PLACING GENETIC RESOURCES CONSERVATION ON SOCIAL FOUNDATIONS: AN ANALYSIS OF HISTORY AND IDEOLOGY IN THE NATIONAL PLANT GERMPLASM SYSTEM

by

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Without the National Plant Germplasm System (NPGS), crop failures would be a common occurrence in American agriculture. Despite this key role, the System perpetually lacks funding and resources. Although technical solutions have made more efficient use of limited resources, they also mask a deterioration of the nation's genetic resource base. The solution does not lie with technology, but with how the science interfaces with social and political processes where resource questions are decided. At this junction of the scientific and political, the crucial questions are what a genetic resource is and how we know it needs conserving. To address those questions I analyze the historical development of the NPGS institutions, the ideological heritage they received from biology, and the technical literature on sampling genetic resources. This analysis supports the contention that the needs of industrial agriculture have become more important than conservation within the NPGS. A reversal is clearly needed.

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CHAPTER I

INTRODUCTION

Researchers in the natural sciences are influenced by non-scientific pressures in two basic ways: first, since research requires long-term commitments and institutions, scientists are supported economically by public or private funds; second, since both public and private institutions have goals for their research dollars, scientists must always make the case that their particular research is relevant to these goals. These influences always exist and they can rise to the point of controlling or distorting the scientific process.

I contend that researchers in the natural sciences must learn to apply social theories to understand, and to effectively manage, the non-scientific pressures that impact their work. This argument has been made before and its most powerