect by the scientists working with the germ theory, fell back in to the virtual and remained current for Pekelharing and Funk, there to be experienced again and differently by scientists engaged with the problem later.

Within these re-readings it is particularly easy to see the virtual as a realm of bubbling potential, because in being re-readings, these results effectively re-virtualize the observations made previously. The effective repetition of the experiments whose results are reviewed by Pekelharing and Funk effects a decomposition of the object and a re-composition in such a manner that appeals to the intelligence of the body (Bergson 1988: 111). For example, in memorizing a poem, a student will repeat the poem in such a way that it is no longer a whole, but enters her memory in pieces. In repeating the poem she decomposes it - memorize this part, this part, this part - and thereby also effects a re-composition. Although their task is not memorization, Pekelharing and Funk effect repetitions which do decompose and then recompose so that "at each new attempt it separates movements which were interpenetrating; each time it calls attention of the body to a new detail which had passed unperceived; it bids the body discriminate and classify" (Bergson 1988: 111). These re-readings are thus an exploration of the limits of the capacities for connection in physiological and nutritional science. Pekelharing and Funk followed attentively the paths blazed by researchers and effectively encountered the byways left behind, circuits of possibilities to which they were ready to respond (Lingis 1998 141).

I suggest that each of Funk and Pekelharing's newly formulated problems from previously published results also points to the power of affect in the emergence of scientific knowledge and scientific objects. Affect, again, is the bodily experience of an encounter with the as yet unknown. Because Pekelharing and Funk did not know what their field needed to become, their sense of the change was not of forward momentum and movement from point A to point B, but of the intensity of transformation without an object. Pekelharing and Funk each had a different sense of the experiments they read than the sense presented by the original authors. This work is significant insofar as neither instance adds new facts to the literature but rather introduces into the literature a different order of things. These attempts to re-read the literature are examples of the workings of mental activity in "striving to transcend the conditions of useful action and to come back to itself as pure creative energy" (Bergson 1988: 15). In the work I review here, Pekelharing and Funk add thoughts on the unknown. They do not add to the store of accumulated scientific knowledge, but insert a qualitative difference into the order of science at the time. They present a "striking quality" of the previous experiments (Bergson 1988: 158). With these different configurations of the problem, as presented by Pekelharing and Funk in their re-readings of the literature, there is a re-configuration of what can be actualized. A re-configuration of the problem brings a qualitative difference, a new order of possibility.

Cornelius Pekelharing

The first of these re-readings was not a publication originally but a speech given by Cornelius Pekelharing, to the General Conference of the Dutch Society for the Advancement of Medicine in Amsterdam, July 3 1905 (Becker 1968: 301).¹⁸ We recognize

¹⁸ Because this publication appeared only in Dutch, it had eluded my attention until I found it translated in whole by Stanley Becker for his dissertation, "The Emergence of a Trace Nutrient Concept through Animal Feeding Experiments" (Becker 1968).

Pekelharing as the supervisor to Eijkman on the trip to Batavia for which Eijkman is credited with the discovery of vitamins. Although Pekelharing's lecture was published, it was published in Dutch and, as a result, does not appear to have affected at all the work of the American researchers or, for that matter, Hopkins. Neither, we will see, did Funk cite this talk in his review of the literature six years later, even though Funk could read Dutch. Such reception is resonant with the character of the presentation itself. It is a presentation of the unknown as such, that which was beyond current knowledge and current narrative lines, of something that would have registered as an affective state. Pekelharing's speech comes within a context of thought, a paradigm if you please. But this context of thought cannot be considered to be only those terrains that had already been mapped. I suggest that Pekelharing encountered the unknown because he tapped into a part of the context of thought which had yet to be mapped. "A certain degree of 'stepping off the board' ... must be undertaken if the unthought is to be encountered in any radical sense" (Hickey-Moody 2006: 195).

In this speech, Pekelharing did not further the formulation of a problem that was *already* being developed in his scientific community and neither did he add facts to a contemporary store of knowledge. Rather, Pekelharing used his talk to argue that contemporary bio-chemistry of proteins pointed to an 'as yet unknown' food factor, necessary for health and growth. That is, he suggested the formulation of a new and different problem within his field.

Pekelharing's talk concerned artificial protein powders popular at the time that consumers could take to supplement their regular diets. The powders were amino acid products of processes mimicking 'light digestion' and it was thought that their benefit lay in providing the body with a readily accessible (because already lightly digested) source of

protein. Such readily digestible products were meant to build poor constitutions. In reviewing the relative qualities of various products, Pekelharing reviewed the accumulated knowledge to date regarding the break down and synthesis of proteins from amino acids in the animal body. From his review, Pekelharing concluded that while he was certain that some of the products were beneficial, any benefit derived from the powder products did not derive from their amino acid constituents.

I find it difficult to accept that the value of the much used food preparations is entirely found in the casein [a milk protein] content or in the presence of other lightly digested protein. ... But I cannot believe that the fame of these food preparations should in the first place be ascribed to suggestion (Becker 1968: 314).

To make his point, Pekelharing reviewed the benefits known to be derived from the specific protein used in each powder and argued that the benefit claimed for the powders could not be derived from the action of the specific proteins in each.¹⁹ Significantly, Pekelharing argued, benefit could be derived from the most effective powder from quantities too small to provide significant caloric or protein values. "If advantage is seen in the use of ten to fifteen grams of sanatogen daily the wholesome effect cannot be ascribed to the protein it contains" (Becker 1968: 316). Such small quantities did not contain enough protein to cause the improvement seen. Further, Pekelharing argued not only that the protein powders provided too little protein to make a difference but also said he had, by experiment, confirmed that it is impossible to keep an animal alive on a diet of purified protein, fat, and carbohydrate.

¹⁹ Pekelharing also claimed that a review of the literature pointed to the fact that animals, and not only plants, as had previously been thought, were capable of synthesizing proteins. Pekelharing therefore shows himself to be at more than one frontier in bio-chemical thought.

Pekelharing therefore argued that the protein supplied was insufficient to make a difference to the diet and even if it was sufficient, he had concluded that these principles do not exhaust what is required in animal dietaries.

It is impossible to keep an animal alive by feeding it protein, fats, carbohydrate, necessary salts and water, even if the quantities of each of these substances are quite sufficient and the animals exist under favorable conditions (Becker 1968: 318).

While on a diet of sufficiently isolated food substances, Pekelharing's animals lost weight, weakened and died within four weeks. However, if Pekelharing fed his animals with milk instead of water, "they remain[ed] healthy" although the milk was not present in sufficient quantities to provide beneficial amounts of protein, fat or carbohydrate (Becker 1968: 318).

Not only was the quantity of milk supplied insufficient to explain the continued health of his animals, but Pekelharing also found that, "the substance of the milk which keeps the animals alive is also found in whey from which casein and fat have been removed" (Becker 1968: 318-319). In his speech, Pekelharing told the audience that he had attempted to isolate the factor from milk and to know its properties more thoroughly. Although this work had "given no satisfactory results", Pekelharing was able to state from experimental evidence acquired in 1905 that, "there is present in milk an as yet unknown substance which is of immense importance for the diet, even in a very small quantity" (Becker 1968: 319). Pekelharing therefore read the contribution of the protein powder in terms of the effectiveness of the 'as yet unknown' factor in milk, and proposed that if the powders were making such a substantial difference to well-being on such a little contribution of protein, which, anyway, might not be all that is needed in the diet, perhaps the most effective of these powders was supplying this 'as yet unknown' factor.

The Power of Affect in Thought

The significance of his talk was not to be found in the new facts Pekelharing contributed to the study of nutrition, but in his re-reading of the facts. He rendered a qualitative difference to the subject at hand. His were questions of how; how to read the literature, how to interpret the data, how is benefit for animal nutrition attained through milk powders? It is interesting to see this effect for what it is insofar as such was not Pekelharing's intent. Pekelharing's point was that commercial protein powders were not what they were cracked up to be. In this pronouncement, Pekelharing's lecture was an event in contemporary thinking about human nutritional needs. His must have been a pronouncement of some surprise, at least to some of his listeners, and would certainly have been so to the distributors of the protein powders. With surprise, of course, comes a world of a different order. Things are not what they seemed.

What Pekelharing had done in his presentation to the Dutch Medical Association is effectively to re-open the field of the known to the unknown. Bergson argues that when objects are known, fully articulated and actualized, it is not the case that we see more clearly the object of investigation. Rather, in the process of actualizing the object, we have obscured and blocked some aspects of it, so that definitive limits can be marked around it.

To obtain this conversion from the virtual to the actual, it would be necessary, not to throw more light on the object, but, on the contrary, to obscure some of its aspects, to diminish it by the greater part of itself, so that the remainder, instead of being encased in its surrounds as a thing, should detach itself from them as a picture (Bergson 1988: 36).

This means that when the actual state is fully emergent, a whole set of potentials drops back out of the actual situation but goes on resonating as abstract potential (Massumi 1992).

Affective states are states enabled by our immersion in the world, they are our capacity to absorb some of the world's action upon us, which is beyond what we experience

directly. Bergson draws the analogy of a black screen which absorbs light to characterize the function of affective states. "Our 'zones of indetermination' play in some sort the part of the screen. They add nothing to what is there; they effect merely this: that the real action passes through, the virtual action remains" (Bergson 1988: 39). In his encounter with the protein powders, Pekelharing absorbs that which has been remaindered in knowledge concerning the effects of protein and because of this affective state can present the re-reading that he does. In this re-reading, Pekelharing does not simply reflect that which is already current knowledge. What we see in Pekelharing's presentation is not only his capacity to reflect the world around him, but a presentation of his body's ability (I want to say his organism's ability) to absorb some "part of the action" of grappling with the world. The re-readings of the literature performed by Pekelharing demonstrate our capacity to take in what is beyond current possible actions and recognizable possibilities.

In treating the protein powders for his speech, Pekelharing approaches the world in a novel and unique manner, revirtualizes aspects of it and encounters the as yet unknown. Specifically, he begins by noticing improvements in health derived from food, in excess of what can be accounted for by cause and effect. Pekelharing stresses that the 'as yet unknown' food factor is required in only very small quantities. "All of this points to the fact that . . . there must be present besides protein another substance which already in small quantities is capable of exciting the appetite and of improving the general eating habits" (Becker 1968: 317). What is significant to Pekelharing here is the unknown substance's capacity to excite, even in small quantities, Pekelharing suggests, therefore that the effect of these food factors is out of proportion to the amount required in diet and thus moves away from a cause and effect model of human nutrition to a consideration of more complex dynamics. In his treatment, health is attained by the insertion of a moment of

indetermination in the food. The vitality provided by food therefore, includes a different *kind* of reaction than a simple cause and effect.

We can see Pekelharing's question beginning, in some respects, in much the same manner in which the questions of Osborne and Mendel began. That is, his was a concern for the effects of protein powders, commercial products that would have emerged from the kinds of laboratory analysis and from a similar interest in the benefits of protein which drove the work of Osborne and Mendel. In some respects, then, his is a concern for the value of proteins, but it is not the specific action and value of particular proteins or amino acids that he is investigating. Rather, he subtracts the power of proteins to argue that there is some vital function, which is not accounted for by the cause and effect of proteins.

Formulating the Problem

We see the beginning of the formulation of the problem here, as Pekelharing moves away from the cause and effect model on which the efficacy of the protein powders had been presumed (build the constitution). Protein powders were presumed to work by the contribution made by the proteins, the addition of the proteins, the proteins as supplement. Pekelharing argues against the notion that a building up of proteins in the body is sufficient for the benefits derived from the protein powders. He insists that something is required, yet in such small quantities that its effect is out of proportion to its possible energetic or metabolic contribution. In stating that the value of the protein powders was beyond the possible contribution of caloric value by the proteins, Pekelharing began to suggest that simple cause and effect relations, the simple building up of protein and burning of carbohydrates, were not all there was to vitality and not all that was necessary to maintain vital functions. Because Pekelharing begins with the well-being provided by a supplementary food product, the vital function which he finds is an excess – well-being. Vitality and well-

being are constituted in more than is possible with building block relationship, even if they are qualitatively supplementary. Life is not lived in maintenance. It is this capacity to go beyond its current state of health and well-being, that characterizes life. Vitality and well-being are functions of excess and Pekelharing's sense is that a function of excess, something out of proportion to its quantitative contribution, is to be found within food. The factor other than protein found in the protein powders is a function of life's excess. It is also in excess of the known food constituents. It is an essential and accessory food factor. And this is the problem of the vitamins: a function of excess is essential in nutrition.

Funk

Casimir Funk (1884-1967) is an interesting fellow in the history of the vitamins. It is never denied in the secondary literature that Funk developed the neo-logism 'vitamine', but historians of nutritional science have been uncertain of which accolades to give him and his status in historical accounts matches his status within the scientific community of his time. He was and is a rather beleaguered and itinerant researcher.²⁰ It is apparent even just from the two articles I will review here that Funk belongs to a rather different scientific culture than either Hopkins, or the American researchers Osborne and Mendel or McCollum. Funk's standards, methods and that which he considered data were different from the standards, methods and data employed in any of the work I have reviewed to this point. Given this difference and Funk's beleaguered status, it would be possible at this point to consider the power relations between and amongst the various scientific cultures working on

²⁰ Because the majority of the research that Funk reviewed had been performed in languages other than English or German, Funk had access to work that the Americans did not. Also, the 'national style' of the experiment stations might have made it impossible to see Funk's re-reading of the literature as data (Geison, 2001: 4). There was a distinct lack of variety in these stations in experimental animals or experimental objects. Funk, on the other hand, utilized a variety of information from a variety of sources which had used a variety of experimental situations and animals. The American researchers simply would not have had the tacit knowledge to appeal to Funk's resources or to Funk as a resource, whereas clearly Funk did appeal to a wide variety of sources for his data.

the vitamin problem. However, I am more interested in a moment when Funk undertakes an examination of the 'unknown' insofar as his different approach to the unknown allows an examination of the potential of the as yet unknown, and of the potential at work in the formulation of problems through which concepts are elaborated. Funk's re-configuration of the problem is an event wherein "the future takes the form of a circle expanding in all directions, and the past is not surpassed but revisited, repeated, surrounded, protected, recombined, reinterpreted and reshuffled" (Latour 1993: 75). I argue that an examination of Funk's formulation of the problem allows us to see that the as yet unknown makes available many possibilities.

Like Pekelharing, Funk engaged with the 'as yet unknown' through a re-reading of a body of literature - the results of animal feeding experiments designed to investigate animal disease. In work that was on-going between 1911 and 1912, he reviewed a vast amount of literature concerning specific animal diseases. These were experiments like the ones that, I argued in the previous chapter, were systematically prevented from developing a vitamin concept because of the theoretical milieu within which the work was framed and performed. Funk re-read this literature and argued first, in the Journal of Physiology in 1911, that upon his reading it showed that there were curative substances to be found in some foods and second, in 1912 in the Journal of State Medicine, that the literature indicated some accessory and as yet unknown factor was missing from the animal diets. He argued that the unknown substance might begin to be known as a 'vitamine'.

Funk presented the findings within the literature as constituting a problem still unsolved. In doing so he offered a surprise ending to the tales as they had been told until that time and established a qualitative difference in the order of things. Indeed, he used the formulation of the problem, as well as the evidence provided by earlier publications, as

evidence to argue against their results and for his conclusion concerning 'necessary protective bodies' of food.

[Deficiency diseases] were considered for years either as intoxications by food or as infectious diseases, and twenty years of experimental work were necessary to show that diseases occur which are caused by a deficiency of some essential substances in the food. Although this view is not yet generally accepted, there is now sufficient evidence to convince everybody of its truth, if the trouble be taken to follow step by step the development of our knowledge on the subject (Funk 1912: 341).

Interestingly, Funk does not ask after the physiological function of the substance. He does not ask whether this accessory substance is an anti-toxin, as Eijkman had thought, or some kind of stimulating chemical, as Hopkins had suggested in 1906. Funk was not concerned primarily with the physiological function of the accessory food factor, but with chemical analysis and isolation of the substance. For Funk the problem is that the chemical nature of the necessary protective substance in foods is unknown. In pursuit of the problem, Funk attempted to isolate the curative and effective substance from various foods and the 1911 and 1912 articles are the record of his results.²¹

Summarizing our knowledge till [sic] 1911 of the chemical nature of the protective substance from the rice-polishings, we find the following well-established facts:

- (1) The substance is soluble in water, in alcohol, and in acidulated alcohol.
- (2) The substance is dialyzable.
- (3) It is destroyed by heating to 130 C
- (4) Is neither a salt nor a protein

We see, therefore, that our knowledge of the chemical nature of the substance was very small (Funk 1912: 345).

²¹ It is difficult to tell sometimes what the conclusions of the experiments were from Funk's translations of the findings he used in his review, as he obviously presents the findings to support his notion of a 'vitamine'. So, for example, Funk reviewed Eijkman's findings with which we are already familiar and stated that Eijkman was able to show that "the aqueous extract from rice-polishings (silver-skin) cures ... and that the protective substance dialyses and is not precipitated by alcohol" (Funk 1912: 343). Now of course, Eijkman did show this but we know already that the conclusions he drew were wrong. It is important to bear in mind therefore, that these are not Eijkman's conclusions from his own research, but the conclusions Funk is drawing from Eijkman's research. Such a process is significant because it signals the qualitative difference Funk was introducing to the field.

Again the results presented are not of physiological concern; there are no growth charts or photos of the animals, but rather numbers and formulas are given for yields, precipitates and drying times. The physiological role of the unknown factor is Funk's last concern. "[T]he physiological importance of the vitamine will be discussed in the last chapter" (Funk 1912: 350).

While Funk's work does involve feeding experiments, he only performed feeding experiments to verify that he was correct in his determinations of the chemical nature of the curative and accessory substance. "The presence or absence of the active principle in the different fractions, obtained during the investigation of rice-polishings, was determined by administering them to pigeons already severely affected with polyneuritis and observing the results" (Funk 1911: 395). It was not these biological results that concerned Funk, but the conclusions of his chemical investigations.

The substance which is absent in polished rice and is contained in rice-polishings is an organic base which is completely precipitated by phosphotungstic acid and by silver nitrate and baryta. ... Reasons for provisionally regarding the active substance as a body giving a crystalline nitrate which has the percentage composition of 55.63% C, 5.29% H and 7.68% N are adduced ... The curative dose of the active substance is small; a quantity of substance which contains 4 mgr of nitrogen cured pigeons (Funk 1911: 400).

Funk pursued the action of the unknown substance within the animal body through processes of chemical isolation. "It was therefore thought useful to compare the contents of nitrogen and phosphorus in the brains of pigeons fed on polished rice with that of normal pigeons" (Funk 1912: 348).

Funk presented his results in a relatively cryptic fashion, presuming that the chemical formulas he presented had apparent significance.²² "The analysis of this compound led to the

²² Later we will see that Osborne and Mendel frame the problem of the as yet unknown factors in terms of growth and present growth charts in a similarly cryptic fashion, without explanation.

formula $C_{17}H_{20}N_2O_7$ " (Funk 1912: 347). The presentation of a chemical formulation to a knowledgeable audience would effectively present them with an image of the chemical. With the presentation of the image, Funk does not present a solution, his is still a tendency; he does not draw the links for the chemists in the audience but points them in a direction. He does not say, 'this is the work performed by this chemical'; rather, he lends to the field of the as yet unknown directionality in presenting a chemical formula. The field is not yet determined, but with the presentation of the chemical formula its potential is given a direction. The tendency given to the field of potential by Funk's chemical formulations, persists and begins the process of actualizing the 'vitamines' – a name appropriate to a substance with a nitrogen base. With his chemical investigations, Funk begins the process of blocking off possibilities within the as yet unknown, begins to limit the possible formulations of the unknown and so begins the process of actualizing the substance.

Funk's configuration of the problem differs in two ways from those undertaken by Hopkins before him and the Americans after him. The differences form a kind of mobius loop, whereby the direction taken by one group of researchers leads them back to where the other started. Funk moved from an investigation of animal disease and reviewed literature that had so far been concerned with animal disease. This concern for animal disease led Funk to an interest in nutrient composition and chemical analysis. Osborne and Mendel, even Hopkins, on the other hand were performing chemical analysis of nutrient content when they found that their animals could not survive on some diets, and so were led to investigate animal disease. Second, Funk's initial investigation into animal disease was not pursued through an investigation of the health of his test animals. Rather, he sought results in the chemical analysis of specific foods. Osborne and Mendel, on the other hand, while concerned for food content, presented the biological results of their feeding experiments as

evidence. The Americans' initial conceptualization of the vitamins was based on biological analysis. That is, not the substance's chemical identity, but its work within the animal body. Funk's concern was a full chemical analysis for the conceptualization of the accessory food factor. The first thing Funk did in formulating the problem was to name the as yet unknown substance in enthusiastic accordance with chemical standards.

The mobius loops of these scientific investigations are resonant with a mobius loop presented by Bergson in his analysis of perception of new objects. At the centre of Bergson's mobius loop is the object of perception. On either side of the centre, or the object, are loops of increasing length. The increasing loops on one side "correspond to growing efforts at intellectual expansion" and the loops on the other side of the object correspond to "the widening systems with which [the object] may be bound up . . . deeper strata of reality" (Bergson 1988: 105). Such a parallel between mobius loops (if such is possible) in theory and in practice shows that work with the problematic idea of the as yet unknown is a working away at the field of the virtual and the field of the actual.²³ We can see clearly in Funk's different approach that the emergence of the new does not consist in finding an object in its right place, but is a relational process, a question of how the world can be encountered. Although the vitamin concept, when it is more fully elaborated, contains reference to physiological function, the name vitamin remains, thereby indicating that Funk's engagement with chemical analysis also bound the object to 'deeper strata of reality' within the scientific community. The question is not one of where the vitamins are to be uncovered but of how the problem is to be elaborated.

²³ Massumi, following Deleuze reading Kant, argues that imagination is a circuit between the actual and the virtual: "Thought-as-imagination departs from the actual, dips into the fractal abyss, then actualizes something new" (Massumi 1992: 101).

Despite his rather beleaguered status in the scientific community and in the history of science, Funk's re-configuration of the problem affected the possibilities for future research. This is apparent insofar as his name for this accessory substance stuck. Such acceptance is more than merely the acquiescence of a community that could not be bothered to think of something else, or that simply liked the sound of the name (there were a few debates concerning vitamin nomenclature). Rather, the acceptance of Funk's neologism signals an acceptance of his work which, I argue, like Pekelharing's contribution, was not the addition of facts to a growing body of knowledge, but the contribution of a qualitative difference to the field, an attempt to configure the problem.

We will see in a later chapter that, for example, Funk's work and progress in the field did not cause the other scientists worry or consternation, but they do use his work and his results when considering how to proceed in their own work. The Americans did not worry that Funk would publish results before they did, but they do refer to Funk's ideas when considering the significance of their own data, and the significance of that data for their future research. They were not worried about the before or after, the more or less of his work. Funk's work provided direction within the field. For example, Hopkins attempted to produce crystals such as Funk had reported and, even though Hopkins was primarily concerned with the physiological action of the substance, he did consider his formulation of the problem in light of Funk's chemical formulation. And as Osborne and Mendel turned their attention to the as yet unknown as a research problem, they speculated on whether their protein-free milk contained Funk's 'vitamines'. Funk's work therefore is not something by which other scientists measure their work in terms of quantity of data, date of publication. Rather, Funk's work allows the scientists to consider their tendencies in approaching the problem.

In this chapter I have considered the role and the power of the 'as yet unknown' in the constitution of the field of bio-chemistry from 1905 to 1911 and I have considered the role of affect in the development of the problems of that field. I have suggested that the scientists' formulation of the 'as yet unknown' as a problem points to the character of the world beyond knowledge, as virtual. That is, as a zone with the potential for various connections, the *capacity* for connection within the world of actualized objects and concepts, rather than a zone where can be found specific connections. I have argued that affect is the experience of the potential for connection within knowers in a changing and problematic environment and have pointed to the power of both affect and the virtual in constituting the field of bio-chemistry in the years between 1905 and 1912. In this next chapter I will continue to analyse the power of affect as it interacts with historical, social and political forces to transform existence and in Chapter Five I will return to the power of affect to shape disciplinary fields and point to affect's enrollment in the politics of discipline development.

Chapter Three

A History of Biochemistry and the Becoming Rat of Early Vitamin Research

In this chapter I take up claims by some historians of bio-chemistry who suggest that the discovery of vitamins was determined by economic, political and social relations at the time. Charles Rosenberg has been suggested that the specific configuration of economic, political and social relations in American agricultural science and bio-chemistry at the turn of the century led to the discovery of the vitamins (1976; 1997). As I suggested in the Introduction, such an account relies upon an epistemology of representation and fails to account for the emergence of qualitative differences in agriculture and biochemistry at the time. I render here a brief historical account of biochemistry as well as the development of the Agricultural Experimental Stations in America. I then give an account of a moment in the history of bio-chemistry when E.V. McCollum, then a junior scientist, scorned by his superiors for his research interests, took flight to the basement of the agricultural institution to trap rats and pursue his labours there. I suggest that a philosophy of interruption, disruption and emergence allows us to maintain what is effective in understanding the limiting and productive effects of economy and politics while allowing room within our ontology and epistemology for the transformative power of that which is in excess of the known.

Biochemistry

Biochemistry is a discipline born of the study of the “typical reactions making up life processes” (Allen 1975: 147). The sciences of biology, physiology and chemistry were all already fairly advanced when the newly unified field of ‘biochemistry’ presented itself. Robert Kohler has argued that biochemistry “became aware of itself as a distinct discipline” about 1900 (Kohler 1972: 181). Biochemistry emerged from the hybridization of two fields of science, chemistry and physiology, and has been, from its inception, more closely allied

with the physical sciences of organic chemistry and physics than with the biological sciences such as evolution and natural history (Allen 1975: 148). Bio-chemistry is the study of the chemical reactions underlying biological processes. However it has not always been known or accepted that chemical reactions underlie biological processes. During most of the 18th century, chemists used heat, water, ether and alcohol to distil organic matter into individual substances. In this way chemists discovered, for example, the substances fibrin, albumin and gelatin, but they could not discover the chemical constituents of these individual substances (Holmes 1963: 53). Chemists could describe the substances but could not determine their basic unit(s). The problems were both technical and conceptual. For example, “one reason that chemists were unable to discern which ... constituents might be the ultimate element of organic substances was that none of the constituents appeared to have constant properties. Acids varied from ‘strong’ to ‘weak,’ and oils were ‘thick,’ ‘gross,’ ‘attenuated,’ or ‘empyreumatic’” (Holmes 1963: 53).

Given that products of organic distillation were substances of varying and various properties, chemists investigating the nature of animal substances and life processes could not render systematically any account of the transformation of one substance into another. Such limited understanding of organic chemistry carried through to the physiology of the time and it was thought that vegetables in the diet underwent “unspecified change of properties until they have assumed the nature of animal substances” (Holmes 1963: 54-55). For example, Albrecht von Haller (1708-1777) proposed that nutrients underwent physical reorganization into aggregates to replace the tissues that eroded during the animal's life processes (Holmes 1963: 55).

Between the years 1780 and 1786, in experiments designed to differentiate the constituents of animal matter from those of plant matter, Claude Louis Berthollet

determined that all “animal substances contain nitrogen” (Holmes 1963: 56). Although he only determined one element of animal matter, his work was fundamental to progress in the field. Antoine-Laurent de Lavoisier (1743-1794) made much progress in determining the elements of organic substances in 1789 while he was investigating the chemical make up of ‘fixed air’. During these experiments Lavoisier burned “many different plant materials in a closed jar filled with oxygen” and found that the products were always carbonic acid and water (Holmes 1963: 56). Lavoisier reasoned that these were not original substances of the material burned but products which were precipitated out during the decomposition (Holmes 1963: 56). That is, they were re-combinations of the elements of organic substances. From these experiments, Lavoisier proposed that carbon, hydrogen and oxygen, as well as Berthollet’s nitrogen, were the elements of organic substances and that all organic compounds might be classified according to their elementary constituents (Holmes 1963: 56). It is important to bear in mind that this is not the site at which carbon, hydrogen or oxygen were discovered, but it was at this time that they were discovered to be the elementary constituents of organic substances.

Following Lavoisier’s proposition, because there was a simple and clear way to envision and conceive of organic substances by their known constituents, there was much more incentive to imagine the processes of chemical change within the animal body. This knowledge provided a stable basis for practices of identification and classification of organic substances, as it seemed that only the relative proportions of the elements varied between organic substances. For example, in 1789 Antoine Francois Fourcroy (1755-1809), “who had already identified and defined the important animal substances fibrin, albumin and gelatin by means of their characteristic solubilities and reactions ... distinguished the three substances by the proportion of nitrogen each contained” (Holmes 1963: 56).

From this point (1789) throughout the nineteenth century, the knowledge of the chemical constituents of organic substances was the starting point for discussion of organic chemistry and the chemical aspects of physiology (Holmes 1963: 57). If their elementary proportions could distinguish organic compounds, it was logical that the only requirement for change in organic substances was a change in their elementary constituents, "either by addition or subtraction of one or more elements, or by recombinations" (Holmes 1963: 58). Between 1800 and 1840, many experiments suggested to scientists that nutritional processes were characterized by balance amongst the elementary constituents of organic substances (Holmes 1963: 64-65). At the same time, organic chemists continued to develop techniques of analysis and were able to develop concepts of organic molecules in which changes in the proportion of elementary constituents enabled the conversion of one substance or compound into another. F.W. Pavy's much cited 1881 Treatise on Food and Dietetics, for example, typifies this approach.

Of the various elements known to exist in nature, only a limited number enter into the constitution of living bodies. The following is a list of those found as constituents of the human body: Carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, chlorine, sodium, potassium, calcium, magnesium, iron, fluorine, silicon, manganese, aluminium, copper. The first four, namely carbon, hydrogen, oxygen, and nitrogen, exist in far larger quantity than any of the others.

The food being the source from which the elements forming the constituents of the body are derived, it follows that food must contain all the elements which are met there with. No article can, as food, satisfy the requirements of life that fails to comply with this condition (Pavy 1881: 14).

With the discovery of the elementary constituents of organic substances, chemists and physiologists began to investigate the significance of the high concentration of nitrogen in animal tissue (Holmes 1963: 62-63).²⁴ In 1816, to decide amongst various hypotheses

²⁴ Biological theories which developed in concert with these new chemical understandings were grounded on the understanding that physiological processes must consist of the exchange of the primary elements between the organisms' living tissues and the substances which entered and left the body (Holmes 1963: 60). I shall not

regarding the controversy, Francois Magendie (1783-1855) performed experiments wherein he fed dogs on nitrogen-free diets and concluded that animals cannot survive unless they receive nitrogen through their diet. From this success were established several principles that would inform nutritional research for practically a century. These principles were: the importance of the proportion of chemical constituents in foodstuffs, feeding experiments and the concomitant methodological principles of purification of foodstuffs and, finally, a concern for the nitrogen content of animals' diets and their excreta.

While work to determine the elementary constituents of organic substance proceeded, so too did work to determine which organic substances constituted food for an animal diet. In 1827, one organic analyst, William Prout, "proposed a classification which arranged food in four groups of principles, viz.: 1st, the aqueous; 2d, the saccharine; 3d, the oleaginous; and 4th, the albuminous" (Pavy 1881: 16; Holmes 1963: 70). Prout and others²⁵ reached this conclusion because these substances were found universally in plants and animals and so they postulated that only minor chemical transformations were necessary to change nutrients into animal tissue.²⁶ Despite this increasingly sophisticated knowledge of chemical analysis, however, physiologists were still left to speculate regarding the chemical reactions that take place between food substance entering the animal and excreta output.²⁷ Such speculation is apparent in an 1879 Textbook of Physiology by Michael Foster who was, as I mentioned in the first chapter, a biographer of Bernard and invited Hopkins to Cambridge.

go into detail here but should the reader wish, Fredric Holmes, "Organic Analysis and Physiological Theories" Isis 54(1) is an excellent starting point.

²⁵ This conclusion was confirmed by Magendie's feeding experiments.

²⁶ Prout's research concerned animal chemistry, specifically the products and components of processes of animal nutrition. In 1815 Prout was the first to argue that hydrogen is the basic composition unit for all matter. In 1920, Edward Rutherford named the atom's nucleus, the proton, in Prout's honour.

²⁷ Some historians of science have argued that such speculation did not detract from the scientific rigour of the method because it left room for a vitalism, which was still considered to be the proper mode of analysis in biological thought (Allen 1975).

Although the body consists, like the food, of proteids, fats and carbohydrates, yet the conversion of the one into the other is not direct. Assimilation does not proceed in such a way that the proteids of the food all become the proteids of the body, the fats of the food the fats of the body and the starch and sugar of the food the glycogen, dextrin, and sugar of the body. ... We have abundant evidence that the various food-stuffs become more or less metabolized, and their elements more or less rearranged and mixed before they appear as constituents of the bodily tissues (Foster 1879: 441).

Speculation of this kind was a hallmark of the work of Justus von Liebig (1803-1873), a central figure in the development of nutritional science and bio-chemistry. Liebig was trained in the techniques of chemical analysis in France between 1822 and 1824 and throughout his career was committed to developing a broad base of chemical analysis so that a thorough physiological theory could be developed. While his training was chemical, Liebig was inspired to investigate the chemistry of animal and plant physiology by “the increasing interest in agricultural problems during the early- and mid-nineteenth century in Germany” (Allen 1975: 154). He was convinced that with sufficient chemical analysis it would be possible to determine the laws by which organic substances were decomposed and recomposed within the animal body. Liebig reasoned that by understanding these processes and the manner in which these transformations occurred, the new fields of animal and agricultural chemistry could develop into sciences by which agriculture could become more efficient, i.e., more productive and less costly.

Liebig thought it would be possible to show “how each atom of the compounds entering a reaction is accounted for in the products” and he thought further that such an account would also provide an understanding of the processes in between reactants and products (Holmes 1963: 73). He argued that all physiological changes that took place in an organism could be calculated (measured and determined) through measuring what the organism took in and what it gave off. He thereby contributed to biological chemistry a

concern to understand physiological changes in animals in terms of input and output balances, a method with much power for many years to come (Allen 1975: 155). A central component to the procedures developed by Liebig was the controlled feeding experiment.

Michael Foster presented this method in his Textbook of Physiology.

We have now to inquire how the elements of such a diet are distributed in the excreta, in order that, from the manner of the distribution, we may infer the nature of the intermediate stages which take place within the body. By comparing the ingesta with the excreta, we shall learn what elements have been retained in the body, and what elements appear in the excreta which were not present in the food; from these we may infer the changes which the body has undergone through the influence of food... Of these elements of the income and outcome, the nitrogen, the carbon, and the free oxygen of respiration are by far the most important (Foster 1879: 411).

The procedure of the controlled feeding experiment and the input and output analysis exhausted the potential of Liebig's science to determine the chemical reactions taking place within animal metabolism. That is, determinations of how reactions proceeded in the animal body could not be made by this method.

Liebig also contributed to scientific understandings of the role of nitrogen in animal bodies. Further to Fourcroy's observation that the albuminous (protein) substances, fibrin, albumin and casein are distinct food substances because of their high nitrogen content, Liebig found that these albuminous foods also had identical proportions of carbon, oxygen, hydrogen and nitrogen (Holmes 1963: 73). Based on this evidence Liebig speculated that the albuminous substances were composed of a common 'proteine' radical "composed of the four major elements, and that the conversion of one into another involves merely changes in the minor elements attached to the protein" (Holmes 1963: 73). From here Liebig also determined that only nitrogenous substances participate in the building of animal tissues.

By 1841, Liebig's research allowed him to state that the basic constituents of animal structure are found in plant tissues and need only undergo minor modifications as they are

integrated to the animal organism through processes of nutrition. These transformations, however minor, were of the utmost importance according to Liebig, as they pointed to the power of living things for chemical transformation. For instance, he observed that while food was similar throughout the plant and animal kingdoms (fats, carbohydrates and proteins), “fats found in cows were different from the types found in pigs or in corn” and he concluded, therefore, that living things “have a remarkable power of chemical transformation” (Allen 1975: 155). Building on this observation, “Liebig²⁸ was the first to maintain that animals possessed the power of forming one kind of organic compound out of another” (Pavy 1881: 13).

In Animal Chemistry, Liebig presented more thorough chemical analyses than had previously been published and also suggested what he imagined to be the transformations undergone during metabolism. Liebig’s work was thus profoundly influential as he presented a comprehensive and coherent theory of animal chemistry. “It seems clear that he was influential also because he expressed coherently and comprehensively a general approach to physiological phenomena which others had been groping after for decades, and he therefore found minds prepared for his theories” (Holmes 1963: 78).

Liebig’s work is important to the history of vitamins not only because he is an example of ‘old school’ physiological chemistry, but also because his ideas informed the work of nutritional and agricultural scientists. Further, his methods and ideas informed the development of agricultural experiment stations in Germany, the model from which American experiment stations were developed. We will see physiological chemists moving

²⁸ In his 1879 Textbook of Physiology, Michael Foster wrote, “Liebig shewed that the butter present in the milk of a cow was much greater than could be accounted for by the scanty fat present in the grass or other fodder she consumed. He also urged, as an argument in the same direction, that the wax produced by bees is out of all proportion to the fat contained in their food, consisting as this does chiefly of sugar” (Foster 1879: 394).

beyond Liebig's methods and knowledge, yet he is important for us here as he had shown that it was possible to conceive of the chemical reactions of living processes, and he had "accustomed physiologists to thinking of biological phenomena in such terms" (Holmes 1963: 81).

Chemical analysis made rapid progress through the mid part of the nineteenth century and the composition of foodstuffs could be determined with increasing sophistication and accuracy. A branch of chemistry, called structural chemistry, developed the idea that proteins not only had a definite chemical composition but also a definite chemical structure and that such a structure affected the chemical property of the protein molecules. For example, by 1900 chemists knew that proteins were large molecules, the physical and chemical properties of which could be irreversibly altered by heat and physical disturbance, that proteins were involved in nitrogen and urea metabolism in animals, that there were different chemical types of proteins, even within the same animal, and that chemical reactions, such as the application of "protein-digesting enzymes such as trypsin or pepsin" could decompose proteins into smaller units, amino acids (Allen 1975: 163).

From this last however, arose confusion at the turn of the century because it was noticed that when proteins were broken down by such chemical reactions, the products varied in size and chemical composition. Therefore, the question for protein chemistry at the turn of the century was whether proteins had a precise chemical structure or whether proteins were made up of smaller units with various chemical structures. Attendant to this question was the question of the chemical role played by proteins in living processes (Allen 1975: 164).²⁹

²⁹ There emerged two schools of thought on the matter of protein composition. One was the colloid school, which "maintained that proteins were large molecules of indefinite proportion, composed of aggregates of smaller groupings" and argued that "proteins had no definite composition" (Allen 1975: 164). The other,

Between 1900 and 1910, the work of Emil Fischer (1852-1919) was key to the development of protein science (Allen 1975: 164). Fischer's work concerned the synthesis and analysis of proteins using the principles of structural chemistry. Fischer sought to deduce the structure of complex protein molecules from "analysis of the cleavage products" (Allen 1975: 164). That is, he sought to determine how the amino acids that he could separate would come together to form the protein macro-molecule.³⁰ Fischer developed a technique whereby he could determine the relative quantity of various amino acids in a protein. By combining this method of quantitative determination with his methods to synthesize amino acids, Fischer managed to join units of amino acids and he developed the 'peptide bond' theory to account for the chemical bonds "by which all amino acids are joined together to form their respective proteins" (Allen 1975: 165). Fischer's work is important here as the problems informing protein chemistry in the first decade of the twentieth century provided much of the context for the development of the vitamin concept. In America, the work of Thomas B. Osborne "had shown that proteins of different origin differed widely, that their amino acid composition was far from uniform and, finally, that these amino acids played specific roles in influencing human nutrition" (Rosenberg 1997: 202).

Establishing the Experiment Stations

Such sophisticated analyses left in their wake correspondingly complex problems for physiology. The great trouble turned on the problem of intermediate metabolism. As we

thought that "proteins were, like other molecules, composed of atoms arranged in definite proportions" (Allen 1975: 164). Although the effects and the influence of the colloid school for the development of biochemistry cannot be under-estimated and the history of the second line of thought is not immune to its influence (even only as it was contested), the development of the colloid school is beyond the scope of this work.

³⁰ This level of structural organization is the primary structure, which is the sequence of the amino acids in the proteins. The secondary structures of proteins result from interactions of local amino acid chains. There are two main secondary protein structure, alpha helixes and beta sheets. Cross-peptide bonds between and amongst alpha helixes and beta sheets constitute tertiary structures of proteins. The bonds which form a protein's tertiary structure occur only between those features which render unique each amino acid.

saw when discussing Liebig, chemistry could provide increasingly sophisticated analyses of the food stuffs that went in and the excreta, heat and movement produced, but what happened in between, the processes by which food stuffs were metabolized, remained a question beyond the reach of the chemical analyst. While chemical analysis could reveal approximations of, for example, nitrogen content to proteins, the technology still did not exist to analyse the chemical processes undergone within animal metabolism. The only conclusive test of the physiological activity of chemically analysed food substances was the feeding experiment. "Coupled with chemical analysis, the feeding experiment technique developed rapidly and proved so fruitful in elucidating nutritional data that in Europe and particularly in Germany, an entire system of research was founded upon the combined procedure: the agricultural experiment station" (Becker 1968: ix).

In 1887, the U.S. Hatch Act established a federal system of agricultural research experiment stations (Paul 1988: 287).³¹ The Hatch Bill, of 1886, "promised each station \$15,000 annually for research and established an Office of Experiment Station within the USDA to coordinate their activities and receive their reports" (Rossiter 1979: 215; Paul 1988: 291). Historians of American agriculture argue that the Bill's success is striking insofar as it followed two decades of intellectual doldrums in agricultural science. There were no scientific breakthroughs within agricultural science in the 1840s and 1850s and state college enrollment numbers in agricultural courses were low (Paul 1988: 287).³² "Given this situation, an inquisitive or suspicious congressman might well have wondered whether ... money had been wasted and rather than finding new ways to spend even more on

³¹ The first state experiment station was established in Connecticut in 1875 12 years before the Hatch Act established a federal system of experiment stations (Rossiter 1979: 214).

³² Despite this, the USDA came into existence in 1862.

agricultural education and research, he might have started asking embarrassing questions, talked about cutbacks, or considered an investigation” (Rossiter 1979: 214).

However, the Hatch Bill was passed at a time when Federal administrators were also faced with agrarian and rural discontent³³, plant disease and insect epidemics, as well as increasing international competition for meat and wheat export. The period was marked by ‘social and economic crisis’ resulting from the overproduction of wheat, the “consequent glutting of world markets” (Paul 1988: 287) and the diminishing fertility of agricultural land (Rossiter 1979). “As the United States entered international commerce following the Civil War, both industrial and agricultural production were thought too important to be left to chance” (Paul 1988: 287). It was thought that state intervention into agricultural practices and interventionist agricultural practices would achieve practical goals. Federal legislators hoped that ‘science-based agriculture’ would alleviate the crisis. Experiment stations, therefore, were a link in a chain of “scientific, social and professional reform” (Paul 1988: 287). Ultimately, despite the situation prior to the establishment of the Hatch Act, American agricultural science in the decades surrounding the turn of the 20th century was hugely successful. “[A] fruitful interaction of massive funding and scientific breakthroughs ... made the period of 1880 to 1920 one of the golden ages of agricultural science in both an ‘internal’ and an ‘external’ sense” (Rossiter 1979: 213).

With the Hatch Act, the government hoped to achieve crop diversification. A three-part policy was adopted to reduce the amount of wheat produced, to increase exports of specialty items and to decrease the US reliance on imports (Kimmelman 1992: 201). Diversification was pursued through “manipulative techniques of hybridization and

³³ This era of American history is marked by profound change in the material and social conditions of life and farmers and rural communities were not exempt from ‘the social question’ (Paul 1988; Rossiter 1979; Rosenberg 1997).

crossbreeding” and experiment stations were understood to be an effective means by which to pursue this policy of diversification through intervention (Kimmelman 1992: 201).³⁴ The work of hybridization in agriculture was not new when it was introduced as part of the policy of the experiment stations.³⁵ “In the closing decades of the nineteenth century [agricultural breeders] had begun using cross-breeding techniques as well as selection in their efforts to create new varieties, following the European tradition of scientific breeding in which Mendel himself worked” (Kimmelman 1992: 201). Techniques of hybridization and crossbreeding served a two-fold purpose for the experiment stations. First, because of the special skill and knowledge needed, they justified an experiment station staffed with skilled scientists rather than those practiced in day to day farming techniques. Second, they were in line with the policy of intervention to achieve state goals (Paul 1988: 287). That is, “hybridization and cross-fertilization [were] valuable for both rhetorical and practical purposes (Paul 1988: 289). Further, science was a particularly well-suited ally as it did not explicitly or apparently have a political agenda and so “it was thought peculiarly virtuous and deserving of support” (Rosenberg 1997: 18)

Crop diversification for export and for domestic markets required science of seed introduction, plant exploration and food values. Therefore, the work of hybridization undertaken at the experiment stations concerned plant physiology as well. Plant physiology and the study of food values of various crops were inseparable from the work of crop diversification through hybridization and cross-fertilization.³⁶ Research into food values of

³⁴ Although we commonly think of genetics research using flies, genetics in the United States in fact began as an agricultural research programme (Kimmelman 1992).

³⁵ Indeed at its establishment in 1903 the “American Breeders’ Association was supported by the Association of American Agricultural Colleges and Experiment Stations” (Kimmelman 1992: 203).

³⁶ “[T]he first three American academic departments of genetics were all located within state agricultural college/experiment station complexes” (Kimmelman 1992: 204).

plant proteins was an integral part of the experiment stations' program of crop diversification (Paul 1988: 290).

Research into hybrid corn³⁷, in particular, was a cornerstone of the experiment stations' success and Thomas B. Osborne did much to further bio-chemical knowledge of proteins with his research investigating the protein composition and value of corn seed at the Connecticut Experiment Station.³⁸ Research with corn was a mother lode for the science of hybridization as each kernel of corn open pollinates and "in a single ear of corn, therefore, the researcher can obtain a large and highly variable population" (Paul 1988: 290). Corn can then be crossbred for different and various values such as oil and protein quantity. Analysis of these properties was an integral part of the success of the experiment stations' corn hybridization investigations³⁹.

Becoming Rat of Early Vitamin Research

Frequently within secondary sources, the history of the discovery of vitamins is given as emerging directly from the economy, or directly from the political principles of the experiment stations. For example, Charles Rosenberg has argued that the "force of economic necessity" exhausted the conditions necessary for the discovery of vitamins (Rosenberg 1997: 235). He says, "[p]rodded by the need to contribute to the economic well-

³⁷ Three geneticists, two of whom worked at the Connecticut Experiment Station, developed hybrid corn in the years between 1905 and 1919.

³⁸ It is undoubtedly the case the Osborne's corn protein analysis work was a part of the Experiment Stations genetic research programme as Connecticut was one of three experiment stations which devoted itself entirely to "purely genetic studies" (Kimmelman 1992: 214).

³⁹ Finally, as I mentioned above, the agricultural experiment stations had their roots in the structure and mandate of the German experiment stations. There is some contention amongst American historians, but it is widely held that American experiment stations were modeled after the German experiment stations. In the decades between 1850 and 1880 most, though not all, American chemists received their doctorates from German universities (Rosenberg 1997: 14). Although their career choice was atypical within the larger society at the time, their travel and training in Europe was the accepted route of academic scientific success (Allen 1979: 177). No matter how closely the system of American stations actually matched those in Germany (which is a point that historians argue over), it is the case that the scientists who were themselves agitating for the establishment of the experiment stations in America declared explicitly that their plans were modeled after the German experiment station and their ideas derived from their time in Germany.

being of dairymen and cattle raisers, research administrators at these experiment stations created a novel intellectual context by bringing the skill of protein chemists to bear upon the problem of explaining experimentally demonstrated inadequacies in seemingly complete diets, thus transcending the limitations of conventional disciplinary training” (Rosenberg 1997: 235-236). As we have seen, the problem of demonstrated inadequacies in experimental diets was not the reason that administrators brought together the protein chemists and biochemists that they did in the experiment stations. It was to work on the problems of cross breeding and hybridization. But beyond this, following Andrew Pickering, I argue that the goals of science emerge in the real-time extension of its practices and culture and so cannot be reduced to any one determinant, such as the economy. I will show that the logic by which one might argue that economic necessity would suffice to force scientific concepts beyond the limits of current disciplinary boundaries, is not, in fact, sufficient to the situation which led to the discovery of vitamins.

Specifically, I will show that it was by a becoming-other than the experiment stations rationale and rationality, that the first rat feeding experiments were undertaken by E.V. McCollum in Wisconsin. As in the story of accessory food factors, this is not a hero’s tale, but an account of the conditions under which ideas were emergent. I am less concerned to highlight McCollum’s genius or bravery than I am to highlight, in his tale, the role of the virtual and affect as conditions that work with “cultural, social and historical forces to actively transform existence” (Braidotti 2002: 137). I suggest that in the story of E.V. McCollum’s first rat feeding experiments we see a moment of normativity in thought and epistemology. My account will be of the power of the virtual in thought and the power of affect in intellectual life as they participate in the production of new intellectual norms (Braidotti 2002: 125).

Couched in these terms it is easy to see this instance of normativity in thought as one of what Deleuze and Guattari call a 'becoming other' and 'becoming animal' specifically. With the concepts of 'becoming other' and 'becoming animal', Deleuze and Guattari hope to capture the intensity of movement provoked by encounters with problems in thought, movement which does not have an object but is rather the emergence of virtual capacities. Deleuze and Guattari follow Bergson and argue that movement, or transformation, change, cannot be seen. Bergson argues that it is a function and a tendency of human intellectual life to spatialize our perception of the world but, with particular philosophical methods, we can also apprehend change and its intensity as they are immersed in temporality. Deleuze and Guattari offer 'becoming other' as one concept to use in apprehending change and its intensity (Deleuze 1987: 233). If intellectual life is normative, as I have suggested, then this moment of becoming other is central to historical epistemology insofar as the ability to institute new norms necessitates a becoming other than what one is at the moment. I am going to suggest that McCollum not only experiences an intensity of transformation but also actualizes transformation in the experimental station where he was employed. I suggest that McCollum enters into relations with already actual entities and ideas in such a way that he effects a specific becoming other - a becoming rat. McCollum enters into relations with the experimental station which are redolent of the relations which constitute the rat such as it is within the framework of the experimental station. As McCollum does not only change his own mind in his becoming rat, but ultimately his becoming rat effects the implementation of structural change, this is a political tale and becoming animal is ultimately a concept of political power.

Finally, I suggest that this story exemplifies quite well, and again, the need for a polytemporal framework to grasp normative intellectual processes. By introducing the

problem of this new experimental animal to the institutional setting of the Experiment Station, McCollum effectively institutes his own 'as yet unknown' and therefore introduces a new chronology, specific to this event, to the typical chronology of the Experiment Station (Deleuze 1987: 263). Time, therefore, is not best conceived as a linear line that passes through all contemporaneous events. Rather, events of various durations exist contemporaneously.

In July 1907 Elmer Verner McCollum (1879-1967) took up a post at the Wisconsin Agricultural Experiment Station as an agriculture chemist and was assigned to participate in "the most talked about experiment in the field of nutrition" (McCollum, n.d.).⁴⁰ The experiment, which had been underway for two years already, consisted of feeding cattle and analyzing the chemical constituents of their foods and excreta. McCollum, who had "never conducted an animal experiment, ... in a few weeks ... concluded that we would never succeed in anything worth while with this type of experiment, large animals with very complex mixtures in their rations" (McCollum, n.d.). Thus, there is here the beginning of a normative process in intellectual life. McCollum perceived that to continue with large animal experiments, the established norm, would not provide anything 'worth while'. That is, experiments with large animals would not contribute anything of value to intellectual life, nothing which would contribute to continued vitality of McCollum's chosen field.

McCollum concluded that he was in a dilemma. He had "lost faith in these farmers" and wanted to throw in the towel on an experiment, which "was very much talked about by animal husbandry men everywhere and even people who had heard about this great experiment at the agricultural college" (McCollum, n.d.). It was McCollum's first job and he was excited because he had been assigned to a renowned experiment. Yet he arrived at the

⁴⁰ All McCollum quotes in this chapter are taken from an undated tape recording of a speech given by McCollum, which I found in his archives at Johns Hopkins University.

station and found he could not concur with the research process, designed as it was with large animals and complex rations. McCollum had expected and hoped that his new job would present him with an intellectual environment in which he and his science could thrive. McCollum thus perceived the potential curtailed by the order and the organization of the work at the Experiment Station (Massumi 1992: 99). "I must produce something of scientific value and advance myself in productive society, there I was stuck with this ... experiment and unknown consternation" (McCollum, n.d.). His consternation is an intensity developing as his potential is curtailed and a desire without an object develops. This is McCollum's experience of "bodily thought, beyond the realm of possibility" (Massumi 1992: 99).

In this melding of desire for his work and perception of his work, the "stimulus-response circuits", of both McCollum and his science, were suspended and there was created a "zone where chance and change may intervene" (Massumi 1992: 99). McCollum's habits have become unhinged and while his stimulus response circuit has been broken, neither has a different possibility been installed in his intellectual habits yet. In this moment, McCollum catches a glimpse of a 'zone of indeterminacy' in thought and in experimental practice, in his world. He cannot yet say what those other possibilities might be but, upon catching this glimpse, he is forced to think. McCollum was lost and at a loss for he needed a new problem, something new which would constitute the "labour of perception" (Deleuze 1987: 283).

I began to find my way by approaching 37 volumes of a German book of all the chemical literature on all the animal and plant substances known to physiology and covered everything from 1870 on. Of evenings I perused the pages from volume 1 to 37 studying the ... the researches of the past 37 years. I read it day by day (McCollum, n.d.).

McCollum's 'way' could emerge within these German texts because he was living and working in a 'zone where chance and change' might intervene. I do not wish to suggest that

McCollum thought that the answer was probably in those German volumes. I mean to suggest that McCollum's 'way' was in those German volumes because he was in that intensity of becoming which we have all experienced when driven by desire without even a problem yet formulated to guide us. This is the moment when the desire for a productive way through informs all our acts of perception, the state of "perception of things, thoughts, desires in which desire, thought and the thing have invaded all of perception" (Deleuze 1987: 283).

I pause in the account briefly here because McCollum's initial encounter with the German texts is a nice site at which to begin to develop a sense of the distinct duration of this 'becoming other than the Experiment Station'. I have already said that McCollum has effectively introduced his own, 'as yet unknown' into the Station's typical chronology. McCollum is willing something for his intellectual future, but he cannot yet conceive of what that will be (hence his consternation). He is willing the unimaginable into existence. His becoming other is therefore an event of the untimely and will develop a duration all its own. In this instance the duration that develops begins in 1870 with 37 volumes of German chemistry literature. And this is a distinct duration from that of the Experimental Station. McCollum's time is poly-temporal (Deleuze 1988).

Because McCollum is immersed in this zone of indeterminacy and because the potential for selecting ways forward is increased, McCollum is enabled in a process of imagination (Massumi 1992: 100). His imagination is what enables McCollum to move from the actual experiments as they were running at the experiment stations at the time, to his experience of the zone of indeterminacy when he felt the experiments were failing and in turn to actualize something new and different (Massumi 1992: 101). In his search of the literature, McCollum found a solution in the form of a problem. A new problem was the

solution to a problem that had been posed in such a manner that it would not be solved. McCollum found in his review of the German literature that someone had fed “little mice” on rations of purified foodstuffs and that the mice had failed.

I found a dozen or so of [the] like that had tried similar experiments mostly with mice and all reported failure so I made note of this. And the most important problem in nutrition research was to find out why a mixture of purified substances re-assembled - that physiologist [and] biochemists could name resulted in failure to the animals (McCollum, n.d.).

No longer was the problem the effect of different rations on the growth and reproduction of cattle. Now the problem was the food value of individual foodstuffs, the isolation and testing of increasingly small food components.

Having developed for himself and his work a new problem, McCollum had the daunting task of presenting his becoming other to his superiors, the face of the institutional establishment. “So having worked out this plan and tabulated my notes some 2 or 3 hundred [experiments] which I wanted to try with natural foods, I had the obligation to go in and tell Hart that I didn’t believe in the cow experiments” (McCollum, n.d.). McCollum argued that to pursue research with cows was effectively to block progress in research and that he felt obliged to begin “researching in some other way” (McCollum, n.d.).⁴¹ Cows, representing the agricultural college’s interests to itself and its funding bodies, would stop the process whereby research at the station might lead to new knowledge. “I suggested we use small animals that we could see in a reasonable time. Animals which would grow rapidly, reproduce at an early age and would answer the sun of life of rapidly” (McCollum, n.d.). McCollum suggested that Experimental Station work be undertaken using rats. McCollum’s desire to use the rats, and the rats themselves, disrupted the signifying project at the agricultural college, that is, what the college represented to itself and McCollum’s superiors

⁴¹McCollum’s expression of his new idea as a lack of faith attests to his status ‘becoming-other’, particularly in light of the title of Charles Rosenberg’s book on science and American social thought, “No Other Gods”.

responded accordingly. As McCollum tells it, “[Hart] was angry” (McCollum, n.d.). Another account of McCollum’s initial presentation of his rat idea has Hart presenting his views rather more explicitly, “Grow rats, the farmer’s worst enemy, in an agricultural college, using Federal money? NO!” (Becker 1968: 118). Rats were not partners in the advance of knowledge; they were rather, “evil harbinger of pestilence” (Clause 1993: 331).⁴² McCollum invokes horror in his superiors in part because he failed to recoil at what was the relational limit of the science being undertaken in the Experiment Stations at that time.

In this moment, McCollum’s becoming other is clearly a becoming animal. Rosi Braidotti argues that, “becoming animal is a brush with the bottom line, with the outside within” (Braidotti 2002: 136). In his appeal to rats particularly, McCollum evokes a two-fold horror. First, he clearly evokes a horror of the rat as a scourge to civilization, a scourge because it lives in a parasitic, and thereby symbiotic relationship with the very civilization(s) to which it is a scourge – the outsider within. Second, rats eke out an existence. They are not cultivated in either sense of the word. Rats are again the outsider within. Which returns us to two points alluded to early in this chapter. First I drew on Canguilhem’s vitalism to suggest McCollum’s becoming other as a moment of normativity in intellectual life. Thus in appealing to the normativity of the organism, I suggest that becoming animal is a thrust of normativity in intellectual life, or becoming other in intellectual life. And second I suggested that all becoming other is political. Here, the competing notions of intellectual life – McCollum’s becoming rat and Hart’s rigidly normed insistence on using cattle – turn the space into a political space.

⁴² Although the horror of his superiors was clearly tied to common sense understandings of rats as pests on farms, it is also interesting to note that even at this institute for scientific research there were no ‘lab animals’ as such. Although the experiment station had animals on which to perform their research, these cows were rather more farm animals than ‘lab’ animals. I will discuss the development of lab animals as a standard very shortly.

In this political space McCollum found an ally in a senior scientist, Babcock. This alliance was formed in a relationship that would typically cultivate similarities, rather than encourage becoming others.

Later I told Babcock what I was thinking, he had sat beside me as I worked in the lab ... giving me such things as a wise man could talk to a young man about. In return I told him about the current experiments recorded in physiology and biochemistry journals. ... He was fired with enthusiasm (McCollum, n.d.).

McCollum refers explicitly to his youth as a foundational aspect to their relationship and I suggest that it is precisely McCollum's 'becoming young' in relation to Babcock that this alliance could encourage becoming other. Deleuze and Guattari argue that becoming youth is not about remaining young, but "extracting from one's age . . . what constitutes the youth of that age" (Deleuze 1987: 277). McCollum was 28 at the time, not so terribly young, but what constitutes the youth of a man just starting a research position is the enthusiasm and excitement for the unknown possibilities of new research projects. The youth of a junior scientist is this willingness to try to keep rats for the sake of testing out his sense of the possibilities. It is therefore the intensity of this becoming youth that enables McCollum and Babcock to connect in such a way that their alliance could cultivate McCollum's continued becoming other.

Babcock could not grant McCollum permission to perform experiments with rats, or secure him research funds or lab space for his rats, but he did support the junior scientist in his ideas and soon he appealed to the administration on McCollum's behalf.

The next day Babcock came in and sat a little longer than usual before he said anything and then he said we got it all wrong we must do this thing anyway. Who was I to do things anyway? I was instructed in chemistry with a take home of 1200 a year (McCollum, n.d.).

Babcock here is also the intensity of becoming. He performs a kind of leap in place, like a catalyst, and connects the virtuality of McCollum's becoming to the actuality of the

institution. Babcock displays the pragmatism of the scientist in effectively calling forth the as yet unknown, enabling thought's creativity in the face of thought's power to judge.

So [Babcock] went and talked to [the dean] and took me along and nobody talked like the Babcock and I can tell you a long story as to why they didn't but it was the art of pleasure. And he said what would come of the line of investigation I suggested using in rats (McCollum, n.d.).

As the result of Babcock's intervention, McCollum was permitted to pursue his rat experiments. The permission that McCollum was granted, as the result of Babcock's intervention, took the form of one administrator's allowance that he would ignore the work and that thereby the experiments might be performed in secret. The work was not to be official experiment station work and administration did not care to hear the results.

They all assented and so I was permitted to go ahead. But they never made a project, they didn't want to run the risk of ridicule for a project on rat feeding and so no funds were allocated to us (McCollum, n.d.).

McCollum's becoming other is a vector of transformation within the institution's own space and time, which we can see in the back and forth between venues that McCollum's project entailed. The work begins in a "borderland zone between modes of action" (Massumi 1992: 106). Thus, McCollum's becoming other had its own duration that existed contemporaneously with the other durations of the Experiment Station. First McCollum approached the administration, then more meekly, he approached a more senior colleague and then finally the senior colleague approached the administration on behalf of McCollum. McCollum's becoming had to persist through the time that McCollum got his nerve up to talk to his administrator in the first place, this was replaced by the rapid dismissal of the idea, only to be slowed down again until McCollum talked to Babcock, the slow wait until Babcock came back with the decision to 'do this anyway', which introduced a sudden change in McCollum's position that still had to endure through the wait of Babcock's meeting with the dean.

Initially, McCollum's research had to be kept a secret and he trapped his rats and set up his feeding experiments. McCollum's was a secret, subterranean relationship with the rats in the basement. Thus, McCollum's becoming other does not stop with permission to perform his experiments. Indeed, with the space and time available for him to do so, McCollum becomes rat. Significantly, where the process of becoming other, discussed above, was an encounter in the virtual, becoming rat is a process of actualization. We see McCollum becoming rat in his desire to escape the physical constraints of the materiality of the experiments designed at the experimental station. What McCollum actualizes is a becoming rat. It was because the rats did not correspond to the identity of the work of an agricultural college, a scientist employed therein, or animals employed thereby that the experiments were a secret and subterranean development. It is this discourse of power and the regulatory regime that renders McCollum's idea of rat feeding experiments positively absurd, and it is via this apparatus that McCollum is consigned to the basement, closed off in the stone subterranean walls so that the agricultural institute can continue to stand as the apotheosis of rationality in man's relation to nature and food. "If becoming-animal takes the form ... of monsters aroused in the imagination by the demon, it is because it is accompanied, at its origin as in its undertaking, by a rupture with the central institutions that have established themselves or seek to become established" (Deleuze 1987: 247). That is, animality is produced through the same discourses of power and regulatory regimes that seek to insure rationality. McCollum's rat experiments develop "beneath assignable relations" – quite literally so as they take place in the basement (Deleuze 1987: 239). These assignable relations are the rationale of the experiment stations but are informed by mythological dread and horror of rats. "Both science and popular culture are woven of fact and fiction" (Haraway 1989: 3). We see the mythology of rats at work in the agricultural science at the

time when McCollum's superiors denied the rat as a possible lab animal precisely because it was the farmer's worst enemy.

In the denial by superiors of support for his existence, whatever horror clings to his choice of experiment animals, his own flight to and persistence in the dank, perpetual night of the basement, McCollum enters a zone which is also occupied by the rats, the underside of the rationale and rationality of science and the experiment station. When he decided to eke out his existence by moving to the basement and working in secrecy, McCollum entered into relations with his surroundings – his work, his superiors, the stratification of academic disciplines – he gives a performance from the rat's oeuvre. Within this process McCollum 'goes feral'. That is he occupied "a semi-wild state on the boundaries of hierarchical civilization" (Baker, 2003: 149). He enters this process of becoming rat not through his relations with the rats, but with other things, in such a way that something rodent emerges out of the relations between McCollum and these things (Deleuze 1987: 231).

This story belies the fiction of science as a series of progressive advancements along routes of logic and through the systemic elimination of error. We can see this tale as a series, certainly, but a series of disruptions, upsets, moments of panic and anxiety. Michel Serres draws upon a tale, which is redolent of McCollum's story, in his account of the progress of social relations. Serres uses the fable of the city rat and the country rat to explain that in considering the constitution of human systems it is essential to consider the excluded third, that which is constructed as outside the system, a parasite, or an intruder (Serres 1987). The city rat invites the country rat to sit with him and eat the leftover bits from a human's meal. Neither rat has produced anything. The invitation to dinner costs neither of them. The human, who owns the house where the rats dine, has not produced anything either. Serres calls him a tax farmer.

The rats' meal disturbs the human in his sleep and he, in turn, disturbs the peace of their meal when he bursts in on them in the dining room. In this story all the players interrupt. They all interrupt what would be the pure logic of the system. But the logic in this tale is to interrupt. Out of everyone's panic, in their encounters with that which is outside the logic of the system, is created the system of relations (Deleuze 1987: 73). What is important is that the systems of relationships are of guest to host, where even the host (the farmer and the city rat) is a parasite. It is the parasite, the interruption, which moves the system forward, irreversibly. It is not necessarily along the lines of reason, a cause leads logically to an effect that a system passes. Rather, the system is conditioned by the obstacles it encounters. Surprise, the power of the leap that is beyond the rational, sparks the new order. The parasite, as the one who is the source of excitation in a system, is also the excluded one from that system. As in the tale of the rats' meal, the parasites drive one another out to feed.

It is not that McCollum begins to act like a rat, scratching at his bread with little beady eyes. Rather, McCollum enters into a process of becoming-other to the façade of rationality presented by the experiment station. McCollum forms an alliance with the rats. They are each pests - to agricultural science and agriculture, respectively. Within the context of the Experiment Station, studying agricultural pests was to proceed by way of vector of contagion. That is, McCollum's idea of rat experiments appears to be an interruption of the rationale of the experiment station. However, Serres would have us see that such irruptions, moments of surprise, are constitutive of the system. Certainly moments of surprise and irruptions found normativity. Were it not for surprise and irruption, norms, rather than normativity would suffice in life.

In this chapter I have argued that if we follow Canguilhem in understanding normativity to be central to healthy intellectual life, Deleuze and Guattari's notion of 'becoming other' is useful for exploring the time and the ontology of normative encounters with a changing, emergent and problematic world. I have suggested that the virtual and affective aspects of McCollum's becoming other to the Agricultural Experiment Station interacted with 'historical, social and cultural forces to transform existence'. In the next chapter I will investigate the materiality of experimental systems by which scientists solicit the as yet unknown and host the future.

Chapter Four

Encountering the Unknown in Animal Feeding Experiments

In this chapter I turn my attention to the materiality of the experimental systems by which the scientists sought to determine the presence of the accessory food factor and to elucidate its effects on growth, reproduction and well-being. Just as in the previous chapters, I am concerned here with the workings of the 'as yet unknown' in scientific practice and I argue that the materiality of the experimental systems functioned to host the unknown and to solicit the future (Livingston 2006: 115).

I begin with an account of the development of the standardized rat as a technical tool within experimental systems. This is the tale of how rats moved from being basement dwellers that invoked horror in scientific administrators to the standard lab animals that they are today. I render this account as a kind of history of the technical objects in the experimental systems used by scientists engaged in research into the unknown dietary factors. I then engage with sociological accounts of lab animals which only consist of the ways in which lab animals are 'like' technical objects and also, by extension, the ways lab animals have been stripped of that which is particularly animal about them. I argue that an account of productive animal experimental systems requires more than the account of animals as technical objects.

Finally, therefore, I will consider some of the ways in which the materiality of the experimental systems is productive of new knowledge. That is, I will consider how problems are constituted, materially. Put differently, I will account for the ways in which the experimental systems under consideration here produce something akin to the changing and problematic environments that Canguilhem suggests compel organisms to live in normativity. To this end I will consider the material production of problematic environments within experimental systems through the establishment of feeding rations,

repetition and rhythm within the experimental systems and finally through the scientists' sense of the animals' well being as established through touch and visual observation.

The Rise of the Rat⁴³

Of course, just as there is not some universal categorical horror attached to the rat, neither is its usefulness to science pre-determined. Much thought has gone into the choice of experimental animal by biologists as well as sociologists and historians of biology. My focus in this chapter will be on the development of the standard lab rat as, "it is generally agreed that most of our knowledge in ... nutrition and endocrinology, has come from observations on this animal" (Richter 1968: 403). Such acceptance required changes in disciplinarity and methodology. The role of the rat in the discovery of vitamins and the development of the vitamin concept cannot be underestimated and requires sufficient attention to the role of the rat in the experiments that led to the discovery of vitamins.

It has been argued by Nobel Laureate, Hans Krebs⁴⁴ (1900-1981), that August Krogh⁴⁵ (1874-1949) determined a principle of biological investigation when he stated that, "[f]or many problems there is an animal on which it can be most conveniently studied" (Krebs 1975: 221). The principle, called the Krogh principle, urges scientists to search for an organism which displays a 'minor modification' of a 'standard biological situation', as these "may present great advantages in studying a phenomenon without affecting basic principles" (Krebs 1975: 225). According to Krebs, Krogh articulated this principle when he argued that scientists must deliberately and diligently search for an organism suitable to their

⁴³ I take this sub-title from (Holmes 1993), "The Rise of the Frog", though it does not have nearly the same literary resonance.

⁴⁴ Krebs was awarded the Nobel Prize in Physiology or Medicine for his discovery of the citric acid cycle.

⁴⁵ Krogh was awarded the Nobel Prize in Physiology or Medicine for his discovery of the capillary motor regulating mechanism.

investigative needs.⁴⁶ So, for example, the frog has been the animal of choice for scientists investigating “mechanical explanations of muscular motion” because of the “convenient size” of its muscle and the “magnitude and tenacity of the contractions it could produce in isolation, did not affect the ‘basic principles’ of muscle contraction” (Holmes 1993: 315 - 316). Or, the fly, for example, was particularly suited to investigations of heredity.

[I]t was small, easy to raise in the laboratory, able to withstand mutation and crossbreeding experiments, fertile all year without interruption, and prolific, producing a new generation every twelve days, or some thirty generations per year, with each female producing close to a thousand eggs; males and females were easy to distinguish; it had only four chromosomes. In short, it was the ideal organism for studying heredity (Jacob 1998: 34).

Choice of experimental animal is of such serious consideration because of the possibilities such an animal presents, but also because of the limitations presented by the animal. “[T]he very nature of an animal, that is, its structure and physiology, limits the research possibilities to certain types of experiments” (Jacob 1998: 53). Certainly we can see the Krogh principle at work in McCollum’s becoming-rat as he searched, if not for ‘the right organism’, certainly for a better organism, based on his experimental needs.

Rat as Technical Object: the Biology

There proved to be aspects of rat physiology, which did, in fact, make rats the right organism for the job of physiology experimental animal. Indeed, it turns out that several of the reasons for using rats, presented by McCollum to his superiors, later became precisely the justifications used by biologists concerned specifically with the use of the rat in the lab. Rats were particularly suited to much scientific research underway at the time for several reasons. Rats were good laboratory animals as they were small in size and easy to keep and

⁴⁶ Robert Kohler has argued that because of the huge explosion in experimental biology between 1906 and 1912, simply finding an organism particularly suited to one line of investigation could make a career.

care for.⁴⁷ In the early nutrition studies, as well as in other physiological and biochemical investigations undertaken at the time, the tenacious qualities of rat bodies made them particularly suitable.⁴⁸ Rats were suitable for investigations into growth because, as is also the case with humans, the young are born relatively immature and consequently much of the developmental process is available for observation by the scientist. Also, the reproductive cycle of the rat is relatively short and so progress in scientific observations could be made quickly (Fujimara 1996: 7). The rat has become a 'laboratory workhorse'; which has resulted in a vast accumulation of data, not only in reference books containing information regarding the rats, but also in the bodies of the rats as model systems and reference libraries (Clause 1993: 330).

Further, rats bear litters, rather than one infant. This meant, of course, that scientists had many research subjects available to them from a single mother. Second, their rapid growth rate is not found in humans. Rats will grow at the rate of 10% per day from 50 or 60 grams at birth until they reach their ultimate adult weight of 200 to 300 grams (Baker et. al. 1979: 11). This growth rate distinguished rat physiology from that of human physiology but also made rats ideal for use in nutritional experiments where quick effect of experimental diets is beneficial – just as McCollum argued.

Further, the complexity of rat nutritional needs signaled to scientists that they would make an ideal standard mammal. While fruit flies, for example, are not fussy eaters, the nutritional needs of rats are fairly complex, as they are for mammals in general (Kohler 1993: 309). It was "the accepted view that the similarities between mammals having the same food habits tend to be close and that ... results obtained with one form can be very precisely

⁴⁷ Despite this, Robert Kohler has argued that rats acquired some prestige as a laboratory animal because they were so expensive to maintain. One can only think that rats were expensive to maintain by comparison with the cost of maintaining a *Drosophila* community.

⁴⁸ Much of the early work performed with rats concerned growth and developmental processes.

transferred to the other” (Lindsey 1979: 5). Finally, rats were an ideal experimental animal insofar as they are mammals, and so would provide evidence of mammalian physiology and biochemistry. That is, the great advantages offered by the organism for their investigation (their size and reproductive rates) do not negate the generalizability of the results (Holmes 1993: 317).

The work to establish the ‘standardized’ rat was “undertaken at the Wistar Institute in Philadelphia as a way of serving the needs of science while promoting the goals of a fledgling scientific institution” (Clause 1993: 331). The Wistar Institute was established in 1892 and began breeding a line of rats known as the WISARAT in 1906. Although standardization was not an initial goal of scientific work with rats, standards and standardization became a goal of the Wistar Institute in 1910. “The Institute should establish standards of all kinds, [the Director] maintained, beginning with ‘standards in anatomical work’ and extending even to the establishment of a system of standard magnification for all illustrations to be published in the Wistar Institute” (Clause 1993: 341). When, in 1912, this interest in standards met with a determination that “the rat colony was fundamental to the successful work of the Institute” the standardization of the rat became a priority for the Wistar Institute and “by early 1915 the rat colony had become a well-established fixture at the Wistar Institute” (Clause 1993: 343). It is estimated that more than half the world’s lab rats today are descendents of the Wistar rat line established in 1906.

While much work to standardize rats was undertaken at the Wistar institute, it is interesting to note that those developments took place after the establishment of rat colonies at Wisconsin by McCollum in 1907 and at Yale by Osborne and Mendel in 1909. For an organism to function effectively as a standard experimental animal there must exist a network by which a steady supply can be obtained by scientists (Dierig, 2000: 6). Wistar

could not yet provide that supply and obtaining rats from the basement proved disagreeable. We know that McCollum procured his rats from a fancier and bred his colony from these.⁴⁹ Osborne and Mendel got the idea to use rats from McCollum, so presumably they followed suit in obtaining their rats from fanciers as well.

I suggest that, in all likelihood, nutrition research proceeded at a faster pace than would have been possible had Osborne and Mendel or McCollum relied upon the Wistar rat. Rats could only move to the conspicuous place they now occupy in experimental science with knowledge of the conditions necessary to sustain their organism. That is, a comprehensive knowledge of the rats' nutritional needs was necessary to produce a reliably uniform animal (Clause 1993: 346). Therefore, the development of the standard rat was not simply a breeding program, but also a feeding program. Because their investigations were of various and specific food values, Osborne and Mendel as well as McCollum fed their animals a purified diet, as they thought such a diet was the key to definitive nutritive studies (Lindsey 1979: 14). On the other hand, Wistar rats were fed with "scraps from local restaurants" until 1920 (Lindsey 1979: 8). A diet of kitchen scraps would have set back the development of a physical standard. This is apparent since kitchen scraps were precisely the source of beri-beri in chickens, which Eijkman pursued with such success. It is interesting to note in this regard that the "Yale strain" developed by Osborne and Mendel was known for its large size.⁵⁰ It is easy to speculate that this large size was a result of generations of good nutrition. In a Wistar Publication of 1923, the contribution made to the development of the standard rat by

⁴⁹ Rats were domesticated to some extent by both sportsmen who fought them and fanciers who bred them. In this way Norway rats became easier to handle and sporadically made their way into laboratories for nutritional research as early as the first half of the nineteenth century (Lindsey 1979: 2).

⁵⁰ Osborne and Mendel's 'Yale strain' and McCollum's rat colony programme also differed from the rats at the Wistar institute insofar as they were randomly bred whereas the animals at the Wistar were inbred. Rats at the Wistar were inbred so that they might provide animals with "theoretically the same genetic constitution" (Fujimara 1996: 7). This was done "to provide a defined and consistent genotype for analysis" (Fujimara 1996: 7). Genetic consistency would not have been as important to Osborne and Mendel or McCollum as genetic variability was not the object of their inquiries.

nutritional research is made explicitly. "Recent observations on the occurrence of rickets in human subjects reared in darkened rooms as well as experimental tests on albino rats indicate the necessity of light to obtain perfect metabolism" (Greenman 1923: 19). In their 1913 report to the Carnegie Institute, Osborne and Mendel wrote that a particularly gratifying aspect of their work was the cooperation with other scholars that they could undertake by distributing some of their stock. "It has been gratifying to be able to cooperate with investigators in various parts of the country in furnishing experimental animals and, to a limited extent, special feeding products" (Osborne 1914: 369). Others must have wondered, 'what are they *feeding* those rats?'

Rats as Technical Objects: the Sociology

While the biological suitability of rats for physiological experimentation does explain the rise of the rat in experimental system, couching the change solely in terms of the suitability of the animals' physiology almost hides the change more tightly under wraps than it does elucidate the changing status of the rat. For if the issue of acceptability of lab animal was solely a question of physiology, McCollum's superiors would have responded much differently initially. The matter was not that McCollum's superiors were horrified at a lab animal which would breed faster and enable faster results. Just as their response was informed by the power of myth and history, so too was the standardization of the rat a constellation of science and fiction.

Bonnie Tocher Clause has argued that the Krogh principle of finding the right organism for the job can be "turned on its head and extended to identifying the right job for the organism" by examining the history of the lab rat (Clause 1993: 330). And if we can see McCollum using the rat as a better organism for his scientific work, we can also see in his superiors' response this reversed principle as they cry, "science is not the right job for that

organism!” Scientific research, McCollum’s superiors protested, was no place for a rat. According to Clause’s reasoning this response might be explained by the fact that “until their use in laboratories became well established [rats] were universally regarded as a useless scourge to humankind” (Clause 1993: 330).

Despite the initial question of whether scientific experiment was the work suited to rats, the horror and revulsion did eventually subside. Rats moved in the scientific and lay imagination from a farmer’s pest and a scourge in the city to something from which it was worthwhile to learn about food, nutrition, development and growth. The rat became a cultural icon for scientific investigations. The rat became not only standard but, the standard for lay imagining of scientific practice.

In re-counting the rise of the rat as a standard laboratory animal, I want also to draw upon the rise of the mouse in experimental practice.⁵¹ Although mice have not met with the same outright horror as have rats, mice are something that make some members of the public squeamish. I argue that while the horror of the rodents needed to subside for their place as ‘legitimate’ purveyors of knowledge to be secured, because of the tone of the public sentiment against animal experiment in America at the time, a small dose of distaste went a long way to the general acceptance of rats and mice as standard laboratory animals. While rats could not be too distasteful, neither could the general appreciation of them cross the boundary that separated them from pet animals such as dogs and cats. Rats and mice could be accepted as scientific laboratory animals in part because of “the kind of cultural

⁵¹ The leading institution world-wide breeding mice for experimental purposes is the Jackson Laboratory, founded by C.C. Little (1888 – 1971) 1929. On the 25th anniversary of both Jackson Laboratories and Mickey Mouse, Little wrote that he would like to undertake some creative endeavour with Walt Disney because, “Jackson Laboratory . . . has done for the mouse in science what Walt Disney has done for it in amusement” (Rader, 2004: 5). Allowed it in, that is. Made it respectable, put trousers on its bottom and rendered it thereby half decent.

boundaries that separated mice [and rats] from other species of pet animals” (Rader, 2004: 35).

I noted above that, once he had returned from the basement, McCollum obtained his rat colonies from a rat fancier. The presence of a rat and mouse fancier community in the U.S. at the time also provided a broader context within which the breeding of rats and mice for specific traits might be understood and accepted as a “cultural activity” (Rader, 2004: 33). Indeed, one of the most active fancier communities in America was in New England, and so Osborne and Mendel’s use of rats in Connecticut would have benefited from the acceptance of rat and mice breeding as a legitimate cultural activity. “By transforming [rodent] material into something of human utility, fanciers ... appear to have lowered the practical and ethical thresholds for putting mice to ... human uses” (Rader, 2004: 36).

The development of rodents as animals for agricultural science was encouraged by a particularly fortuitous moment in the emergence of mice as standard laboratory animals. In 1908, Harvard’s W.E. Castle needed to expand his mouse colony and was given space to do so at the university’s Bussey Institute for Applied Biology, formerly Harvard’s undergraduate school of husbandry and agriculture, which had “just reorganized as the ‘graduate school of applied biology related to agriculture and horticulture’” (Rader, 2004: 30). Though the general public may not have noticed the move to the new institute for agricultural studies, I suggest that university administrations nation-wide must have noticed that Harvard was moving mice into its institute for graduate studies in agriculture and horticulture. Because agricultural science at the time was tied so tightly to genetic research, sympathy and “strategic links” were formed between the Bussey’s standard mouse program and Harvard’s other biological sciences (Rader, 2004: 31).

Significantly, much mouse and rat research has been genetic research and much work has been undertaken under the heading of developing a standard mouse or rat. Public sympathy for the use of rats and mice as standard lab animals would have developed because the standardization of rats and mice was part of the widely understood and supported research programme of Mendelian genetics and was used in research into a 'pure line' hypothesis. The importance of this genetic research to the general acceptance of mice and rats in experimental practice cannot be underestimated. For example, I think it not unimportant that the rats used as standard lab rats were 'white' rats and were bred as 'albino' rats. Rats and mice were "standardized at the locus of the gene" for their whiteness and became purified thereby (Rader, 2004: 18). These rats then are imbued not only with the purity of snow and the power to emanate such purity, but also there is established a kind of conceptual back and forth whereby the rats grant to the science neutrality from their whiteness just as science grants to rats the neutrality of 'objectivity'. The genetic 'blankness' of standard white rats allowed them to function as an 'absent centre' of truth. In the popular imagination, therefore, one is not taking feeding lessons from rats, but from this empty, neutral, central source of 'science' and 'scientific truth'. Perhaps it might even be suggested that we are doing rats a favour by imbuing them with whiteness, a sense of benevolence in the public imagination for granting rats a 'white' status.

Scientific Suitability and Cultural Change

The acceptance of the rat as an experimental animal is written into the methods and standards for keeping and working with these animals. Scientists make statements concerning the ease with which rats are "reared and cared for" (Osborne 1912: 233). This grammar changes the character of the rat-human relationship, from one marked by pestilence to one marked by the benevolence of care. In many ways the presentation of

those aspects of the animal which make them particularly suited to experimental research are such that their acceptance as experimental animals and indeed, experimental standards, is couched right in these terms. "After birth the young white rat depends upon the mother for sustenance for about 20 days" (Osborne 1911: 15). Dependent, young and white, these rats appear more like Shirley Temple than they do a harbinger of pestilence. The coat of the newborn rat is a "puppy coat of white downy fur" and there will remain "puppy fur on the head" beyond the time the rest of the body has grown an adult coat (Greenman 1923: 83).

In rearing and caring for lab rats, scientists needed not only to feed them, but to house them as well. The presentation of the cage in which the animals were kept allows the concept of the rat as something that hides in the basement to be burned away under the lab's fluorescent lighting. Beyond the specifics of the cages, simply the presentation of the rats as something which requires housing and more, has specific housing requirements, moves them in relation to human from the horror of the underworld into a place of mutual benefit. "Confronted at the outset with the necessity of ascertaining whether the conditions selected – the caging, laboratory environment, consistency of the food and the mode of feeding, etc. – were endurable for the animals under any system of feeding, we undertook control trials with a mixed food in the form of dog biscuit and lard" (Osborne 1911: 13).

The image of the caging system presents many familiar aspects to people who are used to keeping animals.⁵² There is the automated feeding tube, and the separation and containment of urine and feces from the living environment and the food. Cages are made of wire mesh so that light can get in. These are not the rats, then, which are capable of persisting in the dark of the basement but animals which must be able to see out, which

⁵² The last figure in the reference work, "Breeding and Care of Albino rats for Research Purposes" is of the Wistar Institute itself. The photo is even redolent of the scientific illustrations insofar as it notes the materials used in the construction of the building and notes its situation as "isolated from all other buildings" (Greenman 1923). Interestingly, however, this photo is not listed as a figure of the reference work.

might just have or cultivate an interest in the human activity going on around them, and whose 'quality of life' is enhanced by a little light, a little outside time. "By a more intimate acquaintance with the habits of this little animal and a study of the means of making it contented and happy, apparatus and methods were devised for developing a colony which, while far from perfect, is very much better than the colony of the older methods" (Greenman 1923: 3). Even the fact that these animals which had been and were typically free to roam, a characteristic which contributed to reactions of horror, is turned into an 'exercise regime' and so made not only familiar but cute, as rats took to their spinning cages in apparently contented attempts to go nowhere, rather than to insinuate themselves everywhere.

Rat hygiene also emerged as a concern for laboratory science. "Intercurrent parasitic diseases, incipient senility, hereditary defects, and other incidental features may be present or arise to interfere with the normal progress of an experiment" (Osborne 1911: 14). Hygiene is certainly a powerful force in larger social and cultural circles at the time in both genetics and in social attempts to clean up the slums. "These diseases, however, have made less inroad upon our carefully selected and cared for experimental animals than upon the animals of our stock colony. We believe that by the application of further hygienic precaution the disease factor can be largely eliminated" (Osborne 1912: 234). Accounts made of the work undertaken at the Wistar Institute are of "the various reforms of colony management" (Greenman 1923: 3). This work with rats, therefore, is a force by which proof is secured of the good effect of these systems of hygiene, whether genetic or domestic. "It became more and more evident that clean, healthy, albino rats were essential for accurate research and that their production was a serious, difficult and worth-while task" (Greenman 1923: 3).

The language used by the scientists conveys a sense of urgency to know and understand the rat for the sake of progress in understanding rat physiology. Until this time, we are told, there has been a “lack of appreciation of the salient features of [the rats’ growth] curves” (Osborne 1911: 15). By 1911 it was incumbent upon scientists to ‘appreciate’ the lives of rats. In 1907, McCollum’s superiors expressed horror at the thought of rats in the laboratory because rats were the scourge of civilization and progress. Ironically, by 1911, for scientists to express a horror of rats, was effectively to turn their back on the progress of science and civilization.

Technically Animals

Scientists’ need for the rat as a part of the technical apparatus, that is, as a known entity of their experimental systems, drove the standardization of rats. Technical objects are those components of experimental systems that “perform according to regularities” (Rheinberger 1992: 310). These are conditions that are thoroughly established, known and reliable. Technical conditions include such things as standard systems and means of measurement, or standard recording devices. The technical conditions of an experimental system are those ideally “interchangeable parts” (Shapiro, 2002: 452). For example, in the discussions surrounding the development of the standard rat, biologists compared their need to the chemists’ need for standardized chemical compounds with which to work. If a chemist needs ammonia, for example, it will make no difference whether she takes it from flask A or B. Technical conditions are those laboratory conditions which have been built up and solidified over time. These are the conditions of the laboratory which, as Ian Hacking says, have “come of age” (Hacking 1992: 33). Conditions of relative age within experimental systems are technical conditions. Uniform animals are required so that any individual differences between rats can be understood as results of the experiment itself.

Just as work went into developing what would be the standards for the standard screw, the same kind of work went into developing the biological traits of what would be the standard rat. The standard rat is standardized as strikingly different from its wild cousin. As McCollum learned, creating lab animals required more than simply caging wild rats and calling them lab rats.⁵³

As compared to a wild or even a first generation captive Norway rat, an albino rat is less vicious, less fearful of people, and less excitable and intent on escape. He or she also has lost the 'threat posture,' and fighting is reduced to playful behavior; mice killing is much reduced as is cautiousness in response to novelty and vocal communication. In general, the albino is more quiet and tractable (Shapiro, 2002: 441).

Within science studies it has been argued that the scientific practice of using increasingly standardized rats as efficacious models for human beings calls into question the boundaries between human and animal, and natural and technical objects (Rader, 2002: 390). For example, Robert Kohler considers newly introduced lab animals to be a fruitful site for analyzing the bio-technical character of lab animals, and the commensal relations which develop between animals and humans (Kohler 1994). However, his analysis does not sustain itself and breaks down into considerations of ways in which the lab is 'like' a natural environment and ways in which a natural environment is 'like' a lab (so that I should put 'natural' also in single quotes). Such an approach does little more than call attention to the border between the natural and the technological, indeed appeals to its very effectiveness. Nature is worthy of attention in this scenario only insofar as it is a product of man's (chattering) imagination. When even nature is constructed in discourse, there is a re-stating of the "classical definition of the human as the rational animal ... the human as the

⁵³ Here again we see the role of myth and fiction in the development of science and scientific practice. In processes by which the rat was domesticated for lab research, or 'laboratorized', when albino rats were selected for this process, the process accelerated. That is, the albino rat provided an image of the rat which breeders could strive to achieve.

chattering animal. Only the animal is bracketed: the human as the chattering of culture” (Massumi, 2002: 38). Within this profound solipsism, nature is either inherent in culture, or is deemed unworthy of attention by critical social theorists.⁵⁴ “It is meaningless to interrogate the relation of the human to the nonhuman if the nonhuman is only a construct of human culture, or inertness” (Massumi, 2002: 39).

Perhaps as an aside, but perhaps to suggest more fully the solipsism of this stance, I might point out that despite Kohler’s alleged interest in *Drosophila* as experimental animals in genetics research, his accounts invariably slip into stories of the men acquiring or caring for the flies (Kohler 1993).⁵⁵ Throughout the history of western thought the category of animal has informed hierarchies of unequal power relations. Without demonizing Kohler specifically, or even his work as a particularly virulent example, this slip in his account is an example of the way this boundary functions to glorify the traits of the human as against the animal.

Early science studies’ accounts of animals in laboratory practice, such as those undertaken by Latour, designate animals in the lab as ‘boundary objects’. The lab rat, as a boundary object, is understood as an object which occupies a place in several social worlds and maintains its coherence as an object in each of these. Such an insight is useful in creating “counterhegemonic possibilities of plurality and contradiction” (Livingston 2006: 110). However, it has been noted more recently that such an account of the work that the animals

⁵⁴ Such a solipsism points to the necessarily open nature of scientific objects and gives the lie to the conclusion by authors who would rely on this solipsism that scientific objects are only ever possible because they derive only from the minds of men. That is, it acknowledges, without meaning to, that the scientific object ultimately is a surprise.

⁵⁵ Much sociological and historical work concerning the rise of the rat and mice into experimental laboratory practice renders an account of the work undertaken by politicians and policy-makers and accounts for the changes in disciplinarity and methodology which necessitated and made possible by the use of the rat in experimental practice. Such accounts can be frustrating insofar as they purport in their titles to be about animals and then are solely about human animals (Fujimara 1996; Kimmelman 1992; Kohler 1993; Kohler 1999; Radar 2002; Radar 2004). At any rate, these accounts are beyond the scope of our purpose here.

perform within the lab is merely an account of displaced human labour. "To become a social scientist is to realize that the inner properties of objects do not count, that they are mere receptacles for human thought" (Latour 1993: 52). Such an account falls prey to criticism reminiscent of the criticism of accounts in which animals become 'like' technologies. That is, it grants to human language a sovereign power in mastering the contingencies of the world (Wolfe, 2003: 43) and it pays too little attention to the materiality of the experiment itself and the rat (as something other than human designate) as an aspect of that materiality. Finally, in this account, the lab animal as boundary object becomes the end point of analysis and the contributions social theorists can make to science studies then become itemizations of boundary objects in the lab.

It has been thoroughly established by science studies since the 1970s that science is not a practice whereby a researcher lays bare nature to reveal her facts. Knowledge is more complex than a simple process of representation. To be taken seriously as a science studies scholar now, one must accept, at least in writing, that "there is no real world 'nature' that we can know apart from our 'technical-natural' tools" (Fujimara 1996: 13). Knowledge production processes are undertaken with meaning-laden and meaning-generating tools. Such an insight is correct, but given it, we must continue to think processes of knowledge production. It is as though science studies scholars point to processes of knowledge production as contaminated forms of knowledge production in the enlightenment model and so deem them unworthy of further attention. It is insufficient merely to point to the ways that natural objects are constituted with technical objects. Mine is not an attempt to account for the components of the experiments that are 'natural' and those that are 'social' and to diagnose the social and scientific forces determining them as such (Latour 1993: 53).

We must investigate the workings of experimental systems as heterogeneous assemblages which are inventive and which give rise to new concepts and new scientific objects. I am not concerned with the top down cutting out of the world by language, but with considering the productivity of the performance of the experiments. I want to investigate the experiments as “weaver[s] of morphisms” (Latour 1993: 137). In this process, we will lose the human subject (indeed, I will argue that this is precisely the productive point of the experiments) but we gain a newly enriched ‘form of humanity’ (Latour 1993: 138). This is not to suggest, with ANT, that the relationship between ‘structure’ and ‘agency’ is a “difference that is at the same time a form of identity”, but that the relationships within experimental systems are differentially productive precisely insofar as identity is suspended (Law 1999: 5). In my examination of the experimental systems we will see that just as we needed to conceive of the simultaneous co-existence of time frames with different durations, we must lose hold of a tendency to naturalize “a single spatialized form, a single topology” (Law 1999: 7).

Another position concerning animal experiments from which I would wish to distance myself is a normative stance concerning the treatment of animals within these experiments. The aspect of this argument from which I wish to distance myself is not concern for the ethical treatment of animals, but a normative judgment concerning animals in experimental systems as fundamentally disrespectful of their animal natures. Specifically, this position decries the ‘deanimalization’ of lab animals through processes of standardization. That is, it decries the imposition of human culture on the rat, but it is only by this same move that the rat can be conceived of as ‘naturally’ dumb and inert. “[T]hings-in-themselves are a convenient fiction, a way language distinguishes itself as intriguing and intelligent by caricaturing its Other as dumb and inert” (Livingston, 2006: 37). The

romanticism of the position that there exists some pre-cultural natural rat *enacts* the rat as dumb and inert. Clearly this normative judgment arises from a presupposition of the nature/technical object division and points to why social theorists of science must re-think the standardized animal as a 'hybrid' of nature and culture. As I suggested in the Introduction, writing social relations as the exclusive determinants of the development of science's objects (experimental or technical), is to write nature as the raw material of culture and to ask for the same nature that objectivist accounts would ask. That is, to write nature as the raw material of culture is to ask that there exist a 'reality' free from the effects of narrative, culture and social relations.

I suggest that it is precisely because of the animal aspect of their lab animals that scientists continue to use them. It is not that the animals become so much 'like' technical objects that they lose everything that is animal about them. If animals had indeed been de-animalized through the process, scientists would likely do away with them as part of the experimental system (why maintain animals which require such constant care, if your work doesn't require anything specific from them). We must therefore develop some novel manner with which to conceptualize the emergence of new knowledge and new objects in animal feeding experiments.

Experimental Systems: Producing the New

When considering the relations that constitute an experimental system, social studies of science have concentrated on the development of the system's technical objects and the ways in which the system contains aspects of the social world which are already existent and already known (Callon 1999; Latour 1996). While not wanting to deny the significance of those aspects of experimental systems or the work that has produced those accounts, I want to consider a different function of scientific experimental systems. Experimental systems are

not established merely to reproduce the world; they are established to produce encounters with as yet unknown aspects of the world and thereby to produce new knowledge. That is, experimental systems, when established as such, function to elicit the unknown, or to allow researchers to plunge into the unknown. Again, experimental systems are not always understood this way in the history, sociology and philosophy of science. "Traditionally in philosophy of science, experiments are seen as singular instances designed and performed in order to corroborate or to refute theories" (Rheinberger 1992: 309). However, as Fleck points out, if a research system was so designed, "it would be altogether unnecessary to perform it" (Fleck 1979: 309).

Technology and technical conditions are necessary but not sufficient for successful scientific experiments. The distinction is that technical conditions are material conditions by which the known can be produced and reproduced, while experimental systems are modes of thinking which are capable of "coping with and producing the new" (Grosz 1999: 40). Scientific experiments are "richer than the identity function on the basis of which it has been designed" and it is with the differential reproduction of experimental systems that I am concerned here (Rheinberger 1992: 312).

This insistence on the material production of new objects as significant to science can be considered as an extension of Canguilhem's insistence that the history of science account for the emergence of new scientific concepts. According to Canguilhem, a new concept can be said to have emerged in the biological sciences when a concept allows different work to be done within biological theory (Canguilhem 2000). A new concept makes new connections within the theory to which it applies and between that theory and other theories in the biological or any other science. At the time of the nutritional theory under consideration here, connections within the theory cannot be made or cannot be completed,

and as a result possibilities for new connections opened within the discipline. New connections had not been made yet, but there was the potential for new connections. If we understand a connection to have an effect, then this is the *virtual* effect of the world, its *capacity* for connection. That which is as yet unknown exists in this “realm of potential” (Massumi 2002: 30). If the unknown is understood as containing various potentials, then new thought requires harnessing the power of this zone of incipience.

Zones of Potential and Affect

If this zone of incipience can be conceived of as a bubbling cauldron of potential, I think we can conceive it, just as effectively for our purposes here as a crowded swarm of rats, bumping and jumping and scurrying (Massumi 2002). The lab rat as technical condition enables more than one scientist now to crowd around McCollum’s basement door and to stare down into that which invoked so much horror for his administrators, the unknown. Every actual rat the scientists put to service has an underside, which joins the swarm. Indeed, not only do the scientists add to the swarm, in their experimental practice they provoke it. Just as the sudden appearance of a swarm of rats in a horror film is meant to make us jump, when the rats make their move, it is going to surprise.

Bergson argues that this field of incipience has an affective force, that is, it appeals to the bodily potential for connection within knowers in a changing and problematic environment. Because of the body’s range of potential physical action there is opened in affective states a range of reactions that are “merely nascent” (Bergson 1988: 30). The solicitation of the future is signaled by intensity, what Bergson, Deleuze and Massumi call affect. Ludwick Fleck, in his Genesis and Development of a Scientific Fact, argues that scientific observation and knowledge production can be characterized as chaotic. “[C]haotically styled observation resembles a chaos of feeling: amazement, a searching for

similarities, trial by experiment, retraction as well as hope and disappointment. Feeling, will and intellect all function together as an indivisible unit” (Fleck 1979: 94). Such an indivisible unit of affect, power, desire, solicitation, modes of being and intellect is not simple knowledge but an experience; “the necessity of being experienced introduces into knowledge an irrational element, which cannot be logically justified” (Fleck 1979: 96-97). It is with this sense, this intensity that is felt but beyond experience situated in a narrative sequence, that the body participates in this realm of incipient potential functions. This is the experience of emergence.

This zone of intensity is inhabitable when the divisions between self and the environment are suspended. I want to see what happens when the scientists ‘take on animality’ in their experiments as a possible “mode of thinking the animal other” (Wolfe 2003: xv). Rats in mazes cycle through their senses in order of primacy to turn what would otherwise be a trap into the route out of the maze. They stand up on their hind legs to try to get a better view; they sniff along the floor and scratch at all the corners. So too do the scientists cycle through the sense receptors within the experimental stations, now weighing the rats, now feeling their coats, now checking their eyes, their digestion, their overall well-being. In pursuit of the unknown, their persistent and constant measurement of the rats’ lives as a way in which to think and imagine the animal, is less the work of representing the rat’s metabolism than a means of “knowing bodies and relating to bodies that is performative” (Hickey-Moody 2006: 193). We will see that the scientists even kept the clock ticking as they learned this maze; the measurement of time is crucial for the scientists constructing and learning the maze of rat metabolism. We can understand the experimental systems therefore to effect “an education of the senses” (Bergson 1988: 49).

Within the account of the experimental systems, which I render here, the scientist is no longer the controlling agent. If it is from this swarm of the virtual connections within the as yet unknown that new ideas emerge, it is easy to move away from the idea of a powerful all knowing human subject as the principle for the emergence of new objects. I want to explore the materiality of the experimental systems while taking into consideration, as part of the experimental system, the production of this excess, that which is present but is beyond knowledge, the as yet unknown. I suggest that new knowledge is emergent with the dissolution of the scientist subject into these affective states.

Building and Learning a Maze

When faced with the question of the unknown, the scientists have only that which is known with which to begin. For example, in his 1915 article, "The Nature of the Dietary Deficiency of Rice" in the Journal of Biological Chemistry, E.V. McCollum presented his findings in a series of growth measurement charts with a brief explanation of the diet used and the significance of the chart for the constitution of the diet. Concerning the "relation between polished rice and purified foodstuffs" he reported,

These curves illustrate the fact that polished rice cannot be supplemented so as to induce growth or prolonged maintenance by the addition of a suitable salt mixture alone. That failure of maintenance was not due to the low protein content of the ration, nor to the presence of proteins of a poor character in rice, is proven by the curves of Lot 381, Chart 22, which grew fairly well on a ration, the protein of which was derived solely from polished rice and was appreciably lower than in the ration here employed (McCollum 1915: 195).

In his various diet formulations McCollum makes his way through the maze that is rat metabolism. These various diets are a means by which to go along with rat metabolism. The scientists, McCollum here but also Osborne and Mendel, create the lines of rat metabolism along which they will follow by instigating blockages with the components of rat nutrition. Through performing this blocking the scientists have different aspects of 'normal nutrition'

stand out of the way. McCollum can thereby enter into and perform, as it were, the rats' metabolism.

To speak of observation and representation is insufficient here insofar as McCollum was not hanging back, making a map of rat metabolism (Livingston 2006: 14). McCollum does not have thoughts that are of the rats' metabolism. Rather, these blocks of rat metabolism are his thoughts. The point about learning rat metabolism for McCollum is that he was going in blind. He did not have the chemical markers he would need to 'see' the path of the foodstuffs. And yet, this blindness did not separate him from the world, render it inaccessible to him. Rather, it is because of this blindness that McCollum is engaged with and by the world in the way that he is.

The boundary between the subject and the environment is dissolved and McCollum can inhabit the field of potential connections by feeling his way along the walls of rat metabolism. In another example, McCollum makes a path for himself through the rats' metabolism, this time through mineral content of the diet and the metabolism of minerals by rats:

In this ration the mineral content was adjusted by salt and free mineral acid additions so as to approximate closely the mineral content of polished rice. The excellent growth curves make it clear that for growth the mineral content cannot be solely responsible for the failure of animals to grow on a diet of polished rice (McCollum 1915: 196).

First salt mixtures do not provide a point of exit; then neither does quality of protein. Having established these points in the maze, McCollum moves over to build for himself the walls of the maze of rat metabolism of minerals.

Nutritional science is deployed as a refrain in the development of the rations fed to the rats. Within these experiments, the content of nutritional science at the time becomes a refrain, 'the bulk of a song', which is repeated variously and if we wanted to pursue the song

analogy, we could see the refrains as occurring in a round. The crescendo is approached through a repetition in difference. These refrains build until there is a break, both in the wave that has been building in the accumulation of refrains and a break in the wall of rat metabolism through which McCollum can move. This particular study by McCollum concludes with a total of 42 charts, each with various aspects of nutritional knowledge blocked. These differences between the repeated experiments are sometimes very small, sometimes only a single change in procedure. For example, the difference between the additions of water extracted wheat embryo to a rice diet compared to the addition of acetone extracted wheat embryo to a rice diet. However, because of the repetition of the experiment, these differences accumulate along these lines of variation and eventually precipitate the emergence of new knowledge. In Chart 3 McCollum can draw a conclusion concerning the character of the deficiency of rice.

The above curves show that polished rice is not supplemented so as to induce growth or prolonged well-being by correcting the mineral content and the addition of 5 per cent of ash-free egg albumin. It is evident from these curves and Charts 1 and 2 that the deficiency of rice rests in something other than the inorganic or protein factor (McCollum 1915: 197).

As the scientists tell it in the scientific journals, these refrains are autopoeitic and auto-catalytic, that is, self-sustaining and differential in their reproduction. Consistently in these lab reports the scientists make statements which indicate that the current experiment was emergent in the 'real-time extension' of previous experiments. The experiments were inspired by and grew out of an experiment undertaken earlier by these same scientists, or by other scientists, or the experiments are a graft onto the results of someone else's previous experiments. Through the development and continued use of particular rations, experimental systems feedback on themselves to continue growing through time, and they

spawn new studies with similar but different character traits, which also feedback on themselves, continue growing.

In former papers from the laboratory we have made clear the nature of the dietary deficiencies of the corn kernel and wheat kernel as the sole source of nutriment for growing animals. In the present communication we present experimental data showing the specific properties of polished and of unpolished rice as a food, and show the supplementary relationship between these and certain purified and naturally occurring foodstuffs (McCollum 1915: 181).

If these experimental systems are self-organizing, perhaps, in addition to understanding them as a maze of rat metabolism, they can also be understood fruitfully as a juggling act. The scientists have several experiments on the go, several balls in the air, the fall of which they must wait through. They must move themselves and conduct their act according to the rise and fall of the rats' weight, health and well-being as they emerge in time and each experiment is launched in relation to the development of the others in the same way that a juggler launches each ball in response to the emergence of the whole juggling act. The scientists begin the juggling act of the feeding experiments to see what is required to keep the balls in the air. They juggle to see what precipitates the fall. In this metaphor but also in the experiments themselves we can see the experiments as a "rhythmic engagement with chaos" (Livingston 2006: 91).

Repetition: Precipitating the Future

Each of the experiments considered here presents data that emerged from feeding experiments which consisted of repetitious feeding and measurement practices. I want to argue for the force of the repetition and the rhythm of the measurements as constitutive of imaginative thought and of knowledge. The work presented in the article, "The Nature of the Dietary Deficiencies of Rice" is perhaps particularly well suited to highlight my points concerning repetition and rhythm. This study is huge and enormously repetitious. McCollum

and Davis try variation upon variation upon variation of diet. The sections of this article are, "I. The Supplementary Relationship between Polished Rice and Purified Foodstuffs", "II. The Supplementary Relationship between Polished Rice and Certain Naturally Occurring Foodstuffs", "III. The Supplementary Relationship between Certain Extracts of Naturally Occurring Foodstuffs and Polished Rice" (McCollum 1915). We hear of results obtained from Lot 308, Lot 313, Lot 316, Lot 334, Lot 317, Lot 329, Lot 340, Lot 309, Lot 382, Lot 351, Lot 355, Lot 326, Lot 383 and Lot 324. This is work in which a scientist could really lose himself.

Observations in the rat feeding experiments are taken and recorded frequently. The records of observations read like a Gertrude Stein story, "Lena was a brown and pleasant creature, brown as blonde races often have them brown, brown, not with the yellow or the red or the chocolate brown of sun burned countries, but brown with the clear color laid flat on the light toned skin beneath, the plain, spare brown that makes it right to have been made with hazel eyes, and not too abundant straight, brown hair, hair that only later depends itself into brown from the straw yellow of a German childhood" (Stein 1992: 172). The insistence of the observations belies any hope of signification. A rose, is a rose, is a rose. There is rather a swarm of roses, a swarm of rats, a swarm of potential and possibilities. The scientist as subject does not head-up the action here because there is no master to this narrative. Rather the sequence becomes a logic of affective sense. The insistence of the virtual possibilities in the presentation of data does as much to explore the limits of reason as it does to advance the logic of the research process. As with Stein's writing, the repetition enables a worrying and an exploration of the limits of reason. With such repetition there are no causal relations, no growth in a subject. The repetition functions in a manner similar to

critique in the social sciences. That is, it enables the scientists to wonder at and to provoke that which is outside the limits of our understanding.

Qualitative differences accumulate through repetition against the different blockages put in place by the scientists to learn their way through the maze of rat metabolism. Irruption of the new occurs, therefore, upon a substrate of repetition. There are differences that accumulate within the experiments and differences that accumulate between the experiments. From this constant repetition, this refrain of difference emerges the capacity for the precipitation of the future. The repetition against the unknown precipitates an overflow. And indeed, ultimately, in this experiment the result of all this difference in repetition “force[s] us to accept the conclusion that *there are necessary for normal nutrition during growth two classes of unknown accessory substances, one soluble in fats and accompanying these in the process of isolation of fats from certain foodstuffs, and the other soluble in water, but apparently not in fats*” (McCollum 1915: 184; italics in text).

Rhythm: Rats in their Wheels and the Dissolution of Agencies

Rat feeding experiments are constituted, in key respects, by the embedding of various durations of different aspects of the material assemblage. The experimental systems take on a rhythm or several rhythms of their own which lock together but which are distinct from the various durations that are embedded within the systems. For example, within these huge and repetitious experiments, observations are made and recorded at specific and rhythmic intervals. Osborne and Mendel provide the most detailed record of the repetition required of the animal experiments in their first Carnegie Institution publication.

Rat XII and rat XIII were caged separately on August 9 1909. Fresh food-paste was introduced daily into the food dishes in excess of the amount eaten, which was at first ascertained daily. The body-weights were at first determined every other day, as was the nitrogen of the excreta (urine and faeces). Subsequently it was found adequate to estimate the nitrogen in weekly periods (Osborne 1911: 19).

The rhythm of these observations is a rhythm of the experimental system that is distinct from any of the various time-frames found within the experiments, such as the rate of growth or metabolism in the rats or the teaching schedules of the researchers. According to Hans Ulrich Gumbrecht, rhythm can be considered a materiality with particular functions in human imagination and thought. One function of rhythm is 'the coordinating function', whereby rhythm facilitates the coordination of bodily movements of different individuals such that it "allows them, metaphorically speaking to become a 'collective subject'" (Gumbrecht 1994: 172). These embedded time frames and durations establish differentials of time and speed within the experiment. These new differentials are the boundaries of a system, which is out of phase with the formed entities such as the rats, the scientists, the mazes and the subject of biochemistry at the time, which went in to producing the system. The rhythms of the experimental systems function to lock together the various time-frames within the experimental systems such that the time of the experimental system begins to function as a whole. The rhythms within the experimental systems enable the emergence of a collective subject, one which is in encounter with a changing and problematic environment. The rhythmic observations of the experimental system function to create a blur of nature and culture, a blur of scientific knowledge and animal metabolism, which is productive of the intensity of the 'as yet unknown'.

Another such function by which the experimental systems are coordinated as a whole and by which a collective subject emerges is the rhythm of the rats' exercise wheel. The rats' exercise wheel is a point so ever-present in this function as to be practically invisible. The rats' exercise wheel is precisely the point at which to see that rhythm allows for the undoing of the human and material agencies constituting the system and that insofar as it performs this function, this dissolution of human and material agencies, this rhythm allows

the system to function as a whole with internal time-frames and durations and thereby enables it to function as a productive experimental system.

The exercising cage is an essential part of the colony equipment if fertility is to be maintained and vigorous rats are desired.

The cage which we have found very satisfactory is constructed upon a 21 inch bicycle wheel. The excellent ball bearings of a bicycle wheel are essential, for revolving cages are subjected to a very considerable daily use. The recording mechanism frequently registers 5000 revolutions in the twenty four hour period (Greenman 1923: 24).

Close consideration of the revolving wheel allows us to consider the experimental system as a system driven by its own differential reproduction, rather than simply the work of scientists upon rats. First of all, the rats' agency is apparent in the need for the flywheel. Rat fertility and well-being are sufficiently touchy as to act as agents in the experimental system; the successful experimental system requires the flywheel to maintain itself. The 'constellating' of the rat as a technical object here relies upon the rats' own material agency. However, the fly wheel does not only highlight the rats' agency, but highlights the dissolution of the significance of any one agency and the importance of that which develops in the refrain of human and material agencies. The fly wheel is the perfect site at which to see that the experimental system, and not only the human aspect of the experiment, opens up to become a performance of the rats' metabolism. The interests of each party are so caught up in the wheel that it is impossible to say where the scientists' interest and knowledge begins and where the rats' interest and metabolism begins.

According to Gumbrecht, rhythm is significant insofar as it is a phenomenon "without a primary representation dimension" (Gumbrecht 1994: 171). I argue that if, as Gumbrecht asserts, rhythm is a phenomenon without a representative dimension, it is perfectly suited to carrying, throughout the experimental system, the unknown. The rhythm of the exercise wheel is fundamental to the development of the much-desired 'fertile and

vigorous rats', which are essential to any successful experiment and is therefore bound intimately with the scientists' desire. This is the rhythm of that of which their desire is in pursuit, that which they do not know, yet. The scientists' desire takes on the surge of the cycle of the rats' wheel and in this way the resonance of the rhythm within the experimental system builds and grows. The rhythm of the wheel, we might say, establishes and perpetuates the wave of the coming to know, throughout the whole experimental system. In establishing this rhythm with the revolving cage, the as yet unknown, the scientists' desire, pervades the rat colony and the experimental system in waves. The rhythm of the revolving wheel therefore is an essential material if the experimental systems are to solicit the future.

Gumbrecht characterizes rhythm as a phenomenon which is experienced as bodily movements. Narrative descriptions, on the other hand, that which is known and can be represented, are phenomena that are experienced as meaning (Gumbrecht 1994: 181). Gumbrecht argues that imagination appears in the tension that is created in movement back and forth between these two levels. This is to say that repeated bodily experiences of rhythm will pull into tension the level of signification available to those involved. The experience of the bodily movement of rhythm is at least not reducible to the signification of any semantic description involved. This is not to say that entire structures of signification are brought down, but that in the tension between the semantics of representation and the physical experience of rhythm, bodily movement can access significance which is not part of a narrative sequence. As Gumbrecht understands it, rhythm builds and grows into imagination.

Knowledge does not emerge solely from the sense the scientists make of the data they collect for themselves, but emerges from the scientists losing themselves, as a necessary effect of the huge task of rhythmic data collection, and from the interlocking of the various

durations and rhythms by the rhythmic drive of the revolving cage. “Every purposive movement, when it catches on, loses sight of its teleology and continues as a periodicity with a force that is not the force of the will launching it and launching it once again and then once again; instead it continues as a force of inner intensity” (Lingis 1998: 61). So it may very well be that the scientists placed the rats on the wheels to the end of maintaining the health of their subjects. However, very quickly, as is apparent to anyone who has ever watched a rodent in a wheel and dissolved in laughter at its absurdity, the logic of the process is carried away by the momentum. The absurdity marks the extremity of the self-reference that the revolving wheel performs in the experimental systems and just as logic is carried away, so intentionality is pulled into the orbit of absurdity; a rose is a rose is a rose (Livingston 2006: 64).

Just as the rhythm of a handyman’s hammer pervades a neighbourhood’s summer afternoon (Lingis 1998: 61), the rhythm of the revolving wheel pervades the experimental system. And, just as the silence of the handyman’s hammer brings into sharp relief the rhythmic tapping of the neighbourhood Blue Jay (Lingis 1998: 61), the rhythm of rat metabolism is brought into sharp relief when the revolving wheel falls silent. Perhaps one effect of the pervasive rhythm of the exercise wheel is the possibility for the experimental system to register the caught fall of growth, in the sudden silence of an exercise wheel, at the end of the day, or whenever the lab tech goes home. If the experimental system consists of a juggling act of purified diets, here we can see that the real trick is juggling while maintaining the balance of the experiment on a unicycle. Certainly we can see the establishment of a closed system through the traffic in differential at either outer limit of the balls in the air and the revolving wheel and the “repercussions [of] the little bobbles of the balls and the wobbles of the unicycle” (Livingston 2006: 84).

Touch and Texture, Sight and Vitality

As I have just suggested, central events in the assemblage of the experiment are observations by the scientists. Observation in a scientific experiment means hosting different and various sensations, which demand the opening and closing of various points of entry and exit and exposing various surfaces to the as yet unknown. "There is no universally accepted system of measurement in biology" and within the feeding experiments measurements are also assessments of the animals' 'vitality' and 'well-being' (Fleck 1979: 63).

At the time of the formulation of the problem of the 'as yet unknown' food factor, the chemical constituents of the accessory food factor(s) had not been isolated. Stanley Becker has argued that because Osborne and Mendel did not recognize the significance of such an insignificant amount of chemical they were unable to "ascertain the presence" of vitamins in their rats' food. The significance of this observation is two-fold. Osborne and Mendel could not conceive the importance of such an insignificant quantity of chemical to nutrition, and further, they did not have the technology to isolate the substance, even if they could conceive of the need to do so. Osborne and Mendel could only re-construct the pathways of cellular metabolism that were detectable at the time with the tools at hand. The only test for the presence or absence of the unknown food factor was a biological test, or an animal feeding experiment. These are the constitutive constraints within which Osborne and Mendel articulated the problem of the unknown food factor and began to develop its concepts. "It is impossible to understand the theories, concepts and facts apart from the experimental systems used to bring them into being" (Fujimara 1996: 51). Observations of well-being and a standard of well-being were practical tools in the experimental system. Noting and recording the well-being of the lab rat was a method for containing and interpreting new results. Well-being therefore was such a powerful tool for biological

analysis because it enabled the conceptualization of the vitamins in a manner that mathematical or chemical analysis would not have allowed.

The animals' well-being and vitality, which cannot be measured other than through a 'sense' of the animal, became both a resource and a result within laboratory research. Significantly, in the publications of these experimental results, the discussions in the results sections are of coat texture and general appearance, activity level and well-being as it 'seems' to the scientists. The significance of the scientists' 'sense' of the experiments as a material aspect of the experimental systems and the emergence of new knowledge appears in their observations of the animals' general health and well-being. "The appearance of these rats was very miserable. They were rough coated and emaciated" (McCollum 1915: 183). The coat of the rat, therefore, no longer marks the boundary of the rat from the scientist, rather it is a zone of intensity within the rat/scientist hybrid, the blur of static that comes with rubbing a dry coat, which allows the scientist to form a line with the rats' interior. Such a statement recalls an image of a sorcerer stroking her cat, stroking her rat, as she ponders the future(s) she might devise. In rubbing the rats' coats the scientists blur the boundary of the animal body and encounter their interior and the functioning of the intermediate metabolism. The lines of both the subject and the object are redrawn, as scientists make observations concerning the rats' coats to describe the influence of an unknown chemical in the intermediate metabolism of the rat.

When sitting and rubbing their rats, the scientists are in a state of suspension. They are worrying the problem - suspended between points of knowledge and points in time, between what they have known and that which they do not know yet. In taking time to rub the rats, they break off from 'commonly accepted standards' of scientific activity to suspend

themselves over the abyss of the unknown, to feel that intensity on their skin. This state of suspension is the site of the power to disrupt the narrative line of nutritional knowledge.

Although we have this information presented to us as a result in publication, we must understand that at the time the scientists' repetitious contact with the rats (in feeding, measurement or stroking) really was an invocation of the future. The sense the scientists glean from stroking their rats is not only of rough-coat or soft-coat. They are interested in rough or soft fur only insofar as it bears with it the traces of the rats' metabolism. In petting the rats, the scientists encounter "fuzzy coats of fur [which] bear traces of entirely different matrices of space-time" (Livingston 2006: 172). The scientists were testing the future with these animals. "The mortality of the young was somewhat high, a fact for which we have as yet no adequate explanation" (McCollum 1915: 189). Things did not look good for the rats. "They failed to make any growth, and died within two months" (McCollum 1915: 182).

Graphs: Picturing the Problem

Other aspects of the results sections of these feeding experiments are compelling. Frequently in the publication of their rat feeding experiments, there are no arguments presented as we would normally conceive of them. Rather, a presentation of graphs and charts suffices as results, without a written explanation of their significance. Time and again in the journal articles with published results, there is a distinct lack of verbal explanation for the phenomena the scientists were witnessing in their laboratories. The arguments contained within the articles are not articulated, they are not presented verbally. Rather, significant results are presented in graphs.

I argue that the use of graphs for their results enabled the scientists to present something about which they could not yet speak. What they wanted to show did not yet belong to a narrative sequence and their observations could not be indexed to any

conventional meaning. And yet, clearly the scientists presented findings that were of significance, as their papers were published by scholarly journals. The presentation of data, therefore, is the presentation of images and sensations, not as meaning but, as a pulse of the world. "Chart III shows in graphic form the favorable influence exercised by different percentages of this type of fat. Not only was growth resumed in most cases at a very rapid rate but all evidence of malnutrition, especially the affection of the eyes, promptly disappeared" (Mendel 1914: 402). The function of the principle found in the fat can be seen in growth and a return to well-being and can be comprehended in graphic form. We must see here that what is pressing but what cannot yet be articulated is a part of the materiality of the experiment. This part of the world which can be sensed but not yet articulated, also works to shape that which is possible, or that for which the system contains the potential.

Another example of this graphic presentation of results is in "Observations on the Isolation of the Substance in Butter fat which exerts a Stimulating Influence on Growth" in the Journal of Biological Chemistry, October 1914. In this work, McCollum had separated a growth-promoting factor from butter fat and transferred it to olive oil, which does not naturally contain an accessory food factor. "We have now observed that the property of inducing a resumption of growth in rats which have grown as far as possible on a fat-free ration, can be conferred on olive oil by shaking the latter with a solution of the soaps prepared by completely saponifying butter fat in a non-aqueous system with potassium hydroxide, according to the method of Henriques" (McCollum 1914: 246). Here there is discussion concerning the growth achieved by the rats when fed with the olive oil, which had been enhanced with the saponified butter-fat. There is also an articulate discussion concerning the chemical contents of the food ration used. However, despite these articulate

discussions concerning the experiments, the results, the work of the accessory food factor is still presented in the form of a chart.

The accompanying charts indicate clearly the stimulating effect of the olive oil thus treated on growth as compared with the same oil without such treatment and seem to leave no room for doubt that the growth promoting substance was transferred from the butter fat to the olive oil by the procedure described (McCollum 1914: 247-248).

It is significant to note that the scientists do not calculate from these graphs. Their concern in developing the graphs is not with how the experiments add up, but with the differential, the 'more' that makes a curve – a learning curve. As results, the graphs show the unknown food factor to be growth-promoting. However, and this is my point, within the scientific community these graphs function to represent the outlines of the field, the shape of the problem, the potential of the unknown.

The presentation of the graphs has the effect of resonating with and increasing the amplitude of the affective intensity felt by the scientists that I have already discussed. This increase in intensity occurs on two levels at least. First, the presentation of graphic data in scientific journals will effectively spread the effect (of affect) throughout the scientific community. The graph works to say, "look, my friends, this is what we're up against". Second, although the graphs are presented as evidence, they were first used as tools in the research process and so the scientists would have spent their days encountering the rats and the rats' coats and would then encounter the graphs that would serve to amplify the intensity they experienced in their daily interactions with the rats. That is, the experimental system is set up such that regular ingress can be made into the system by that which exceeds the limitations of the system itself.

The Other Affect in Science: Friendship

I have argued to this point that the intertwining of human and material agencies in experimental systems establishes a zone of intensity, a field of possibility which functions to host the future and solicit the new. Here I want to maintain the argument that new knowledge emerges in a refrain of human and material agency, but I want to pursue this interaction through the actualization of the vitamins. That is, I want to track the interaction of the human and material agencies which effect the process by which knowledge and objects move from being incipient to being actual. According to Bergson, processes of actualization are processes of winnowing away potential from the realm of the virtual and I will consider an instance during which vitamins were actualized (in part) from 'as yet unknown' food factors. An exchange of correspondence between Thomas Osborne and Lafayette B. Mendel in the late spring and early summer of 1913 details nicely a refrain of human and material agency through which the vitamins become increasingly actualized. This exchange of correspondence occurred while Mendel was on summer vacation and is a particularly compelling site of investigation as the human agency here consists in the expressions of friendship between the two men.

The exchange follows the publication of McCollum's 'successful' rat feeding experiments three weeks before Osborne and Mendel published their 'successful' rat feeding experiments. McCollum's publication caused much excitement in the Connecticut laboratories and it is fortuitous for the historical record that Mendel was on vacation at the time because it meant that Osborne and he had to communicate by letter, thereby leaving a written record. McCollum's perceived insurgency into 'their' field mandated that Osborne and Mendel stop and take stock of the substance of their current experiment. What I want to show here is that a good deal of relationship maintenance was also undertaken and, without

knowing how 'necessary' the emotional work was, certainly the trust and intimacy in their friendship were contributing factors to the development of the scientists' work with the rats and the 'as yet unknown' food factors (Shapin 1994: 359). In these letters, both the friendship and the rat compel Mendel in his work, even while on vacation. In response to an initial letter from Osborne, Mendel writes:

You are not 'bothering' me when you write. I always like to hear from you. Nor am I making any serious 'change of thought' here: in fact I am putting in several hours a day trying to 'catch up' with the literature which I have neglected in the past few months. I find, however, that the problems in which I used to be interested – purine metabolism, parenteral absorption, lymph, etc. – all see 'tame' to me now when the center of my interests has so long been pivoted on our common problems of the rat (Becker 1968: 241).

Tame - more on the order of a pet mouse than a swarm of rats. That is, not nearly so intense, certainly not of the same affect. And what renders the problem so intense is the back and forth of the friendship ('our common problem') and rat physiology, the back and forth between human and material agency impresses Mendel as particularly intense, as compared to other aspects of research.

On July 9 1913, Osborne wrote to Mendel, still on vacation, to tell him he had read a piece by McCollum in the Journal of Biological Chemistry.

We have just received the July number of the Journal of Biological Chemistry and are greatly interested in McCollum's paper which I should suppose you had seen before it was printed. If not, I will say that he might just as well have supplied his data from our notebooks as from his. His results agree with ours in all respects and he has discovered that butter makes them grow (Becker 1968: 237).

In stating that McCollum has discovered that 'butter makes them grow', Osborne provides a peep into the process of actualization of a scientific object from the virtual. Osborne refers to data that Mendel and he have built together. Osborne says that McCollum has their data. He then goes on to say "*and* he has discovered that butter makes them grow". This clearly then is their result. McCollum's data correspond to their data and McCollum's discovery

corresponds to their discovery. This is the purpose of Osborne's letter to Mendel. It is not to inform Mendel of some discovery made by McCollum that is a surprise to Osborne. He wrote to Mendel out of frustration and disappointment, "It seems to me a pity that we should be in competition with McCollum in this line of work" (Becker 1968: 237). The point I want to make concerns what this letter and its tone tell us about the process of actualization.

The process of actualization involves these kinds of discussions among friends concerning butter and its effects on rat growth (in this case). The conclusion reached between them that butter makes rats grow is a grasping, a reach which grows up between human and material agency. It has occurred to (or, for?) them that butter makes the rats grow. This example shows nicely the obscurity of the process of actualization. "Butter makes them grow" – I don't know what that means, do you? Osborne and Mendel understood the implications of such a statement because they were friends. This, "butter makes them grow" was a scientific result only because the friendship at work could sustain it as such. Only because of the intimate relationship between Osborne and Mendel could this tenuous result, "butter makes them grow" be sustained in actuality as a result. It was because of the intimate bond between them that they could nurture this incipient result into actuality. The tendencies and potential of the virtual are being pulled through the refrain of friendship, lab bench relations and rat physiology. 'Butter makes them grow' no longer provokes a swarm. By naming these tendencies within the material-human relations, 'butter makes them grow', Osborne and Mendel begin the process of inhibiting other tendencies so that the accessory food factors are increasingly actualized.

Conclusion

I have argued in this chapter that while experimental systems are constituted in and through technical conditions, this does not exhaust their identity. Indeed, it is precisely the differential reproduction of the experimental systems that marks them as productive and vital experimental systems. The new scientific objects that emerge from experimental systems cannot be understood as merely all that was possible from the technical conditions. To conceive of experimental systems as uncovering the world is to spatialize time; everything is already existent, spread out through space. Experimental systems therefore, must have a temporal function; they must work to solicit the new and function to host the future. I have argued that experimental systems are open to emergence through various material systems and practices which effect a dissolution of the boundaries between the scientific subject and the scientific object so that a zone of incipience, of potential, is created. I have shown that it is through performance of the rats' metabolism, overwhelmingly repetitious tasks and the rhythm of the rats' exercise wheel that these experimental systems create this zone of intensity and so host the future and solicit the new.

Chapter Five

Disputes without Discovery

In this chapter I will review two disputes that took place between scientists, one in 1912 and the other in 1913. The first dispute concerns the formulation of the vitamin problem, as a problem. I argued in the second chapter that despite integrating the 'as yet unknown' factor into their experimental systems, Osborne and Mendel did not pursue the matter as a problem. I am not the first to notice this aspect of their publication. Following Osborne and Mendel's Carnegie Institution publication in 1912, Frederick Gowland Hopkins published an article dealing specifically with the 'as yet unknown' factor and suggested that, based on earlier experimental work he had undertaken (which I reviewed in Chapter One), Osborne and Mendel's 'as yet unknown' was in fact a fruitful site of problematization for bio-chemistry at the time. In making this suggestion, Hopkins engaged more deeply with the accessory food factors than he did in 1906 and opens, as a problem, the outline of his accessory food factors. Despite Hopkins' engagement with their work, Osborne and Mendel remained uninterested in the as yet unknown factors as a problem for investigation and stated as much in a publication that cited Hopkins' most recent work. Hopkins was quick to discipline Osborne and Mendel publicly because he took their lack of interest in the as yet unknown factor as a denial of the shape of the field of bio-chemistry at the time. Thus, I suggest that the virtual aspects of the world, in the form of problems and the experience of the intensity of transformation in affect contributed to the development of bio-chemistry at the time.

After being subjected to a public disciplining by Hopkins, Osborne and Mendel were quick to acknowledge to Hopkins the significance of the as yet unknown food factors as a problem in the field of bio-chemistry, and began to investigate the as yet unknown food factor in earnest. Later, in 1913, Osborne and Mendel were embroiled in another dispute

when, having finally developed their interest in the problem of the as yet unknown food factors, they were upset to find Elmer McCollum publishing results which were also their own, before they had. Whereas between Hopkins and Osborne and Mendel the contest concerned the terrain of the field, between McCollum and Osborne and Mendel there developed a contest within the terrain.

My interest in these encounters between scientists is not in adjudicating their arguments, but rather in examining the problematic and conceptual elaboration going on at the time. Indeed, an examination of these relations as part of the refinement of the problem and concept of the vitamins points to the impossibility, and indeed the undesirability, of adjudicating their arguments. I will review first the interactions between Hopkins and Osborne and Mendel throughout 1912 in order to show the problem space as negotiated terrain. My point in considering Osborne and Mendel's contest with Hopkins is not to consider Hopkins' formulation of the problem of the 'as yet unknown'. Rather, I am interested in the moves Hopkins makes that render the 'as yet unknown' as a problem and the reciprocal moves made by Osborne and Mendel to render the 'as yet unknown' as unproblematic. My point, especially in comparison to the argument that Osborne and Mendel have with McCollum is to argue that the scientists' sense of the field determines, in part, what constitutes the problems and objects of scientific investigation at the time.

After reviewing the year-long interaction between Osborne and Mendel in Connecticut and Hopkins in Cambridge, concerning the status of the 'as yet unknown' as a problem, I will consider the infamous 1913 contest between McCollum on the one hand and Osborne and Mendel on the other. I suggest that what is significant in the dispute is not the fact that the scientists argued, but the articulation of the problem of the organic nature of the unknown food factors. Here I will be concerned less with the social relations than with

the actual content of the problem as both Osborne and Mendel and McCollum formulated it. Having reviewed the establishment of the problem of the organic I will consider the philosophical significance of the “discovery” made by both McCollum and Osborne and Mendel.

Hopkins and his Non-Sense

If Osborne and Mendel were content to integrate an ‘as yet unknown’ factor into the technical conditions of their experimental system for the Carnegie Institution publication in 1912, upon reading their publication, Hopkins rather seized it and used their report as a jumping off point to further his accessory food factor research and conclusion from 1906. In 1912, in the Journal of Physiology, Hopkins published, “Feeding Experiments Illustrating the Importance of Accessory Factors in Normal Dietaries” in order to “put upon a more quantitative basis results which I obtained as far back as 1906-1907” (Hopkins 1912: 425). In this publication, Hopkins cited Funk’s 1912, Journal of State Medicine article as “extremely interesting recent work” undertaken since his own 1906 experiments, which realized the “fact that ... protein supply and energy supply do not alone secure normal nutrition” (Hopkins 1912: 425). Hopkins’ main focus, however, was Osborne and Mendel’s work. “In particular I may refer to the ... extensive researches of Osborne and Mendel upon rats” (Hopkins 1912: 425).

Hopkins introduced his article with a review of Osborne and Mendel’s work, stating that in their early work (part one of the Carnegie Institution report), they found “in particular that synthetic dietaries which were capable of maintaining the life of fullgrown rats⁵⁶ ... were quite unable, no matter what the protein, to maintain the growth processes in young animals” (Hopkins 1912: 426). Hopkins then reviewed Osborne and Mendel’s later

⁵⁶Of course the conclusion was reached that synthetic diets could maintain life in adults because the experiments were cut short by the lab fire.

findings (part two of the Carnegie Institution report), which stated “that the addition of milk products to the food promoted rapid growth in rats which had remained stationary in weight when on the original artificial dietary” (Hopkins 1912: 426). He reviewed the methods of preparation for the protein free milk used by Osborne and Mendel, noting its inorganic elements and its relative caloric contribution to the diet and concluded his review of their work with a smooth transition to his own; “In my experiments . . .” (Hopkins 1912: 426).

The transition to Hopkins’ own experimental methods is not even marked by a paragraph separation. Such close integration of the work of Osborne and Mendel with his own research methods and problem effectively renders Osborne and Mendel’s work and results as Hopkins’ own problem.⁵⁷ Hopkins’ point in the article is not to make a case for what the unknown substance in milk might be. But neither does he leave the field untouched, and merely assert his place in it. Hopkins’ move in this engagement with Osborne and Mendel’s work is to re-assert his claim that the factor is *unknown*, i.e., a problem in the field, not one of the already certain food substances. “In all cases the influence of milk upon growth was so large that it could not have been due to any alteration in the quality of the protein eaten or in its ratio, nor, in my own belief, to the presence of any known milk constituent” (Hopkins 1912: 427). Hopkins did make some suggestions about the as yet unknown factor(s). He suggested that it or they functioned in growth and that disease resulted from their absence from the diet. But these suggestions are not the contribution Hopkins’ article makes to the field at the time; such work had already been done – remember Funk. Rather, Hopkins’ contribution with this article is to present the as yet unknown as a potential site for research and investigation, as a site which might be elaborated, as a site of incipience in research.

⁵⁷ Throughout the article Hopkins made statements such as, “on comparison between Osborne and Mendel’s experiments and my own” (Hopkins 1912: 451).

In this article and in a correspondence exchange with Osborne and Mendel, Hopkins stated that his own research results indicated the as yet unknown as a problem.⁵⁸ In August 1912, Hopkins wrote to Osborne.

I trust that neither you nor Prof. Mendel will think that my paper which has just appeared in the *Journal of Physiology* ... intrudes unduly into your domain. As a matter of fact I have been engaged in such feeding expts. for a good many years, and the great pleasure with which I read your work as it appeared in the Carnegie Institute Publications was (human nature being what it is) somewhat spoilt by the fact that it takes the wind out of the sails of many scores of experiments of my own!

I suspect that I was led to concern myself with specific growth factors in diets for reasons very similar to those which influenced you. I began, in 1906, to feed rats with amino-acids mixtures in the endeavour to discover whether there were differences in their relative importance in metabolism. One soon found that growth could not be maintained on a mixture of all of them, though growth followed satisfactorily enough when small amounts of yeast-extract etc. were added. This led me to fractionate yeast extracts in an endeavour to get at the really active stuffs – a task which has occupied me at intervals for at least two or three years.

The problem is no easy one, and I think there is work there for many of us.

Thanking you for the gift of your papers ... (Becker 1968: 197).

Upon receiving this correspondence from Hopkins, which was the first Osborne had heard of the *Journal of Physiology* article, Osborne wrote to Mendel. Osborne spends the letter considering what he thinks of Hopkins' work and also what he thinks of his work with Mendel. In this way, Osborne's letter speaks to the way in which he was thinking of the field of bio-chemistry and its problems at the time.

F.G. Hopkins wrote me a pleasant note, as he presumably did to you. I have not yet seen the reprint which he states he has sent but I shall reply that we do not regard ourselves as the sole scientists entitled to feed rats (Becker 1968: 197).

⁵⁸ Hopkins and Osborne had a close professional relationship. They were clearly fond of each other and corresponded frequently, requesting the other's presence at conferences and exchanging letters of reference regarding students. Well beyond this initial publication furor, Hopkins can be found to apologize to Mendel for publishing elsewhere than in Mendel's *Journal of Biochemistry*.

By invoking rats in this manner, Osborne implies that if Hopkins has tread upon their pastures, they are hardly very bright or glorious pastures. If Osborne appears to disparage the experimental work as 'feeding rats', neither is he particularly excited about Hopkins' results. "Evidently he is impressed with the idea of a 'growth substance' or something of that sort, aside from the ordinary nutrients" (Becker 1968: 197). This idea, Osborne tells Mendel, is redolent of an idea presented in another paper by a scientist named Suzuki which, in his letter, Osborne reviewed for Mendel because he thought it unlikely that Mendel had seen it yet. Osborne's account of Suzuki's paper to Mendel appears to be an effort at a raised eyebrow regarding the entire state of the field at the moment. In his review of the article for Mendel, Osborne, for the most part, disparages the work, but ultimately concludes that it provides a cue for their work in Connecticut.

You probably have not yet seen the elaborate paper by Suzuki and Colleagues in the latest *Biochem. Ztscher.* on 'oryzanin' the constituent of rice polishings which cures pigeons and other animals depleted by feeding with polished rice. The evidence (!) lies in the use of alcoholic Extracts of these substances to cure pigeons or prolong the life of polished rice-fed animals. The conclusions are not convincing; but there are points involved which will give us a cue to new lines of work (Becker 1968: 197).

Mendel, in his response to Osborne, was not entirely convinced by the results of Hopkins' experiments either.

I am not so thoroly [sic] convinced yet that other combinations of pure, isolated, foodstuffs (with less fat) would not lead to considerable growth without milk or oryzanin. ... However, these 'accessory' extracts may be an invaluable aid to us in studying the comparative role of proteins if they permit us to keep our animals in better form (Becker 1968: 198).

Interestingly, rats are no longer the disparaged subjects of scientists who wish to 'feed rats' but are 'our animals'. 'Feeding rats' perhaps is the occupation of those in pursuit of a growth factor, 'our animals' are necessary and valued objects of study to determine the nutritive value of isolated proteins. If Osborne and Mendel were not ignoring the 'accessory

factor' situation, they certainly were not seeing it as the most profitable site of investigation. They did not see the as yet unknown as growth promoting – for either the field of biochemistry or for their rat subjects. Mendel did suggest, however, that they take up a criticism made by Hopkins and work toward better quantification of their protein free milk (p.f.m.). In this way, despite their best intentions, Osborne and Mendel entered into an investigation of 'accessory' factors. "The point in our paper which Hopkins questions is very important i.e. the quantity of p.f.m. that is sufficient, and it needs to be repeated perhaps" (Becker 1968: 198). However, it is important to bear in mind here that their attempts at better quantification of protein free milk were not similar to Funk's chemical investigations. Osborne and Mendel were not convinced that the as yet unknown factor was a problem. That is, they didn't consider it a point for fruitful investigation within the field of biochemistry at the time. Their efforts at quantification were made to this end, to retain the unproblematic character of the unknown factor in food. While an 'as yet unknown' factor was proving a problem to them as a technical condition of their animal feeding experiments, it was not yet presenting itself to them as a problem within their conceptual framework.

Putting the Gloves on a Straw Man

Following their decision to acquire better quantification of the protein free milk, Osborne and Mendel wrote a set of directions for research.

Investigate the effective substance in milk and pfm [protein-free milk]. ...
 Why is milk ash a failure, but artificial p.f.m. not?
 Dissect p.f.m. ... cf Suzuki. also Funk. ...
 Study the possibilities of very large growth, i.e. after 130 grams.
 Repeat some of Hopkins' experiments with his proportion and methods using p.f.m. in place of his salts. Also try his salts in place of our artificial p.f.m. Determine what is an insufficient quantity of milk for growth and use this small addition like Hopkins.
 Employ Suzuki's oryzanin and see whether our 80 day crucial period can be extended. ...
 Calculate efficiency of growth in some of our experiments. See Hopkins, p. 444, top, and p. 448 (Becker 1968: 203).

From the research carried out according to these plans, Osborne and Mendel published two articles, one in German and the second in English in the Journal of Biochemistry. Hopkins responded to these articles in a published "Note" in the Biochemical Journal. Despite their genial response to Hopkins' correspondence, and their attempts to solve the problem of quantification that Hopkins had brought to their attention, Hopkins was not generous in his response to these efforts. Although the German article is inaccessible to me, we do have their article "Maintenance Experiments with Isolated Proteins" from the same year in the Journal of Biochemistry and Hopkins' "Note" responds to both the German and the English articles.

In their "Maintenance Experiments with Isolated Proteins", Osborne and Mendel are concerned with sources of nitrogen in animal dietaries. That is, the focus of their attention remains upon the value of different isolated amino acids for rat growth, health and reproduction. Osborne and Mendel do speak to the possible existence of "food hormones", but do so once again only as a technical condition necessary to the "long continued" experiments necessary to attain certain results in the amino acid experiments and to see their amino acid experiments through to completion. Osborne and Mendel even cite the Hopkins' piece reviewed above in their consideration of the substances necessary to an artificial diet that will maintain the animals. However, immediately following their mention of Hopkins, Osborne and Mendel return to the problem of nutritive value of isolated proteins and their results are of the success and varied results achieved from specific amino acids. Once again, therefore, Osborne and Mendel integrate the as yet unknown factor as a technical condition, while continuing to concentrate on the problem of the qualitative value of different amino acids for nutrition. And it is with this focus that Hopkins takes issue in his "Note Concerning the Influence of Diets upon Growth".

From Hopkins' "Note", however, one would think the matter of the unknown food factors had not even arisen in Osborne and Mendel's publication. Hopkins' "Note" states that in the German article, "Osborne and Mendel have described certain experiments which seem to show that young animals (rats) can grow when fed upon artificial diets consisting of 'purified constituents alone'" (Hopkins 1913: 97). Hopkins remarked that the results obtained by Osborne and Mendel "would therefore indicate that the accessory factors of uncertain nature (complex lipoids or 'vitamines' or 'hormones') which others have believed to be necessary are not, as a matter of fact, indispensable" (Hopkins 1913: 97). As Hopkins tells it in his "Note", the incidental treatment given to the accessory food factors in the Americans' 1912 articles was effectively a denial of the importance of the investigation of accessory food factors and therefore posed a potential threat to continued research in the area. In a tone that disparages the efforts of Osborne and Mendel, Hopkins wrote, "These particular experiments [in which animals grew on 'purified constituents alone'] were in a sense merely incidental to a wide enquiry, on the part of the authors quoted, into the nutritive efficiency of various proteins" (Hopkins 1913: 97). The tone of this comment is one of disciplining Osborne and Mendel for approaching their work in the wrong light. It seems, therefore, that Hopkins had a problem with Osborne and Mendel's approach to the as yet unknown factors. He does not even address their amino acid results. His point is that the contribution their article makes is questionable insofar as it does not even take up the problem of animal growth on a diet of purified foods, let alone the problem of the as yet unknown factors. Hopkins argues that the article's problematic is problematic. Hopkins concluded the "Note" in the Biochemical Journal by implying that Osborne and Mendel's two 1912 articles were a call to bio-chemists to give up the search for these accessory food factors. "The purpose of the present note is to indicate that there is still reason for a continuance of the search for

special accessory substances of potent influence upon growth” (Hopkins 1913: 99). Hopkins was re-asserting that accessory food factors were in fact a problem in bio-chemistry at the time, that is, they were a site for productive investigation.

It is interesting that Hopkins’ disciplining tone is achieved in part by his mis-reading of Osborne and Mendel’s data to have concerned accessory food factors, when in fact they had only been concerned with the effect of different proteins in the diet. The crux of the argument for this reading by Hopkins flails against a strawman. In his “Note”, Hopkins paints Osborne and Mendel’s results as specious because in their second article of 1912, in which they claim, according to Hopkins, to have secured growth without accessory food factors, they do present evidence of growth and further do not cite their first experiment where they did present evidence of growth.

In a later paper [Osborne and Mendel 1912, 2] the authors again refer incidentally to the subject and speak of having obtained ‘a considerable degree of success’ by feeding in the absence of ‘the hypothetical organic hormones,’ etc [1912,2, p. 242].

But the weight-curves given in this paper show little more than maintenance of the animal, without growth; while, for some reason, the three experiments of the earlier paper which showed complete success in the promotion of vigorous growth are not further quoted (Hopkins 1913: 97).

Such would be an entirely valid move on Hopkins’ part if securing growth had been the objective of Osborne and Mendel’s second 1912 article. This argument does not hold, however, as the ‘success’ claimed by Osborne and Mendel referred to work undertaken to investigate the possibility of maintenance, not growth, on such a diet. It is therefore reasonable that Osborne and Mendel would not further cite the growth attained in their German article, because growth was not relevant to the second study (concerning maintenance) published in 1912.

Hopkins was not grappling here with error on the part of Osborne and Mendel, but with their approach to the field. Hopkins sets up the straw man of Osborne and Mendel’s

work as a denial of the possibility of accessory food factors and effectively establishes a site for his outrage that the importance of research into accessory food factors would be denied. In doing so, he effectively establishes the as yet unknown as central to the field of bio-chemistry research at the time. Hopkins' contribution through this "Note" cannot be considered one of fact; he mis-reads data and adds none of his own. Rather, his contribution here is to insist on the encounter with the as yet unknown as a problem in bio-chemistry. It is no longer acceptable not to consider the as yet unknown as a problem. His success with this article is to insert into the field the unknown factors as a site of potential. This is a point, therefore, in the actualization of the vitamins.

Osborne and Mendel waste no time informing Hopkins that his objection to their data is mis-informed, in a letter to him in February 1913. "First we wish to point out that you were under a misapprehension when writing the last paragraph of page 97. Our paper in *J. Biol. Chem.* 13, p.233, concerned only maintenance, and all data relating to growth were intentionally omitted" (Becker 1968: 222). Significantly, however, Osborne and Mendel are sensitive to the real difference Hopkins' article made to the field and their work and in their response to Hopkins, formulate the 'as yet unknown' as a problem. In this letter Osborne and Mendel argue that because Hopkins has mis-read their efforts, he had painted them as not interested in the accessory food factor question, when really, "we are more alive to the importance of this question than ever before" (Becker 1968: 222).

It is significant that Osborne and Mendel invoke recollection as they reconfigure the field to include the as yet unknown as a problem. Memory, Bergson tells us, "has successive and distinct degrees of tension or of vitality" (Bergson 1988: 170). It is precisely insofar as their recollections are gathered that they (the recollections) have an increasing vitality. That is, Osborne and Mendel feel now more alive to the problem than they ever have before

because they are concentrating on it and all the memories of their encounters with the as yet unknown are concentrated in their present attention. The concentration of their memories enables a different perception of the as yet unknown factor than they had previously enjoyed. The increased tension of duration in this instant, that is the length of time that the past stays with and informs the present, expresses a “degree of the concentrating power of perception” (Bergson 1988: 210). As we saw in Bergson’s mobius loop, there are two aspects to memory and perception here. On the one side of the mobius loop, the scientists “receive impressions sent in by . . . a real object” and, on the other hand, they are subject to “the influence of a virtual object” (Bergson 1988: 129). In their “than ever before”, Osborne and Mendel collect their recollections in a moment of tension which both opens them to the virtual, to the problematic idea within the problematic moment of their experiments, and projects their powers of concentration and perception toward the recognition of the unknown object (Bergson 1988: 116).

In their February 13 letter to Hopkins, Osborne and Mendel address some of the concerns Hopkins expressed regarding their experimental system, but also point to the complexity of the moment in biochemistry. “All of us are dealing with problems which involve extremely complicated conditions, and we must be prepared to be confronted with unexpected results for some time to come” (Becker 1968: 223). Osborne and Mendel even reply to one of Hopkins’ more inflammatory remarks that “it is difficult to understand how rats can depend for normal growth upon a very exact balance of particular acid and basic ions” (Hopkins 1912: 98) by pointing to the power of the unknown in contemporary biochemistry. “We have long since ceased to be troubled by things which we find it difficult to understand” (Becker 1968: 223). I have anticipated this remark in the Introduction. Given the context, this statement does seem to be Osborne and Mendel putting on a brave face

and it seems clear that this is not the place they would confide to Hopkins that their anxiety won't let them sleep, but neither does this mean the statement is entirely false. I argue that the statement resonates with Canguilhem's notion that health allows one to feel one is the establisher of vital norms. Osborne and Mendel are used to living in this problematic environment. Healthy intellectual life is lived in the space of the problem and Osborne and Mendel tell Hopkins that they have long since been convinced that the difficulty of the situation is not an indication that either they or their work are in any trouble.

Osborne and Mendel's letter to Hopkins was genial, given the acerbic tone of Hopkins' "Note", which was littered with phrases such as, "for some reason" and "it is difficult to understand". Further, in his "Note" Hopkins said several times that Osborne and Mendel needed to repeat their work, the account of how Hopkins followed the exact plan of Osborne and Mendel is strident and the whole article is curt. In response to Osborne and Mendel's letter regarding the "Note", accepting that there were errors in the German publication, and pointing out the misapprehension of their second article, Hopkins expressed his gratitude for their benevolent response.

I am really grateful to you for sending me your very kind letter: especially grateful because ever since the "Note" was published in the *Biochemical Journal*, I have resented its tone, feeling that it ought to have been written differently. The fact is I have done so much work in this (and very successful) endeavour to separate the unknown substances which affect growth, that when your paper in Vol 80 of the 'Zeitschrift' came out I suffered from an attack of nerves! I was at the British Association in Dundee at the time, when Casimir Funk brought me a copy of the above Journal and we were both somewhat overcome (Becker 1968: 224).

We are granted here a view of a moment in the process by which an object moves from being 'as yet unknown' to fact. We see here Hopkins and Funk grappling with the unexpected with the resources they have already available to them (Massumi, 2002: 214). At the British Association in Dundee, the as yet unknown factor is an "object of anecdotal

discourse of gossip” (Massumi, 2002: 214). Hopkins’ case of nerves and his gossip session with Funk remind us that despite all the institutional support scientists have to maintain their sangfroid, they are dealing with surprise (Massumi, 2002: 233).

Also in this letter to Osborne and Mendel, Hopkins told them that he was going to be too busy in the next year to pursue this particular line of investigation. “I do not see much chance of personal efforts in connection with the matter during the present year. I have an unpleasantly busy time ahead of me” (Becker 1968: 225). Again, Hopkins’ approach speaks to the significance of the site of the problem in the field, rather than the accumulation of data. Osborne and Mendel now see the as yet unknown factors as a problem, Hopkins did not need to see the research through to results, did not need to establish the facts and results. His effect in this exchange was to establish a site of incipience, a point of potential in the larger field of bio-chemistry.

Perhaps because Hopkins knew that he would not have time to pursue and publish research, he spent this letter to Osborne and Mendel telling them of his thoughts on the matter of the ‘new domain’ with which they ‘were all dealing’. Interestingly, Hopkins had not successfully secured growth using what he referred to as “crystalline Vitamines of Funk” (Becker 1968: 225). Whenever Hopkins managed to obtain crystalline fractions, they were not effective in securing growth, but Hopkins could secure “great concentration of activity in amorphous residue” (Becker 1968: 225). This is Hopkins’ formulation of the problem; what is secured in amorphous residue is not a chemical, but an activity. In their response to Hopkins, Osborne and Mendel communicate in the same vein. “In fact we shall not conclude that artificial foods are complete unless they promote normal growth as do our milk powder foods” (Becker 1968: 229). Here then we can see the incorporation of the idea that growth, vigor and well-being are the test for the presence of some chemical in the food.

In this way, growth, vigor and well-being also come to characterize the chemical, whatever it may be, Funk's Vitamines or not. The as yet unknown substance is being marked increasingly by its activity, rather than its chemical formula.

In their response to Hopkins, Osborne and Mendel stated that as a result of the work they had just told him about, they had to admit that their earlier work had caused confusion. This admission is significant because it speaks to the importance of problems in the development of a field of research. The confusion they caused lay in painting the field of bio-chemistry as not containing the problem of the as yet unknown. The confusion, therefore, was not over the identity of the facts in bio-chemistry at the time. Rather, there was confusion over the points of difference, incipience and potential in the field. Success here lies in the correct formulation of the problem. Once Osborne and Mendel had been disciplined by Hopkins to consider the unknown factors as a problem, they requested that Hopkins not publish anything refuting their earlier work until they had had a chance to "straighten out the confusion which we have caused" (Becker 1968: 229). In this way Osborne and Mendel signal their intention to become players in the field by attempting to formulate the problem of the as yet unknown food factors.

McCollum vs. Osborne and Mendel: What's the Problem?

In accounts of the emergence of vitamins, an opportunity to mention the dispute of 1913 between Osborne and Mendel in Connecticut and E.V. McCollum in Wisconsin is never missed. On June 1 1913, the Journal of Biological Chemistry received for publication an article by E.V. McCollum entitled, "The Necessity of Certain Lipins in the Diet during Growth" and on June 21 1913, just three weeks later, the Journal received for publication an article by Osborne and Mendel entitled, "The Relation of Growth to the Chemical Constituents of the Diet", each of which concentrated on the isolation, purification and

effect on growth of the 'as yet unknown' factor of Osborne and Mendel's Carnegie funded research. These publications appeared just about one year after Hopkins' publication and the correspondence between Hopkins and Osborne and Mendel. Osborne and Mendel's response to McCollum's publication was drastically different than their response to Hopkins' work. Osborne and Mendel wrote as quickly as they reasonably could to McCollum⁵⁹ to discipline him for treading on their toes, indeed for even entering into the field that they considered their own. The dispute has managed to preserve itself in the historical literature, as the tale is often told as one of Osborne and Mendel's 'discovery of vitamins' and McCollum's fortuitous prior publication of his findings.

While Hopkins had also trodden on the toes of Osborne and Mendel and their feeding experiments with isolated food substances, they were not angry with him. Indeed, they had been understanding and hoped that he would not think they were so ungenerous as to be unwilling to share the field. Later, in disciplining McCollum, they reprimanded him for not handling himself more in the manner that Hopkins had by writing to them and letting them know his intentions. Although they tell McCollum that in this way he might have acted 'like Hopkins', this is not how Hopkins had handled himself. He had only written to the Americans after he had published his work. McCollum was sufficiently contrite in his reply to appease Osborne and Mendel, but he also made the point that he had not known the course Osborne and Mendel would have liked him to take, and thus it must be apparent that he meant no offence by preceding in the manner in which he had.

Osborne and Mendel, as should be apparent by now, were not interested in the question of the as yet unknown food factors when Hopkins published his 1912 results. They did not see the food factors as a terribly interesting site of investigation and so were quite

⁵⁹ There are some really interesting letters between Osborne and Mendel that summer as they decide exactly how sharp to be with McCollum in their letter (Becker 1966).

happy to have Hopkins pursue the matter even as it emerged from their own work. It might also be remembered, however, that Hopkins was senior to Osborne and Mendel and it is possible that his position at Cambridge allowed Osborne and Mendel to see his interposition as inoffensive. McCollum, on the other hand, was of a younger generation than Osborne and Mendel, he had worked in Osborne's laboratory when he could find no organic chemistry work, and Osborne considered McCollum a student. One wonders too about the effect that institutional hierarchy would have had on Osborne and Mendel's sensitivity to McCollum's publication. Perhaps just as it was acceptable that Hopkins at Cambridge intruded upon their field, it simply was not right that groundbreaking work should come out of the Wisconsin Agricultural Experimental Station before it came out of the collaborative work between America's first ever Experiment Station and the prestigious Yale University. It seems apparent that some differential power is at work in the relationship between the laboratories involved in investigations of 'accessory food factors'.

The dispute between Osborne and Mendel and McCollum is consistently presented in the literature as an event of some significance. I would want to caution against attributing too much significance to the simple fact of the argument and as the end of analysis. The event is perhaps given some significance in the literature because these are the papers that, it is claimed, are the presentation of the 'discovery of vitamins'. However, my analysis to this point has developed multiple epistemological stages through which the vitamins were actualized. Upon examining the publications it seems just as likely that the attribution of 'discovery' to this moment may well have been precipitated out of the argument itself, as each of the publications is of yet another working away at the problem of the as yet unknown food factors. We have seen in this chapter and previous chapters the many approaches made to the as yet unknown factor: surely these two approaches differ

qualitatively as well, and are not the approaches which are the 'more' of discovery. In fact, these works emerge out of work that was so run of the mill that Osborne and Mendel can be seen in their correspondence with each other prior to these publications, not to be worried in the least about interlopers and even to assure one another that there was lots of time to publish. I argue that a careful review of the articles will show that the fight is not over the 'discovery' of vitamins, but rather points precisely to the irresolvability of origins.

As in the case of Osborne and Mendel's isolated protein feeding experiment, in his article on the relation of lipins⁶⁰ to growth, it appears as if McCollum began investigating one aspect of nutrition, only to encounter another problem along the way. In McCollum's case, he began by investigating the influence of salt mixtures in purified rations on growth and encountered the problem of lipins. Here again we see that the 'as yet unknown' opens a number of possible and profitable routes for research. Various limits yield various potentials. This time, rather than encountering the as yet unknown through research on proteins, or reviewing the literature, or trying to isolate a chemical, the as yet unknown is encountered through feeding experiments designed to investigate salts in diet.

In this publication, McCollum noted that in beginning this investigation, his laboratory had been interested in the "influence of the composition and quantity of the inorganic content of the ration on growth in the rat" (McCollum 1913: 167). To investigate this influence, McCollum's lab, "employed rations compounded of pure casein [a milk protein], carbohydrates, and salt mixtures made up of pure reagents, and the same rations in

⁶⁰ Despite the fact that the investigation had been begun to test the value of the salt mixture for growth in the rats, McCollum pursued further investigation of this interesting phenomenon of growth resumption. Indeed, the last we hear of the salt mixture experiments is when McCollum apologizes for the graphs he had chosen to present as they represent success in inducing a resumption of growth, rather than success with the salt mixture experiments. "It should be borne in mind that these experiments were intended primarily to furnish data as to the values of the salt mixtures supplied by the rations and do not in all cases represent the highest degree of success attained by feeding these rations" (McCollum 1913: 169).

which a part of the carbohydrates was replaced by lard” (McCollum 1913: 167). During these investigations it was found that rats could grow for a period of 70-120 days and that even after growth had ceased (before it would on a diet of whole foods), the rats could be maintained in what appeared to be a healthy state, despite their lack of growth. Once growth had ceased and the rats had been maintained in apparent good health, McCollum noticed, they could be made to grow again if they were placed back on a ration of whole and mixed foodstuffs.

Speculating upon the resumption of growth in the rats, McCollum reasoned that there was something missing from the purified rations, which the rats had stored while eating whole foods and drew upon for the period during which growth was observed but that when their growth ceased, they had exhausted their stored supply. The ration of mixed and whole foods would supply immediately and then replenish the store of this substance to the animal. Such a statement sounds particularly similar to others that we have heard repeated by other bio-chemists previously. However, McCollum’s repetition is also different.

The fact that a rat of 40 to 50 grams in weight can grow normally during three months or more on such rations, then cease to grow but maintain its weight and a well nourished appearance for weeks and then resume growth on a ration containing certain naturally occurring food-stuffs would lead one to the belief that on these mixtures of purified food substances the animals run out of some *organic complex* which is indispensable for further growth but without which maintenance in a fairly good nutritive state is possible (McCollum 1913: 168; italics mine).

Significantly, the argument here is not that some chemical test determined that the substance was organic. The significance of what McCollum was presenting, therefore, was not some fact. Rather, it was a re-formulation of the problem. This is not an actualization of an object, but a maneuver in the realm of potential, the zone of indeterminacy. Here we do not have an object that is more fully revealed, but the beginning of a differentiation of a difference from within the zone of potential. This statement is a growing articulation of the

problem and a growing store of knowledge of what the problem is, even beyond the gossip session of Hopkins and Funk. That such was a presentation of the problem is made apparent by the fact that it was after the researchers in McCollum's lab had come to believe that the substance was organic that they found that the substance could be extracted from eggs or butter with ether, a solvent for organic complexes. They were still tinkering with other aspects of the diet when they found the substance was ether soluble.

After numerous attempts to prevent the occurrence of growth suspension by nice adjustments between the various ingredients of our diets, we have found that the failure of rats to make further growth, after being brought to this 'critical' point on mixtures of isolated food substances, is due to a lack of certain ether-soluble substances in the diet. These can be supplied by the ether extract of egg or of butter (McCollum 1913: 168).

It is interesting to note that at this point, McCollum does not argue for or even attempt to show that the effect of these substances on growth is beyond the proportion of energy supplied by the caloric contribution of the foods which, before this report, was a usual method for distinguishing these 'accessory food factors' from traditionally accepted food substances. Such is no longer the problem. McCollum cites Hopkins' observation that the benefits of milk are out of proportion to their calorific or nitrogen contribution to metabolism, but does not make the argument for his own findings. These statements are part of the constitutive constraints in place through which the problem could become increasingly clarified. McCollum might be read as assuming that these aspects of the accessory food factor definition had already been sufficiently established. McCollum does distinguish the accessory factors from regular food substances by showing that not all lipins provide the beneficial effects of egg extract or butter. "In no instance have we obtained such a result by the feeding of lard, or of olive oil. It is therefore not merely the absence of fats from the diet which causes the suspension of growth" (McCollum 1913: 174). The significance of the work is having established that the complex, essential to growth, was

organic. Such an attribution had not been made yet and the attribution narrows the possibilities for the 'as yet unknown', the significance is not in the answer 'organic', but in the development of the problem of the vitamins. The attribution of organic to the accessory food factors, renders the problem of the vitamins one of organizing principles. Further, in the development of the problem of the vitamins we see a narrowing of the possibilities for the 'as yet unknown', thus the vitamins are increasingly actualized.

In their article, "The Relation of Growth to the Chemical Constituents of the Diet", which followed the publication of McCollum's article by three weeks, Osborne and Mendel reviewed the literature to the time of their writing to argue that the field of possibilities of the roles of various substances needed for life and their chemical reactions was almost limitless and that it was "futile at the present time, to develop detailed hypotheses respecting the almost innumerable possibilities involved" (Osborne 1913: 312). They argued that the "greatest promise of success in discovering the food factors which determine successful growth lies in seeking them in some chemical constituents of such diets as have proved adequate to promote growth" (Osborne 1913: 312). Here we have an explicit statement of the almost limitless potential presented by the problem. It is significant that the statement is not only of the work that Osborne and Mendel were undertaking at the time. It is a statement of the state of the field of research at the time. This statement is significant insofar as it highlights nicely that the virtual is wholly relational and also confirms that Hopkins' contest with Osborne and Mendel's work was not with their results, but with the effect on the field made by their formulation of the problem. Because the unknown is at the limit of the field of research at the time, the limits are constituted by the field, and so are shared broadly by the community of researchers.

♦

Osborne and Mendel reviewed their experience to the current time and asked the question, "What light does the experience thus far accumulated throw upon the nature of the essential substance, if there be such? Is it organic or inorganic, or both?" (Osborne 1913: 314). Because of the character of organic chemistry at the time, such a question is not just a matter of whether the complex had a carbon backbone or not. Rather, the question was whether they were investigating a chemical compound whose effects could be accounted for according to linear causality, as in a mineral or a salt, or whether they needed to consider a dynamic of a different order, statistical, exponential, those found in self-organizing systems. That is, the problem was the organizing principle of nutrition and the question becomes, is this principle one of cause and effect, or one of excess?

Such is the problem as it was emerging and yet Osborne and Mendel's approach was different from McCollum's. "It seems probable that the missing substance is organic in nature; for the 'protein-free milk' may be presumed to contain all of the inorganic constituents of the milk" (Osborne 1913: 315). McCollum had concluded that the necessary accessory food factor was organic based upon the fact that it was ether soluble. Osborne and Mendel made their conclusion based upon a presumption that all inorganic elements were being supplied. These conclusions are not derived from different problems but from different lines of reasoning. Osborne and Mendel also come to an investigation of milk fat but from a different direction than McCollum. Where McCollum extracted carbon compounds from butter and egg with ether, and so found there existed an ether soluble substance, Osborne and Mendel note that they had centrifuged off "the cream and likewise any cellular elements (mammary gland cells, leucocytes, bacteria, etc.)" (Osborne 1913: 318).

The mention of mammary gland cells also makes more explicit the re-configuration of the problem as it is introduced by the 'organic' and that is its connection to the salient

characteristic of life, self-regulating organization. Because these elements had been “removed mechanically by the centrifugal process preliminary to the manufacture of our protein-free milk, or removed by subsequent filtration processes”, Osborne and Mendel were “led first of all to supply the cream component, in the form of butter” when looking for the “essential accessory factor” (Osborne 1913: 319). Osborne and Mendel go on to state that when, following the reasoning suggested above, rats that had stopped growing were fed a quantity of unsalted butter, they recovered and growth resumed. “It would seem, therefore, as if a substance exerting a marked influence upon growth were present in butter, and that this is largely, if not wholly, removed in the preparation of our natural ‘protein-free milk’” (Osborne 1913: 319). Osborne and Mendel had already stated that all the inorganic constituents necessary to an adequate diet were supplied in their protein free milk. Therefore, it could be concluded that because the substance was probably removed from the protein free milk, it was an organic constituent. Although Osborne and Mendel were sufficiently confident that the essential accessory complex was organic, they felt it was too early to speculate as to the chemical nature of the “effective substance supplied by the butter” (Osborne 1913: 319).

The relevant point to emerge from Osborne and Mendel’s as well as McCollum’s experiments, is not that a substance had been isolated from butter fat. What is significant about the finding of McCollum and Osborne and Mendel is that with the assertion that the ‘as yet unknown’ factor was organic, they re-configure the problem space of the as yet unknown factor. In determining the substance to be organic, they still have not determined what the substance is. But what they have done is to distinguish it from mineral and the power of minerals to act in causal relations and linked the accessory factors with life, and powers of transformation.

The establishment of the significance of the organic to the problem of the unknown food factors allows us to return to the significance of accessory food factors as formulated by Hopkins.⁶¹ In the first chapter we saw accessory food factors contribute to a changing understanding of human physiology. The principles of the organism's organization were no longer constancy and the maintenance of a steady state, but rather were constituted by systems and processes in dynamic equilibrium. The added significance of the problem of the organic in unknown food factors is that the organic is that which enables regulatory processes. The fact that the complexes are organic ties them to the functions of regulation within the organism. Therefore, the problem of the vitamins becomes increasingly one of the organizing principles of life as a self-regulating system. The problem emerges as one of a higher order of dynamics than cause and effect relations, an order of dynamics which concerns life. When the problem of the unknown food factors is defined as organic, the scientists conceive the vitamins as a set of organizing principles; principles along the lines of which organisms' regulatory processes flow and follow.

These organizing principles can be articulated by considering what was known about the vitamins. The conclusions concerning the vitamins by this point are three-fold and telling in their attributes. First, they are 'accessory', second, they are 'essential' and third their power is in organizing life. That which is accessory, therefore, is in principle necessary to the organization of life. It is the excess that stimulates the organization of life. The excess is essential and is a driving force of life. We will see this later too, once the vitamin concept is more fully articulated. Insofar as the vitamins are in excess they are the power of the organic, of life.

⁶¹ Hopkins did also postulate that the accessory food factors were likely organic.

Despite the fact that these two articles are hailed as the ‘discovery of vitamins’, what is of interest is not the fact that they add ‘a vitamin’ to the accumulated knowledge of the field, but the further configuration of the problem which they contribute. This configuration of the problem is what Canguilhem would characterize as a correct formulation of the problem because it is a formulation of the problem of self-organization. The objects of the life sciences are self-organizing systems. That is, the problem has not gone away, has not disappeared with even the solution of ‘organic’. It persists as an organizing principle – for the organism, and therefore also for the science.

In the first chapter I elaborated the significance to the organism’s organization of the catalytic function of the accessory food factors. In this chapter I have elaborated the significance of the vitamins as organic complexes. In the final chapter, “Grasping Life’s Potential: the Biopolitics of Nutrition”, I will return to the vitamins as organic catalysts to consider the significance to a sociology of food, eating and nutrition, of an organism organized along a line of differential.

Chapter Six

Grasping Life's Potential: The Bio-Politics of Nutrition

In his article, "Artificiality and Enlightenment: from Sociobiology to Biosociality", anthropologist Paul Rabinow writes that, "[t]he emergence of modern food, that is, food industrially processed to emphasize uniformity and commoditized as part of an internationalization of world agriculture and distribution, can be dated to the 1870-1914 period" (Rabinow 1999: 413). Rabinow cites as example the rise of white bread as a "constructed consumer need linked to advertising, transportation expansion, [and] a host of processing and preservation techniques" (Rabinow 1999: 414). Rabinow characterizes these processes as the "enculturation of nature" and argues that this 'enculturation of nature' continues today, even through a growing consumer demand for 'natural' food and 'traditional' products (Rabinow 1999: 414). Rabinow argues that once techniques were developed by which to manipulate natural products to suit consumer and industrial demands, they could be manipulated such that they fit other bio-political specifications, such as 'nutrition' (Rabinow 1999: 414).

Rabinow alerts us to the pitfalls of normative judgements in our historical analyses, which link artificiality to risk and danger and which form a sense of "certitude that the initial situation ... was always incomparably better" (Rabinow 1999: 416). He appeals to the work of French historian of science François Dagognet in order to remind us that nature is malleable and for millennia has been a "*bricoleur*, an elementary logic of combinations, yielding an infinity of potential differences" (Rabinow 1999: 416). That is, there has never been a 'nature' without an 'enculturation of nature'. Dagognet dares us to accept, in our historical accounts, the possibility of manipulation against the threat of "veneration before the immensity of 'that which is'" (in Rabinow 1999: 416). Ultimately, Rabinow says that if nature is always an encultured nature, then culture too must be understood as a 'encultured-

naturalized-culture' and he argues that new agricultural knowledge and processes have led to new processes in life, such as new tastes, and in labour, such as new distribution networks (Rabinow 1999: 415).

Traditional tastes pose a challenge, not a threat to technoscience; the more one specifies what is missing from the new product the more the civilizing process proceeds. ... If eighteenth-century tomatoes are your fancy, there is no reason a priori why one day a boutique biotech company aiming at the Berkeley or Cambridge market couldn't produce one that is consistently pesticide resistant, transportable, and delicious for you – and those just like you (Rabinow 1999: 415).

Rabinow thereby poses the challenge of accepting the possibility of cultural processes as fundamentally made of manipulation. "As with nature, so too, it seems, with culture?" (Rabinow 1999: 416). I argue, however, that we need not appeal immediately to culture to understand our human selves as bricolage, for it is not only our culture which renders us malleable. Following Rabinow and Dagonnet, we might enquire into the natural aspects of human life processes, such as eating and nutrition, without appealing to some monolithic and unchanging 'human nature'. We might thereby understand human life processes, such as nutritional processes, as a bricolage of manipulation, precisely insofar as they are natural. That is, it is not only because nature has been affected by culture that it has been manipulated, but I suggest we might look to nature for our bodies' capacity to be affected in different ways.

We might ask after the natural processes as well as the cultural processes by which 'nutrition' has become a bio-political specification. Our understanding of what happened to food and agriculture as the result of new knowledge and practice between 1870 and 1914 can be enhanced if we use the biology of the vitamins to ask what bio-technic arrangements brought about this transformation of the life world. To this end, I want to return in this chapter to an analysis similar to that undertaken in the first chapter, "Understanding the

Catalytic Function”, to consider some of the differences made by the emergence of the vitamin concept and the discovery of vitamins. In the first chapter I considered the difference made by the vitamin concept to theories of physiology. Here I want to consider the significance of the emergence of the vitamin concept and the discovery of the vitamins to a sociology of the body and of nutrition. In this chapter I am following Canguilhem on two fronts. First, this chapter is a consideration of the concepts of the life sciences. Second, in this chapter I will also argue for the significance of the vitamin concept for emergent life norms. In arguing for the significance of the vitamin concept to emergent life norms, I am following Canguilhem in understanding the concept as integrated to the organism.

In Canguilhem’s understanding that the concepts of the biological sciences are integrated to the organism, we have an understanding that problems of the life sciences are attempts to investigate and understand the problems and the elaborations of life. Because the life sciences grapple with questions of life, their problems are of problems posed by life and the concepts of the life sciences must be understood as integrated to the organism under study as a solution to the problems posed by life. The life sciences apprehend the activities for which living organisms are equipped, and the information they are capable of assimilating. The concept in the biological sciences is of something, some process, which makes a difference to the life of the organism itself. Canguilhem’s argument therefore is that the life sciences diagnose that which is of value to a living organism. That is, concepts within the life sciences are an articulation of some vital capacity within the organism.

And yet, Canguilhem makes it clear that the history of science presents histories of discourse, not of natural objects. Scientific objects are secondary objects. In articulating concepts of the life sciences, biologists are responsible for what I think of as sourcing vital concepts. This notion of sourcing is useful because ‘to source’ has the double sense of

finding something that is already there and also of putting something in place. Therefore, 'sourcing' allows an understanding of the concept as integrated to the object under study and also the status of the scientific object as a secondary object. This two-fold meaning enables historians of bricolage to make an account of the subsequent elaborations of life from the finding and the placing of vital capacities in scientific objects. Armed with an analysis of the concepts of the life sciences, it is the role of the historian with a sense of nature as bricoleur to pose the problems through which life's forms have been and might be elaborated. That is, an historian attentive to the concepts of the life sciences might suggest to the social sciences some of the capacities by which bodies are capable of being affected variously.

I have a second guide for this chapter, and that is Henri Bergson. For Bergson, philosophy (I am taking disciplinary liberty here, I realize) functions underneath and "alongside science", "drawing it out" and "making explicit assumed commitments to philosophical and ontological concepts" (Grosz 2004: 12). That is, I want to go further than I did in the first chapter, to take some liberties with the biology and to render biology as a contribution to sociology. If we want to think a sociology of the body, we need to analyze, for sociology, the significance of biology and biology's concepts and objects. I want to expound upon the significance of the biology of the vitamins through a reading of contemporary social theory with an eye to the sociological and political significance of biology.

Even at the time of their discovery, it was understood that vitamins and the difference vitamins would make to human nutrition was a matter that properly belonged to the consideration of the 'social philosopher', because they brought to bear questions of life, the organization of life and quality of life. "We have seen that the problem of food supply is not one which can be dismissed by the social philosopher or solved by the calculations of

the economist” (Mendel 1916: 59). That is, we must take these problems seriously and neither will a simple accounting suffice. To understand the problem of food supply here requires thinking about life – a subject that simply does not ‘add up’.

Until this point, I have concentrated on a discussion of the significance of that which is real, but is not actual, or current, in the experiments which led to the discovery of vitamins. I have concentrated upon a discussion of the virtual. My discussion has concerned problems, the ‘as yet unknown’ and the press of the materiality of the world beyond that which can be named, called, said. I have focused on the significance of various forms of excess. I have done so as a kind of corrective to approaches to the social study of science and scientific knowledge which would accord the social world of the scientist too much power, all the power, which limits the power in science to merely the power to pin down or name. In this chapter I maintain this focus on the effect of material agency, beyond the realm of human agency, to constitute the social world.

This chapter is a kind of sketch, more a speculative act than a definitive argument. The empirical evidence is not provided in the detail it is in earlier chapters, but is sufficient to the gesture I want to make. In this chapter, I mean to suggest a terrain, without necessarily providing all the tools necessary to explore that terrain. Driving the chapter is a desire to explore what I found to be an uncanny resemblance between the social theory informing my research and the explanations of the functions of the vitamins within the biology I was reading. I would read a piece of social theory, which would talk, for example about accounting for the cleavages that connect together the lines of processes. And I would think, ‘yes, the vitamins do that’. In fact, if anything, the resemblance between the social theory I was reading and the biological theory in the empirical material assured me that I was reading theory that would let me account for the vitamins in the social world. This chapter

therefore is an effort to lay out, to sketch briefly, the iso-morphism I saw between the social theory and the vitamin science. I do not suggest that such an iso-morphism is a sufficient account of the integration of the vitamins into bio-politics (it allows only for a sketch here). My point, rather, is that the iso-morphism is suggestive in important ways.

I argue that the vitamins are a site of the co-production of nature and society. I will argue that the vitamins are sites of the 'mutational aptitude' of human existence and that the changes in human life processes discussed by Rabinow above were enabled by a seizing of the potential of human life for change as embodied in the vitamins. This happens, as Rabinow has pointed out, in processes of 'nutrition'. Below, therefore, I will explicate what about the organic and catalytic character of the vitamins for human existence constitutes a site of life's mutational aptitude that can be grasped variously. First, however, I suggest briefly that such an undertaking requires that the social sciences take seriously and read for themselves the data of the life sciences.

Reading Biological Data for Sociology

In arguing that vitamins are a site of the co-production of nature and society I do not only want to ask after the cultural and social forces affecting our biology. I want also want to consider our bodies as contributing to the possibility for such cultural and social milieux to sustain themselves. Such an approach is uncommon in contemporary science studies; social theory that concerns itself with the body, has not often appealed to the biological and life sciences for help in thinking the body for the social sciences. For example, the thrust of much of the early feminist literature on the biological sciences was to uncover the ways in which gendered discourses and practices had been naturalized within scientific discourse. Work such as this took as its object of investigation figurations of gender that had been written as natural in scientific discourse and had been naturalized within physical bodies

through practices and performances (Butler 1990; Fox Keller 1989; Grosz 1994; Jaggar 1988). That is, there is no biological process that is not shaped by social and scientific processes, practices and discourses. Such work was groundbreaking because it spoke to aspects of social life that had been naturalized and therefore were thought to be entirely biological.

It was easy to forget, vilify or discount any explanations of bodies offered by the biological and life sciences. The unfortunate side-effect of a vilification of the life sciences for social theory was to understand bodies, human bodies, as an undifferentiated and undifferentiating mass of corporeality, waiting to be shaped by the hands of social forces in their image. While the work was significant as it opened the door to an understanding of the work of social forces as becoming, as emergent, the limitation of this perspective is that the world is only opened to becoming, only emerges, as an effect of human intervention and the human mind. “[B]iochemistry ... can be disregarded only in a theoretical milieu that takes biology to be inert, a milieu that, despite its expressed interest in rethinking the body, still presumes that the microstructure of the body does not contribute to the play of condensation, displacement, and deferred action that is now so routinely attributed to culture, signification, or sociality” (Wilson, 2004: 5).

I suggest that an understanding of cultural and political transformation will require a more nuanced account of “the body’s immersion in the world” (Grosz 2004: 3). This account will be of processes in the social world which are not entirely determined by, are not entirely effected by human minds and the actions of human agents, but in which our bodies and the bio-chemistry of our bodies participate. These are accounts of the body as a constellation of forces in excess of the forces given to it by current cultural concepts.

The social sciences will not cede their place to the biological sciences, or any biological reductionism, in such a move, but rather will be made richer and more capable of responding to and enabling social change; the social sciences will be rendered more response-able. Because an appeal to the biological sciences from cultural and social theorists takes its cue from the work of social theorists on the role of political, cultural, scientific and technological practices and discourses in shaping bodies, their attempt is not to revive a biological determinism. Rather, this is an attempt to take bodies rather more seriously, as more than inert masses functioning as the stage upon which the performances of "biology" are played out and inscribed. The attempt is to understand the intermeshing, the reticulation of the biological and the cultural, social and political in thinking the place of our bodies in our social worlds. For example, perhaps feminists would do well to consider seriously the science of digestion and excretion in their considerations of the anorexic body. Surely there is something to be learned about the composition of the anorexic body from a study of the kinds of biological material that enables women (and increasingly, men) to starve themselves.

One way to begin is to take seriously the biological and life sciences as materialist accounts of processes of becoming. Taking seriously the life sciences as materialist accounts of the world in becoming is to take account of a body that is of the world, has forces of becoming and develops new modes of being, independent of what we might think of them. Feminist theorists, having taken to heart the initial feminist insight that our bodies are not inert through time, have taken up considerations of biology in order to think the complexity of operations that our bodies must undertake in their differentiation and emergence. Within biology, cultural and social theorists have found allies insofar as the objects of the biological and life sciences are characterized by dynamism, change and openness to new modes of being. The appeal to the biological and life sciences by cultural and social theorists has

provided them with elaborations and articulations of dynamism in life. They have come away with an understanding of biological material as differentiating (Wilson 2004), as something, the successful functioning of which is marked by an openness to difference and change. The unique characteristic of biological material consists in its capacity for change. To an already developed understanding of the shifting effects of social and cultural forces on bodies has been added an understanding that biology and physiology also affect and effect shifts and changes in its forces. For example, we can see the highly contested and ethically fraught work of genetic engineering as the outcome of the biological invention and inventiveness of genes and genetic replication as it is the invention of contemporary science, technology and bio-medicine.

This is not to argue that cultural theorists have discovered in the biological sciences a cultural or sociological analysis of bodies and embodiment. However, they have discovered in biology and the life sciences some useful tools for thinking bodies for social and cultural theory. In the biological and life sciences, cultural theorists have found an aid to and an imperative for their “effort to substitute for the ready made what is in the process of becoming” (Pearson 1997: 125). In taking up a point of view that arises out of life itself, cultural and social theorists have been required to develop an “ontology or a concept of becoming” (Olkowski 1999: 94). Only a theory of difference and becoming will allow us to account for the interaction and development of different modes of embodiment (social and biological) and the new and different life forms and concepts (Olkowski 1999; Squire 2004), which understands these differences as qualitative. Now that the body is no longer an inert mass, we may as well accept the need for new theoretical tools.

The Physiological Significance of the Vitamins

The biological, or physiological, significance of the vitamins is three-fold. First, the body cannot synthesize them, they must be acquired by the organism from its environment. Second, they are organic. That is, they serve a regulatory function in the body, serving in the aid of the organism's organization. Finally, although they serve in maintaining the body's organization, they are catalytic and so are needed in only trace quantities.

We may reasonably regard a substance as a vitamin if it is essential to the life and wellbeing of an organism which does not possess the power to synthesize that substance and also if it is organic in nature and does not belong to any of the three great classes of foodstuffs, protein, fats and carbohydrates' (Drummond in Bronson 1930: 213).

In Elements of Food Biochemistry we read another definition,

The various vitamins differ so greatly in composition that it is impossible to define them in terms of their chemical structure as is done with other classes of compounds, such as carbohydrates, fats, and proteins. From the physiological point of view, which is probably the best for an accurate characterization, vitamins may be defined as organic compounds that are essential constituents of the diet. . . Thus they may be seen to differ from hormones in that the body cannot synthesize them, from trace elements in that they are organic compounds, and from the main structural and energy-yielding materials in that they are required in extremely minute amounts. The reason why such small amounts are effective is that their role in the chemistry of life processes is essentially catalytic (Peterson et.al. 1943: 181).

The vitamins are moments when the order of the organism is stimulated by a source from the external environment. Catalysts are differentials; they increase the rate of thermodynamically feasible reactions. In the scientific discussion concerning vitamins, they are frequently referred to as 'potencies', a term which can, and frequently does, replace any of the terms for vitamins at the time, such as 'accessory food factors', 'vitamines' or fat-soluble A and water-soluble B. "Since these experiences, published in 1913, comparable 'potencies' have been discovered in a number of animal fats . . ." (Mendel 1923: 67-68). These unknown and accessory food factors are packets that function as potential energy in

the metabolism of the organism. They boost reactions to proceed which, despite having all the elements they need to go forward, do so only at very slow rates, or not at all. Because they are packets of potential energy, we might visualize the vitamins as kind of spring, that, when released provides the energy for metabolic reactions to proceed. The reaction might look like a growing wave in the ocean.

Organisms require the vitamins because there are sites of abeyance within their metabolism. The significance of these sites of abeyance is that the differential of the vitamins is established across the boundary of the organism. There are various rifts in the metabolism of the organism. Because metabolic reactions are too slow, do not arrive on time, the integration of the vitamins from a food source provides that point of connection, makes the leap over the rift. The significance of this connective power is that an organism's nutrition is constituted in part by these points of potential, points of a kind of spring, waiting to be released and to provide the boost over the points of abeyance (Deleuze 1987: 255). It is the vitamins' traffic in differential that constitutes the organism's boundary. The border of the organism now is constituted by a kind of wave, "the positivity of transformation itself" (Doyle 2003: 131). There are, of course, several vitamins, meaning that there are various forces constituting the boundary of the organism's body, and further, that these forces, though each essential, are variously major and minor. For example, "at no time is the need for vitamin E great" (Chaney and Ahlborn 1934: 205). Human nutrition is therefore constituted in part, but fundamentally, as an assemblage of energetic abeyances and sites of potential energy.

Because the abeyance in human nutrition and the catalysis of the vitamins constitute a differential, vitamins are sites of a potential for connection within nutrition. And this site of abeyance within the metabolism of the animal body must be understood as part of what is

relevant when thinking about the organizations in which the animal body becomes involved. Connections can be made between the organism and the environment because of this difference in energy. A capacity for connection is therefore fundamental to nutrition. The connections that can be made in this regard are not determined in any one organism. It is not that humans must eat blueberries to attain their vitamin C requirements. In fact, because the vitamins are found in various sources, the need for vitamins is the capacity to make potential connections. These energetic abeyances and differentials are the potential to connect up with and become part of potentially various, different assemblages of environments and organisms. We can attain our vitamin C from blueberries (my preferred route), pine needle tea or fresh seal meat, to name only a few potential sources for this potency. They are the possibility of possibility for the organism and they are found in nutrition. One essential function of nutrition, we might say then, is this possibility for possibility.

At this point, we must bear in mind the organic character of the vitamins. The significance of the organic is that the vitamins are essential to maintaining the organism's organization. Openness to difference, therefore, is an organizing principle of the organism whose nutrition is constituted in part by the vitamins. That the body organizes itself along the lines of this differential is not a passive being carried, but rather is the very activity of life. The organism, therefore, is not a being, as we might encounter in our everyday lives, but a becoming, "a difference driven process" (DeLanda 1999: 31). The possibility to make new and different connections within nutritive processes is one way in which a self-organizing system is established and maintained. The differential in intensity as the "onset of self-organization" engenders the capacity to institute new norms, to make new connections. A principle of life is the openness and capacity to make various and different connections. The

vitamins, therefore, are sites of the body's aptitude to adjust to its environment. Nutritional success for an organism is constituted as a "capacity to be affected, an active drifting" (Doyle 2003: 131). For example, Vitamin A is a vitamin that is protective because it aids in the development of mucous membranes that are essential for maintaining the organism against invasion by bacteria. Vitamin A is not toxic to bacterial infection, but without sufficient vitamin A, the organism does not have sufficient protection against bacterial infection (Peterson et.al 1943: 185). "When this vitamine is excluded from the diet, they cease to thrive and become highly susceptible to bacteria" (Sansum 1925: 64). Vitamin A, therefore, enables organisms to introduce new norms as needed to make their way through the bacteria of their environment.

We might conceive of a life where any lack in the organism was simply accounted for by the addition of some substance from the environment, thus completing the organism along the lines of some blueprint of determination. However, given the evidence of the vitamins, this is not the way we must understand life. Life is that which meets the challenge of the environment with excess and inventiveness. Because the body of the organism is constituted with the potential energy of the vitamins, the organism accesses energy that is in excess of its maintenance needs. Recall that catalysts stimulate reactions to go forward which have all the necessary reactions but which do not proceed or proceed only too slowly. Bataille tells us that luxury, or an excess of energy, is the fundamental problem facing life on the planet (Bataille 1988). Organisms use the excess energy available from the sun to grow, increase their complexity and reproduce themselves. Excess is the prompt for organization, it is the logic of life. The excess of the vitamins organizes nutrition. Here I want to focus on the relation between this economy of excess, Canguilhem's notion of normativity and health and illness.

Canguilhem argues that life is lived in normativity and that successful living is precisely the ability to accommodate differences in the environment, rather than a strict adherence to a set of norms, the conditions of which may not always be present. Accordingly, Canguilhem defines health as the ability to institute new norms. Because they function at the on-set of an organism's organization, vitamins are a crux of normativity. Vitamins enable the organism to live in normativity, rather than with strict norms.

When organisms are not receiving the catalysis provided by the vitamins, they do not have available to them the planet's excess energy to expend on increased organization or complexity. Without access to the potential energy of the vitamins, the economy of life does become an economy of circulation, rather than an economy of excess and the organism inevitably fails and dies. We have examples of this in ophthalmia, an eye disease brought about by a lack of Vitamin A in the diet. "In the absence of adequate amounts of vitamin A the epithelial cells of the mucous membranes lose their ability to secrete normally and, as a consequence, become dry and hardened" (Peterson et.al. 1943: 185). Without the excess necessary to life the circulation of reactants and products does not increase in complexity but leads to a simple building up, a hardening that inevitably leads to failure. "When the tear glands cease to secrete, the eyes are not cleansed properly, they become noticeably irritated and infection ensues" (Peterson et.al. 1943: 185). Without the capacity to live in normativity, the organism inevitably institutes strict norms for the circulation of its metabolic reactants and products and in this process we see the breakdown of the organism and life. "The formation of intercellular substances which tend to acquire properties resembling a gel seems to be under the influence of vitamin C. In the absence of adequate amounts of the vitamin these intercellular substances, which normally act as a cementing medium, are not formed properly or may even be resorbed" (Peterson et.al. 1943: 224). The resorbtion of intercellular

materials indicates reactions are not those of increasing complexity and enabling normative movement through the environment, intercellular material is not taken up in a self-organizing system, but rather the equivalencies of a relationship of linear causality, cause and effect rather than the catalytic function provided by the 'accessory' (excess) food factor.

The point concerning health is one where it is particularly easy to see and understand the similarities in work of Deleuze and Canguilhem. According to Deleuze, "health . . . means, concisely, the actual measurable capacity to form new relations" (Deleuze 1987: 82). In the differential formed across the boundary of the organism by the abeyance in the organism's metabolism and the potential energy of the vitamins in the environment, we see created the possibility for life lived in normativity. The vitamins are material instances of the capacity for connection. Vitamins are the possibility of possibility within biology. This is not the possibility for the inevitable within life. "Not all species are equally sensitive to deprivation of the various types of essential factors thus far described" (Mendel 1916: 70). This possibility of possibility, Deleuze argues, is the "mode of what is", and he calls this the virtual. Vitamins are biological traffic which constitute the virtual. Deleuze's 'virtual' acquires increased significance when it is placed beside Canguilhem's notion of normativity, as above. The virtual is the capacity to make various connections, the possibility of possibility within life. It is the virtual aspects of life, as Deleuze would have them, therefore, that make normativity at all possible. The institution of new norms is dependent upon the capacity, the potential within the organism to make new, potentially various and different connections. Such a capacity is found in organisms whose nutrition is constituted in part by the vitamins.

In the next section I will argue that precisely because the capacity to live in normativity is a capacity to create the possibility of possibility, it is also a site at which the

power of life is grasped for political ends. It is because the vitamins are an instance of life's normative aptitude, its capacity for connection, that nutrition became bio-political.

Becoming Bio-Political

Knowledge about vitamins within nutritional science changed understandings that people had about human nutritional needs. This shift in the understanding of human physiology precipitated by the discovery of vitamins necessitated an equally profound re-problematization of human nutrition at an international level. In 1925 and 1928 the delegates of Yugoslavia and France respectively, brought the problem of nutrition to the General Assembly of the League of Nations. Yugoslavia's appeal is interesting insofar as it practically replicates some of the statements made by the scientists concerning their rats in the feeding experiments. That is, Yugoslavia complained to the League that its population had sufficient food for sustenance and yet the population was starving and, given its overall ill health, apparently lacked the nutrients it needed. The response to these appeals, a publication in 1935, was slow and not directly helpful. At the Sixteenth Ordinary Assembly of the League of Nations, in 1936, several nations including Argentina, the UK, Chile, New Zealand, Austria, Sweden, Australia, Canada, Denmark, Poland, Italy and France suggested that nutrition be put on the agenda of the current assembly and that the Second Committee consider the matter.

I argue that the emergence of the vitamins into international consideration of human nutrition precipitated a consideration of the becoming, or the development perhaps, of bio-politics⁶² on a world-wide scale. This consideration was precipitated in two ways. First, the simple insertion of an un-attributable factor into the bio-political landscape meant that the

⁶² This is not the Foucauldian notion of bio-politics as I do not mean to invoke specifically a science of population. Rather I mean to characterize more generally those processes which concern biology and politics in a similar vein as the phrase bio-technology.

future was highlighted as unknown. Although the effect of the vitamins was understood increasingly within bio-chemistry and physiology, their effects in social life and on international politics were entirely unknown. Particularly among the scientists involved in the international discussions of the newer knowledge of nutrition it was felt that its effects upon international bio-politics could not be accounted for; “so far reaching may be the consequence ... that I am convinced we need to proceed with caution” (Mixed Committee on the Problem of Nutrition 1936: 2).

Second, the significance of the vitamins as a possibility for possibility meant that the unforeseen was of potential. The newer knowledge of nutrition hosted a sensitivity to difference and an openness to the future. The emergence of the future from this unknown factor was not going to be a matter of simple cause and effect. Vitamins would not merely be a new addition to the organization any one nation's food economy, but the insertion of a qualitative difference. The unforeseen would effect new organizations of the political. That is, vitamins maintained their organic function and the effect of their insertion into the organization of international bio-politics would be of a non-linear causality. “[Food supply] is highly complex with its involvement of factors and interests in agriculture, commerce, industry and nutrition. Here, as in other domains, there is opportunity for an interplay of science and the arts, of experience and investigation. To attempt to foretell the future seems more like an act of ill-considered rashness than a keen intellectual venture” (Mendel 1916: 59). There was real potential here. In fact, I think it feasible to argue that the international political importance given to the newer knowledge of nutrition by the League of Nations stems precisely from a very real comprehension of the potential of the vitamins: “Their political efficacy lies in this: that they actually work to produce differences we could not ever have imagined” (Shaviro 1995: 53).

The emergence of vitamins catches up many processes in a movement of becoming. The spring-like action, or the 'rising column of air' of the vitamins emerges within the social and political organization of human nutrition. With the emergence of the vitamins, there is created within bio-political processes a moment of potential, the possibility of possibility. "Once again heads of state, chemists, biologists . . . and industrialists find themselves caught up in a single uncertain story mixing biology and society" (Latour 1993: 2). There is a moment of abeyance in these processes as government bodies and institutions assess the possibility for possibility before the vitamins catalyze a profound reorganization of food economies, a seizing of the possibility of possibility in human nutrition. This differential is reminiscent of the dynamics creating waves and wind and I suggest that the emergence of the vitamins effects a sharp intake of breath within social and political processes concerned with human nutrition.

I want to dwell for a moment on this sharp intake of breath within social and political processes. It is not so much that political actors, presidents, prime ministers, ministers and deputy ministers, collectively gasped when they heard about the discovery of these 'accessory food factors'. Rather, the emergence of the vitamins effected in the assemblages of food economies and human nutrition a moment of abeyance. I mean to highlight here a synthesis of heterogeneity, to point to the workings of an assemblage which functions without distinction between orders. The capacity for connections in human nutrition emerged within bio-political structures and effected a moment of abeyance during which time the possibility for possibility was assessed.

Of course, because it was a member state, the bio-political structures in Canada were also effected by the introduction of the vitamins to international politics by the League of Nations. In November 1935 the Mixed Committee on Nutrition of the League of Nations

undertook two actions that were important for Canada. First, it published and released the London Report of its Technical Committee, Physiological Bases of Nutrition which was drawn up by E. Mellanby and E.V. McCollum. It was a report that addressed the Health Organization's desire that "human nutrition requirements should be determined by scientific methods" (Mixed Committee on the Problem of Nutrition 1936: 2). Second, in November 1935, the League sent out a series of questions to member states concerning nutrition and nutrition programs in their countries. The League had set about studying physiological standards and compiling information from its member states, which it might compare to these scientific investigations (LAC RG77, Box 182, File 386-7-6, pt 5, Mixed Committee on the Problem of Nutrition, Second Session).

Within the workings of the Canadian government, this dwelling in abeyance and the assessment of the limits and possibilities made possible by the connective capacity of the vitamins consisted of extensive research into the current eating habits of the nation's population, the constitution of the nation's food, and contemporary food distribution patterns. Following the international discussions of the newer knowledge of nutrition in the 1930s, Canadian government nutrition research and publications consisted of attempts to account for social relations in the country according to the organization of food and diet. The steering committee of the Canadian Council on Nutrition, at its meeting of February 19 1938, under an agenda item entitled, "What are the most urgently needed types of information in Canada?" decided that, "tables showing the composition of Canadian foods were most necessary" (NAC RG 17 Volume 3641 File N-5 (24-12)). There appeared studies and publications such as, An Economic Study of the Consumption of Milk and Cream in Certain Urban and Rural Districts of Canada, Consumption of Milk and Cream in Vancouver, The Wholesale Marketing of Fresh Fruit and Vegetables in the City of Toronto,

The Marketing of Perishable Farm Products in Ottawa, and surveys of consumption patterns in Edmonton and Halifax. Subsequent to this government research work, it was concluded by members of the Canadian Council on Nutrition that, “the populations studied have similar deficiencies of protective foods and also indicate that one of the important means of correcting this condition is through education” (NAC RG17 Volume 3120 File 59 part 3, Letter from Wodehouse, Department of Health and Welfare to Barton, Deputy Minister of Agriculture, 3 June 1941).

The insertion of the vitamins’ capacity for connection into the bio-politics at the time effected an “itinerary of disaggregation” within bio-political practices, a kind of undoing of previously formed connections in order to sustain the capacity for connection entailed by the vitamins (Doyle 2003: 6). With the emergence of the vitamins there is inserted into social and bio-political relations a problem of organization. With the emergence of the vitamins we have a moment in which the relations and the limits of the relations of human nutrition are no longer what they were. If we look across the top of the rising column of air we see created a smooth space. Relations of food production, distribution and consumption are deterritorialized.

Living in Liminality and Seizing the Virtual

If the matter of significance here was the animal body as bounded by the skin, then we would conceive of the abeyance in metabolism as a need, a lack, which could only be filled according to a blueprint of the species. The constitution of animal life then would be about identity, leaving little room for any sociology of the body, or any understanding of the bricolage of human nutrition. However, if we understand relations of force to constitute a body we can see the abeyance in metabolism and the propulsion of the vitamins, and not the skin, as the constitution of the animal body. This understanding of the profound significance

of potential energy enables us to move away from a notion that forces of power and knowledge merely impress upon biology, or corporeality, whose only power is a necessary resistance. Such an understanding is of the significance of the positivity of transformation to the life of the organism and thus leaves room for difference and therefore a sociology of the body.

In her book Liminal Lives, Susan Squier considers the re-making of the lines of life through bio-medicine and invokes the anthropological term 'liminality' as that which is not of one state and yet is not of another, but is in-between. It is tempting, especially when thinking about bio-medicine, to argue that only intervention by human culture, language and power will constitute any life as bricolage. Squier's point (briefly and without sufficient justice) is that the forms of life emergent from new forms of bio-medicine are neither entirely natural, nor entirely cultural. For example, the genome project is not possible without the natural product of DNA. I argue that the vitamins are a material that constitutes life as liminal, which means that life continues, is enabled through this liminality. Normativity makes the liminal possible in human life (the this and that constitution of life). It is because healthy life is normative that life can be liminal. It is at the point of the vitamins' dynamism, their function as potential in biology that life can be constituted in liminality and as a bricolage. The dynamism of the vitamins means that life is constituted as a kind of fabric, a weave of different assemblages. The vitamins mean that there exists the possibility of new planes of immanence for life, new planes of connection for life. This is of crucial importance as these are the sites, the planes where the social enters, where the social and the biological encounter one another and life scatters.

Vitamins are an openness, but they are also simply *the capacity* for connection, they are not the *necessity* for *particular* connections. Because the vitamins and our nutritional need

for the vitamins provide an interface surface, our bodies can become engaged in projects that are discursive and institutional. A vitamin subject is only constituted through transversal links between the plane of immanence created by the vitamins and modes of power and knowledge. That is, vitamins are necessarily also an opening onto bio-power, enabling “political control over the fundamentals of life” (Livingston, 2006: 142). Further, vitamins will work in different ways in different cases. I will develop this consideration further in the Conclusion.

We can return to the League of Nations for examples. The first meeting of the Mixed Committee on the Problem of Nutrition at the League of Nations, from which Canada was absent, took place February 10-16 1936. At this meeting were presented the responses of member countries to the questions asked by the League concerning nutrition and nutrition programs. Also at this meeting a motion submitted by M. Queille and M. Gauthier was passed urging that committees on nutrition be set up by member state governments with a view to ascertaining information on: already existent centres of study and research, actions desirable, actions already taken, as well as what should be done to improve future nutrition and to endeavour to ascertain the effects on the nation's food supply of these food rules. At a meeting, two days later, in an aide-memoire concerning its work to date, the Mixed Committee stated that it would be the responsibility of experts within each country “pro-actively” to elaborate “a variety of adjustments and alternatives of food stuffs” for their nation, based upon the scientific standard of the report of the Technical Committee, the “Physiological Bases of Nutrition” (LAC RG 29 Vol 937 File 386-7-6 pt 1, Mixed Committee on the Problem of Nutrition Aide Memoire 14 February 1936). We see here that the ‘committees on nutrition ...set up by governments’ are charged with the elaboration of nutrition programs based on a scientific standard. Member states are

asked to seize the potential of the vitamins for their national politics. Nutrition becomes biopolitical.

With the emergence of vitamins into Canadian government discourse, there arose attempts to re-organize social relations along the lines of food economies that emerge with new understandings of nutrition. In one Canadian government publication, Canning Fruits and Vegetables by Edith L. Elliot, we learn that, “Our present day knowledge of nutrition, and particularly of the value of minerals, vitamins and roughage in our daily diet, leads us to give Fruits and Vegetables their true place in the menu” (Elliott 1934: 3). That the vitamins now (in 1934) occupied their “true” place in the menu points to a process wherein changes in the social order were necessitated by the vitamins, but only in order to manipulate the vitamins’ capacity for connection. These changes were a biological imperative insofar as they were the capacity for manipulation for political ends.

We saw above that the vitamins do not necessitate any particular connection. The “true” place of vitamins in the menu cannot be considered to be a politically or socially neutral truth, not given the extent of the investigation within Canada to determine the ways in which Canadians could organize their diets. The vitamins necessitate a shift in social order, but only insofar as one is considered desirable. And yet, the shift is desirable because it is a biological imperative for improving health and well-being. The new life forms that emerge, therefore, grow up in a refrain of human and material agencies.

Such a refrain is apparent in a drawn out discussion in the Canadian Council on Nutrition in late 1941 concerning the fortification of bread with vitamin B. The discussion is worth quoting at length because within the various considerations undertaken we see the emergence of a vitamin subject from the back and forth of the vital function of the vitamins and various human agencies, including scientists, policy makers, farmers, millers and bakers.

Under an agenda item

Flour and Bread (not for the Press)

Dr. Wodehouse pointed out for the private information of the Council members that various people had been trying to encourage millers to produce a flour containing more of the B vitamins. Regulations are now prepared which are likely to demand 360 I.U. of B1 per lb. of flour, or even 400 I.U. Bread from this flour will contain about 200 I.U. per lb. Regulations for non white flours and breads, specifying 550 I.U. of B1 per lb. of flour, giving about 200 I.U. per lb. of bread were also included. Under existing regulations synthetic vitamins are really not permissible in flour and bread.

Dr. Hopper asked if the new bread is a 'good' or an 'excellent' source of vitamin B one, under the vitamin regulations.

Dr. Morrell said that this depends on what is a 'reasonable daily intake', which is not yet decided. At 400 I.U. per lb. of flour a daily intake of 8 oz of flour would make in an 'excellent' source.

Dr. McHenry mentioned that at the present time breads containing much more of the B vitamins were being made by adding germ or using high vitamin yeast or by other methods. He would prefer that these regulations did not go through.

Miss Bell said that we should get rid of the idea that white bread is desirable. Whole grain bread consumption should be encouraged.

Dr. McHenry pointed out that using whole grain bread will increase the iron intake too.

Miss Willard thought that we should emphasize the use of 'brown' bread. There was quite a bit of discussion on the types of brown bread. Some of the brown bread available is no good at all, just fixed up for the public.

Dr. Wodehouse pointed out that if the flour comes up to 55 I.U. per lb. It would be Canada approved.

Dr. Cook asked if reinforcement with high vitamin yeast is permitted, since it is high only in B one and not in the B complex.

Dr. Wodehouse thought that bakers will give up the expense of such yeast if they can get the flour.

Dr. McFarlane raised the question – do these regulations make any real contribution to the needs shown by the surveys.

Dr. McHenry thought it would mean only 25-50 I.U. of B one a day, which he considered worth nothing at all.

Dr. Young said this might be considered a negligible contribution to the problem.

Dr. Wodehouse mentioned that, after all, oatmeal every day would give more than a pound of bread, and there are other sources. But every small increase in consumption of the B vitamins will help the total in a definite manner. 25 to 50 I.U. daily could not be disregarded along with the improved intakes of other foods containing B vitamins, which can be advocated in the educational campaign.

Miss Alexander asked if bread consumption goes in an inverse ration to income.

Dr. Young and Dr. Hopper mentioned studies showing that it is about the same for all incomes.

Dr. McHenry said that lunches put up bread consumption, as by school children, etc. Considerable further discussion took place on general topics relative to flour and bread" (LAC RG 29 Box 3465 Binder 1, Minutes Canadian Council on Nutrition Meetings, 28 November 1941).

Just like the rats' running wheel within the experimental systems, it is impossible to tell where the material agency of the vitamins begins and ends and where the work of various human agencies begin and end. The differential in the plane of immanence is a catalyst insofar as it garners its potential by winding together the social and the natural.

Despite inevitable improvement in health, the scene is far from idyllic. Particularly in the discussions concerning the fortification of bread with Vitamin B in Canada, we see that new life forms emerge from the seizing of the capacity for connections in the vitamins for political ends. The initial interest in Canadian politics in the fortification of bread with vitamin B arose during WWII in an effort to aid the civilian population in dealing with the stress of the war. The Canadian Public Health Association (CPHA) at its annual meeting in Winnipeg, September 19-21 1940 passed a resolution in which it urged the Dominion Government to take action to ensure that Canadians were getting a sufficient supply of vitamin B in their bread and to ensure that this supply of vitamin B was derived from wheat germ, rather than from the addition of any one of the known components of the vitamin B complex. The CPHA resolution declared that "such action is urgent in view of the necessity both of maintaining national health under the special stress of the times and of preventing the possible exploitation of this important new knowledge of the field of nutrition" (LAC RG 29 Vol. 959, File 387-91- pt 10, Resolutions, Canadian Public Health Association 19-21 September 1940). Certainly population health would benefit from an increase in the supply of vitamin B available, and yet this biological capacity for connection was grasped for the political end of Canada's participation in the war. The normativity that marks the health of

the organism also renders it bio-social. Nutrition is bio-political due to the capacity to grasp human health at the level of the virtual, the body's capacity to make various, different and new connections, through the vitamins.

This kind of grasping of the capacity for connections occurred in industry and agricultural practices as well. For example, it was discovered that injecting cattle with vitamin C, rather than feeding it to them orally, could increase the rate at which the vitamin was synthesized by the animal and thus increase the animal's rate of reproduction. Such a discovery was a boon for the dairy industry, which requires a cow to be pregnant for her to produce milk. "The amount [of vitamin C] synthesized or the rapidity with which the synthesis is carried out may not always be great enough to supply all the physiological needs of the animal this fact has been strikingly demonstrated with the last year or two by the discovery that injections of vitamin C are of marked value in overcoming various breeding difficulties of cattle that have long been a source of loss to the dairy industry" (Peterson et.al. 1943: 224).

Embedded Durations

The emergence of the vitamins into world political order through the League of Nations is best understood with an eye to temporality. There are two reasons why an account of the difference made to bio-social and bio-political relations by the vitamins must be an account of temporality. The first is that, as we have seen, the vitamins are sites of the organism's becoming. The vitamin subject, as constituted in bio-political arrangements, is one grasped in its becoming, its capacity to connect, its openness to possibility. As we have seen in the examples above, with the discovery of vitamins and the seizing of their potential in bio-political arrangements, life acquires "novel capacities"; "new capacities for subjection are installed ... by novelty" (Doyle 2003: 10). Capacities are installed and so we find a

subject in process, a becoming vitamin subject. A good deal of social and political re-organising can and does turn on the capture of different life-forms in their becoming. Second, our accounts must be of temporality because it is the vitamins as novelty that effected these new capacities for connection within bio-social life. The integration of the newly discovered vitamins in the world food economy was effectively a hosting of the future and a precipitation of novel bio-political and social organizations.

An account that asserts the importance of temporality will do justice to the emergence of the vitamins not only because, as we saw above, the becoming of bio-politics is highlighted in this instance, but also because the effect of vitamins is to change the life and time of the bio-political. To account for the effect that the discovery of vitamins had on the social world and on bio-politics, it is necessary to think the new and the old, as well as their engagement. It is insufficient to consider the new bio-political arrangements emerging with the discovery of vitamins as a different inscribing onto bodies of the political. Given what has been said of the liminality of life above, in fact we can see that a discussion of this kind of inscription or impression and resistance is relatively futile. If human nature and human culture are bricolage at this site it is because of the embedding of various durations into one another. At issue are the intersections of becomings, for example, the becoming of the body, the becoming of the nation, the becoming of motherhood and the becoming of the dairy industry. Questions of social order are precipitated with the discovery of the vitamins but only because of the immanent possibilities within life, that which is beyond the social order of identity and difference. What is significant is the taking up of the becoming of those categories of identity and difference, rather than the re-arrangement in the social structure of identities and difference.

New relations are established through the embedding of the different durations of old relations. We see, for example, that life span extends with the discovery of the vitamins and an extension of the time within which it is possible to have babies. These two newly emergent durations within social life will profoundly effect social relations. Simultaneously we see the sudden death of different social orders, such as the organization of diet on the basis of meat and potatoes. Life does not move to different geographical locations (at least this does not exhaust the process). Rather, life's scattering is effected by a submersion of what was contemporary into the past and an eruption of the untimely.

Because what is at stake is the potential of human nutrition for bio-politics, we see in Canadian government documents the explicit consideration of the possibilities for organization. These necessarily come in statements regarding the development of the Canadian state and its population and an awareness of the passage of time as a constitutive force for the life of the nation. This awareness emerges from the vertigo of interaction with the vitamins' catalytic function. Out of this awareness come explicit statements of the nation's becoming.

In Department of Agriculture pamphlet Healthful Meals at Low-Cost, Edith L. Elliott and Laura Pepper tell us,

At no time in the history of our country has wise expenditure of money been more necessary than it is today. Every woman who is faced with the problem of providing low cost meals must remember that the health of her family is of first importance. She should, therefore, know what foods are needful for proper health and growth so that she may select these essentials from lowest cost products and in this way secure a balanced diet giving the most possible nutrition.

Adjustments may be necessary as prices vary as to locality. If prices are lower or more money is available the daily amount of milk should be increased. More fresh fruits and green and yellow vegetables should be added. An increase in the amount of meat and eggs should be made. More money might be allowed for other groceries (Elliott and Pepper 1934: 1-2).

Of course there are nuances and subtleties to consider within the notion of "nation",

as Pepper would have it. My point here is simply to highlight that with the emergence of vitamins into international bio-politics, the processes of becoming are of primary importance. There is an embedding of the time here, but it is not simply a time laid out on a linear line, but a duration. We see that it is a duration insofar as it appeals to the nation and its history. With the appeal to the nation, the process is not one of a simple linear sequence of events, but refers to the time of processes of change, of constitution and development. The emergence of the vitamins is not simply another event along the linear sequence of time, but rather participates in the development of the nation. The development of the nation, its becoming, is now constituted by the embedding of the new into the duration, or the history, of the nation.

Temporality and duration must be negotiated in the emergence of new social forms.

In The Canadian Mother's Book 1938, we read,

Our good health depends largely on having enough vitamins in our daily food. This is very important for the mother to know and to do. We have been eating vitamins all our lives without knowing it, but most of us have not been eating enough of all four vitamins every day. Eat these foods first (MacMurchy 1938: 16-17).

This is an apparently new manner in which to think about the organization of one's diet. The organization of new social relations is an explicit negotiation of temporality and duration. There is an attempt to allay fears that it is a novel concept to organize one's diet around the intake of vitamins. There is an appeal to familiar social orders and a sense that re-organizations of social relations around these new economies of food are not actually new, but merely a more concentrated form of the old. It is through an appeal to the old, a pulling through time of past practices into the present, endurance of old practices, that new social relations are constituted. The emergence of the new in this instance requires a melting of the past into the present. Duration, therefore, is clearly an integral component shaping emerging

social relations. Indeed, in Canadian records of the League of Nations. Mixed Committee Meeting On the Problem of Nutrition, April 12th to 17th 1937, the concern is expressed by a Canadian member of the committee that nutritional information convey an understanding that the aim of nutritional information, “was not to supersede the family, but, on the contrary, to make the family function” (NAC RG 77, Series A-1, Volume 165, File 28-1-7 “Mixed Committee of League on Problems of Nutrition”, Minutes of tenth meeting). There is an apparent concern here that with the emergence of the vitamins we will see the final expiration of family life. Such is the significance of duration to thinking social relations. Even if the family was not at its end, it is significant to see the power of duration in shaping concerns regarding family life.

We saw this kind of melting of the past into the present above, in the quote regarding the injection of vitamin C for cattle. It was said that this injection would serve to compensate the dairy industry for the loss it had been suffering in the previous couple of years. However, I speculate that such a loss could only be constituted as such in hindsight with the boon of injection vitamin C. The past endures and becomes relevant to the significance of the vitamin C injection to the cattle industry. In this instance then the duration of the past and the emergence of the untimely in the form of the vitamins merge and through this embedding of duration social relations endure through the present.

Conclusion

The League of Nations, of course did not request that only Canada investigate the nutritional status of its population. These projects were undertaken worldwide. I suggest therefore that we can see emerging from the League of Nations process a kind of topological map of embedded durations. Vitamins were an organic catalyst in international bio-politics just as they are in the environment of various organisms. That is, they provided potential

with which to organize in each instance in which they were encountered. Nutrition became bio-political through the seizing of life's mutation aptitude in the form of the vitamins. Therefore, accounts of these new life forms are necessarily accounts of the becoming bio-political of nutrition.

In this chapter I have argued, once again, for the significance to the social world of material agency beyond human agency. Indeed, I could argue that human agency, in this chapter is of no avail without the material agency of the vitamins. The material agency of the vitamins enables an extension of human agency to life's very mutational aptitude, a power which, in social theories of embodiment to this point, would normally be left to the minds of men and their purposes. Vitamins, I have argued, mark a differential in intensity, the line of which constitutes the body of the organism. It is because this line of differential intensity constitutes the organism that we can live in normativity and because of normativity that we live and thrive in liminality. The power of the vitamins' differential is that bio-political specifications can keep us in health and well-being. That which is beyond human agency therefore enables human agency.

Nutrition, as a bio-political specification, cannot simply be thrown off as perhaps might be possible were it simply an impression of the social onto the corporeal. Rather our biology enables these bio-political specifications to constitute nutrition, which is necessary to our vitality, health and well-being. Therefore, while I am attributing power and effect to material agency outside the realm of human agency, such an analysis reveals the extent of the grasp on life had by the political and the social world. With an analysis of the power of becoming of the vitamins, in our bio-political analysis we see insidious political power grasp life at its very potential to be healthy.

Conclusion

Through this dissertation I have argued that, because organisms live in normativity, in knowledge seeking, or knowledge production, there is opened up a space of creative and productive potential. I suggest that this space can be understood as a zone of virtual connections. Knowledge production occurs through a form of realization in which connections can be made variously between the knower and the known depending on the potential created in the both the phenomenon encountered by the scientists and the technique by which scientists encounter that phenomenon. That is, new objects emerge out of a blur of natural and cultural (in this instance, scientific) phenomena, which is enabled by the space or zone of potential that opens up in knowledge production. The zone of virtual connections enables the development of a blur of the natural and the cultural, which is productive of new knowledge and new objects. This transformative “phenomeno-technic” encounter is the site of new knowledge and objects. Therefore, understanding this space of the virtual is of central importance in understanding the production of new knowledge and new objects, processes of change and transformation. Within the tradition that this work is situated, a focus on the transformative and productive nature of phenomeno-technic encounter in science derives from attempts to take sufficient account of temporality, the working of time and the fact of our immersion in temporality. In this work, the virtual, transformative and productive character of the phenomeno-technic encounter in the production of new scientific knowledge and new scientific objects is highlighted in the theme of the ‘as yet unknown’ as it runs throughout the history of the vitamins.

The ‘as yet unknown’ as I have presented it here highlights the role of temporality in processes of knowledge production and the emergence of new objects. However, my focus has not been to theorize time and temporality, though certainly I have considered aspects of

time and temporality for my account. My focus, rather, has been on the ontology of the unknown, and the character of the encounter with the unknown. The significance of the 'as yet' for me has not been that it allows access to think about temporality but that it lends itself to an understanding of the world as not already laid-out through space, but emergent in time. I have pointed to aspects of the ontology and the epistemology of both a knower and a known immersed in time. That is, an unknown that is as yet unknown is emergent through time and therefore of a different ontological status than an unknown that is already present in space but waiting to be uncovered. I have argued that the ontology of the unknown is characteristically virtual, blurred, transformative and productive of new knowledge and new objects. Thus, I have taken the opportunity of the apparent disruption in the smooth flow of time, of the 'as yet unknown' to consider the ontology of the encounter of the realization of scientific objects and I have pointed to instances in which the virtual, transformative and productive character of the encounter are highlighted.

Mine has been a concentration on the ontology and the epistemology that enables a blurring of the natural and the cultural, of the world 'out there' and the scientific apparatuses for knowing the world. The aspects of the blur that I take account of include the press of the world outside experience, or narrative sequence, the virtual, problems and affect. I have outlined the technical apparatus by which the scientists encountered the 'as yet unknown', both unwittingly and on purpose. I have outlined the modes by which the object was known and in so doing have outlined the object known through its increasing realization. I have detailed the problem as it developed because it is at the site of the problem that the phenomenon is provoked. And I have outlined the transformations in the field necessitated and enabled by the emergence of this problem. Thus, in my first chapter I considered the difference made to physiology at the time by F.G. Hopkins' concept of 'accessory food

factors'. I showed that within the Bernardian framework of physiology at the turn of the century it was not possible to conceive of human physiology as either consisting in processes of regulation or of requiring some component from the external environment to initiate and drive these processes of regulation. The concept of accessory food factors rendered the problem of human nutrition differently.

My point, for the most part, has been to elucidate these different virtual and productive aspects, ontological and epistemological, of the phenomeno-technic encounter with the 'as yet unknown'. In the second chapter I reviewed three instances in which phenomena were provoked at the limit of knowledge as problems. Vitamins emerged within a context of research concerned to understand the various qualitative differences of purified amino acids. The more purified the amino acids the more trouble the diet presented to the animals. From their own failed experiments of this nature, Osborne and Mendel noted that some small portion of protein free milk was sufficient to restore their animals to good health and allow Osborne and Mendel to conclude their protein feeding experiments. From the observation that some portion of protein free milk was necessary to their rats' health, Osborne and Mendel concluded that there existed in their protein free milk some 'as yet unknown' food factor essential to growth, maintenance and reproduction in animals. I considered the integration by Osborne and Mendel of an 'as yet unknown' food factor into the technical apparatus of their experiments as an ideal site at which to notice the work of the unknown in scientific experiments.

I then reviewed two instances in which two scientists were convinced by the data to date that the conclusions reached left an unknown to be explored and the scientists suggested that the data pointed to the importance of re-formulating the problem rather than refining wrong results. After pointing to the work of the unknown in Osborne and Mendel's

experiments I set out to explore the materiality of the unknown as it impressed the two scientists, Cornelius Pekelharing and Casimir Funk, who each, in reviewing their contemporary fields of nutritional science, argued that their problems needed to be reformulated. In this chapter I both insisted upon the materiality of the unknown and sought to elaborate, following Canguilhem, the “internal characteristics and conditions” of the problems of this particular life science at the time (Canguilhem 1991; 2000; Deleuze 1994: 323). In the years during which scientists worked but did not isolate a vitamin substance, contributions to the field did not take the form of the addition of new facts to the store of knowledge. I suggested that it was through various configurations of the problem that scientists contributed to the field.

In my third chapter I told the tale of Elmer V. McCollum’s first rat feeding experiments, which were performed in relative obscurity in the basement of the Wisconsin Experimental Station. I suggested that McCollum’s flight to the basement is exemplary of normativity in thought and intellectual life and suggest that the story is a good place to see the power of and the work and role of the virtual connections opened in processes of knowledge production. I told the tale in order to highlight the role of affect and the virtual as conditions which are equal players with social, political and historical forces and to suggest that it is in the coming together of affect, the virtual and already existent conditions that transformation of existence occurs. My effort in this chapter therefore was to highlight instances of the virtual, how it is produced and how it manifests itself, how it affects the conditions in which has arisen and the transformative power it effects.

Part of what made the work on vitamins unique within the field of bio-chemistry at the time was that because no substance had been isolated, chemistry experiments had to proceed by way of animal feeding experiments. In my account of these experiments I have

followed historians of science who argue that scientific experiments are not only reproductive in character but are differentially reproductive in character. These scholars argue that a successful scientific experiment is one whose function exceeds those of its technical constituents. That is, successful scientific experiments use knowledge and technical apparatuses to solicit the unknown. In Chapter Four therefore, I analyzed the historical record for the practices by which scientists solicited novelty and aimed at producing the future (Pickering 1995; Rheinberger 1992; 1993; 1995; 2001). I argued that the massively repetitive and rhythmic nature of the rat feeding experiments, as well as the centrality of practices of touch and sensibility enabled the suspension of the boundaries of human and material agency, allowing the experiments to solicit the unknown and host the scientists' encounter with the unknown.

In Chapter Five I reviewed two arguments which occurred between scientists in the field at the time over the formulation of the problem of the vitamins. That the problem, as a problem, is of sufficient significance that it provokes public disputes, speaks to its power for the production of new knowledge and new scientific objects. First, I suggest that F.G. Hopkins instigated a public argument with Osborne and Mendel over their failure to problematize in their work their 'as yet unknown' factor. Hopkins was not angry about any mistake Osborne and Mendel had made in the work they had undertaken, Hopkins' point was to suggest that Osborne and Mendel had undertaken the wrong work. Hopkins disciplined Osborne and Mendel for the *kind* of work they did as it effectively painted the quality of the field as different from what Hopkins had been insisting in the literature was the state of the field. After this, Osborne and Mendel did in fact take up the problem of their as yet unknown food factor, as Hopkins hoped they would and very quickly determined that there was in fact some organic catalyst found in butter fat which was essential to growth and

reproduction in animals. Not quickly enough, however, as E.V. McCollum published his exceptionally similar results just three weeks prior to Osborne and Mendel's publication. The ensuing argument between the two laboratories is frequently cited as having been precipitated as a priority dispute over the discovery of vitamins. However, I suggested that given that the two papers do nothing exceptionally different than all the work that had gone before, in fact, the fight precipitated the 'discovery of vitamins'.

Chapter Six introduces a rather sharp turn in focus to consider the bio-politics of nutrition necessitated and enabled by the emergence of the vitamins. Such is the result of my having noticed that the explanation of the vitamins and the work of the vitamins in the biological theory bore an "uncanny resemblance to various already extant theories of postmodern economy, society and culture" (Livingston, 2006: 157). The chapter is an attempt to demonstrate this resemblance. The chapter does not claim to deal exhaustively with the elaboration of nutritional subjects enabled once the problem of the vitamins is introduced to international bio-political formations. I have used the formulation of the problem of the vitamins as I understand it from the bio-chemistry at the time to suggest how this problem is elaborated with already existent bio-political formations. The formulation of the problem of the vitamins by the scientists while they encountered the unknown endures and is "still at stake for us" in our constitution as nutritional subjects (Livingston 2006: 111). I suggested that the "betweenness" of vitamins is significant (Livingston 2006: 113). They are sites that allow for the moving together of differences, a circulation of power. I have suggested that the vitamins function as a site of the virtual in health and nutrition and have argued that their integration into bio-political formations is a moment when insidious iniquitous power relations grasp life's normative potential. Just as I suggested that the

experimental systems are a space where the world is a blur of the natural and the cultural, the vitamins also are a space where the world is a blur of the natural and cultural.

My Own 'As Yet Unknown'

I have found the resonance between social theory and vitamin science compelling throughout the course of the dissertation and yet could not elaborate upon it to my own satisfaction. Hence, this resonance still works in my intellectual life as the 'press of the world', and 'as yet unknown'. Allow me to explore briefly here some of my encounters with this as yet unknown. I suggest that the vitamins are one of the places in which we live in a social world constituted in part by open-ended connections in space and time. Because we live with the problem of the vitamins such as it is, we live in a world that requires an account of open-ended connections in space and time. I suggest that contemporary social theory is productive in the ways that it is in part because the world for which it accounts is one in which our nutrition and our nutritional selves are organized around vitamins. That is, we live in a world in which, as I have suggested in the last chapter, the connectivity of the vitamins has been highly productive for the organization of social and political life. It is logical, therefore, that social theories that emphasize the power of the possibility of possibility, of the capacity for various, different and multiple connections are effective explanations of social life.

I showed in the final chapter the extent to which the "interminglings of bodies in society" were organized with the vitamins (Deleuze 1987: 90). Deleuze suggests that "what regulates the obligatory, necessary or permitted interminglings of bodies is above all an alimentary regime and a sexual regime" (Deleuze 1987: 90). And the vitamins have been assigned to and seized for the purposes of regulating the organization of bodies within an alimentary regime. What Deleuze would call the collective assemblage is organized with

vitamins enabling some of the symbioses that exist. If social theory of late deploys so productively notions of affect, becoming and the virtual, it might well be because these are the characteristics of the connections within the alimentary regime that we find ourselves. The possibility of possibility is limited by the alimentary regime, by the seizure of these points of possibility in our diet by insidious political forces. This seizure of the virtual in the alimentary regime therefore functions as an outside limit to the possibilities for social life. It is in writing up against what we are currently that social theorists work with these notions of the virtual and affect. It is in the normative process of knowledge production and new object creation that social theorists deploy these concepts. We are hemmed in by the seizure of the vitamins' possibility of possibility by insidious political forces. Appeals to the creative and productive power of the virtual aspects of the world are re-problematizations of our organizational capacities.

It is difficult and frustrating to think about this resonance between the vitamins and social theory in terms of what the resonance *means*. I have found, however, that it is productive to consider what this resonance *does*. In a wide-ranging and ambitious essay entitled, "How to Talk about the Body?: The Normative Dimension of Science Studies", Bruno Latour suggests that having a body means having the capacity to be affected. Thus, he suggests, the body is not a substance or an entity, but a set of capacities, perhaps enabled or enhanced through things like language, tools and equipment or acquired knowledge, skills and practices – what Latour calls mediations. Latour suggests that a body comes into being when it learns how to be affected and suggests further, "the more mediations, the better when acquiring a body" (Latour 2004: 211). Differences in capacity are layered, according to Latour and he suggests naming these layers of difference, "articulation" (Latour 2004: 209). Thus, "an articulate subject is someone who learns to be affected by others" (Latour 2004:

210). Latour begins the article with the example of developing a 'nose', as described in Alain Corbain's intellectual history of scent. Using this example, Latour asks what is articulated in an articulate subject and concludes that it cannot be 'words' because, for example, the bottles of scent used in learning to be a nose are not words, and they cannot be 'things' because if what is articulated is a "substance defined by primary qualities" the affected body, and therefore articulation, will disappear (Latour 2004: 212). Latour suggests that 'propositions' are what is articulated. Because of the character of articulated propositions, (articulated: material, artificial, mediated; propositions: obstinate, no definitive authority, negotiable) they are perfectly suited for "progressive composition of a common world" (Latour 2004: 212). Articulated propositions can compose a world "at once solid, interpreted, controversial and meaningful" (Latour: 2004: 212). In sum, Latour suggests, "the world is made of propositions, and ... knowledge is conceived of as articulation" (Latour 2004: 214).

Latour suggests that to theorize the body in this way is to re-consider what we, as social scientists, grant to science. "If science is left to its own devices to define by itself – without further scrutiny or court of appeal – what the body is made up of, as if it pertained to the realm of primary qualities, it will be impossible for other versions of what a body is to be sustained. Thus it will be impossible for something like a democracy to be sustained when bio-power has taken over" (Latour 2004: 224). Against ceding the ground of the body to physiology and medicine, Latour argues we must see all disciplines as having articulating roles. Following Latour, therefore, I suggest that with the acquired knowledge of the science of vitamins, I have added a mediation to the body of social theory. I have added yet another "artificial setting" with which the body and the body of social theory might be affected.

With our understanding of vitamins we inhabit an enriched world of the working of the virtual in the everyday and the power of the political in everyday as well. Thus, it is not

that I have discovered that ‘everything is virtual’ or that ‘it’s all about the virtual’. Rather in generalizing I have allowed an increase in articulated propositions. In understanding the vitamins as I have I have made connections between widely different phenomena – food biochemistry and social theory. My point has not been that, now that we know how our organism moves through the world because of its nutritional needs, we do not need an account from social theory. Neither do I suggest that the sciences or the politics of the vitamins take something away from our natural state as nutritional organisms. Rather, our understanding of each of these differences is heightened. I have found in the vitamins a capacity to be affected given to us by science. And by generalizing between the science of vitamins and social science, I have increased the number of versions of the same phenomena. I do not withdraw possibilities of articulation from the world. The resonance between contemporary social theory and vitamin science that I see, my generalization, allows me to see the vitamins work in different situations and therefore even to increase the possibility for the articulation of propositions concerning the vitamins. That is to say, this generalization makes possible new and different articulations of the proposition of the vitamins. I am adding the vitamins to the arsenal of “bio-counterpower” (Latour 2004: 227). According to Latour, following Isabelle Stengers, this generalization is ‘good science’ insofar as it ensures that the composition of the world “has not been prematurely simplified” (Latour 2004: 223). Thus, future research will need to include further examination of the propositions through which we have been articulated as nutritional subjects.

Directions for Future Research

The productivity of the vitamins for organizing social and political life is apparent in the development of the vitamin concept through World War I. Prior to World War I, vitamin research was undertaken primarily in America, despite Hopkins’ initial contribution

at Cambridge. Throughout the dissertation I have suggested that by 1913 the skeleton of the vitamin concept was in place. By that time two American laboratories had determined first, that vitamins were supplementary to the basic constituents of a diet, second that the vitamins performed a catalytic function in metabolism and third that the vitamins were organic in nature. Equally important, the 'discovery of the organic' had rendered the vitamins as a problem of regulatory function. With the outbreak of World War I, vitamin research became a focus of activity for British bio-chemistry. Within this context, vitamins' regulatory function was conceived of as providing harmony, or balance, to nutritive metabolism and the concept developed into a Hermes figure. Accessory food factor research was taken up in a significant way in the UK in 1914 when the British government gave high political priority to questions of food supply and the quality of military and civilian diet (Kamminga 1998: 83).

In a 1914 monograph entitled, The Fundamental Basis of Nutrition, Graham Lusk stated, "It has thus far been shown that vitamins produce a *harmonious interaction* between the materials in the food and their host" (Lusk 1914: 44). Vitamins are vitamins because they do the work of establishing harmonious relations within the specific theoretical discourse of balance, harmonies and economies.

Again, Lusk, "[V]itamins are essential to the *harmonious correlation* of the nutritive function of animals. . . . '*food hormones*' would be exactly descriptive of the nature of these substances" (Lusk 1917: 378). Within nutritional science at the time, vitamins do the work of providing balance. Anything that does this work falls into the category of vitamins. Long before nutritional scientists know the chemical composition of any individual vitamin, they know that vitamins are present where the work of balance in diet is being done.

“It now appears that a proper diet for growth or maintenance must contain not only protein, fat, carbohydrate, and salts, but also some substances existing in natural foods, in very minute quantities, which are *absolutely essential to the harmonious fulfilment of the life process*” (Lusk 1917: 362). Lusk stated,

Olive oil when taken alone is a poor fat in a nutritive sense, but when taken with green leaves, these furnish that one of the peculiar accessory substances, commonly known as vitamins, which is present most abundantly in butter-fat, and gives to butter-fat and to the fat in whole milk its dominant nutritive value. Here then is the message of economy in diet. One can safely refer to such a diet as a balanced diet (Lusk 1918: 8-9).

Lusk give us the significance of a balanced diet, “Therefore, let us cry, ‘Eat corn bread and save the wheat for France, the home of Lafayette!’” (Lusk 1918: 10-11). Also in 1918, in The Newer Knowledge of Nutrition: The Use of Food for the preservation of Vitality and Health E.V. McCollum tells us, “The need for knowledge of nutrition was never greater than at the present time when so large a part of the energies of the people of Europe and America are absorbed in the activities of war” (Lusk 1918: v). Thinking about nutrition as a balanced diet has an affinity with thinking about the national position within international relations, and the organization of the national population so that the nation might be so positioned within international relations.

Vitamins were conceptualized as something that establishes and maintains relations, within a balanced diet. They were the food messenger during metabolism, they were that which enabled people to balance the food in our diet and they were that which will enabled governments to balance the food in a nation. Vitamins were conceived as providing inclination to relations in the body, the nation and the world, within specific material conditions. In turn, it is precisely because they were conceptualized as providing inclination to relations that they were perfectly suited to the work of shifting social relations around economies of food.

The Medical Research Committee⁶³ (MRC), the Royal Society and the Lister Institute established themselves as hubs for networks of scientists working on problems of food supply and healthy diet (Kamminga 1998: 83). While the social and political relations at the time informed the establishment of scientific research networks, the scientific importance of accessory food factor research in turn informed the government policy concerning food research and accessory food factors became an increasingly central component of Britain's war-time scientific research into food supply and nutrition (Kamminga 1998: 84). I suggest that within the context of government sponsored war research, British researchers saw the problem of accessory food factors expand suddenly. No longer could the problem be simply the physiological function of the accessory food factors in individual animals. Rather, the problem became one of population health and food supply as means to meet various international and national nutritional needs.

Beginning in the first world war, scientists' research efforts were taken up with large scale feeding experiments with human populations, both adult and children. The experiments therefore were a crossing place for scientific interests, national concerns for military and civilian diet and the disseminating power of the vitamins, which made for unique and new encounters with the 'as yet unknown'.

Therefore, research along these lines will require a three-fold problematic. The first aspect is to determine the changing architecture of the vitamin concept and its three components: accessory, organic and catalytic. Second will be to determine the social conditions for the changes to the vitamin concept. That is, one might ask after the conceptual and epistemological practices and the course of critical correction by which the vitamin problem was formulated and by which the architecture of the concept was

⁶³ The Medical Research Committee only became the Medical Research Council after WWI.

elaborated. And third, the influence of the scientists' experimental practices on the changing nature of the architecture of the component concepts. One might consider the social and scientific practices the scientists used to generate and support productive encounters with the 'as yet unknown'.

There were a range of social conditions that affected the development of British vitamin science during world war one, such as laboratory practices and techniques, military dietary needs, population health concerns and international food distribution responsibilities and policies. My future research therefore will investigate how scientists made sense of changing social conditions and integrated these conditions into their scientific milieu. Thus, one might explore the materiality of experimental systems in relation to the ways that they engaged the scientists' sense of the problem, in order to consider how experimental practices influenced the emergent vitamin concept. For instance, experimental practices support the scientists' encounter with the unknown and facilitate the elaboration of the vitamin problem. Further, exploring the epistemological techniques these researchers adopted offers an opportunity to develop an account of their encounter with the unknown and thereby to expose the changing character of the problem of the vitamins and the increasingly refined vitamin concept. Vitamins constitute an encounter that enable an investigation of the re-composition of life forces in terms of national and international food distribution policy. They are a prime site for reading together the data of the biological sciences and the social sciences to understand the problems and concepts informing contemporary nutrition and its attendant life forms. Following Deleuze, I suggest that the vitamins offer a problematic that can be investigated historical, philosophically and scientifically.

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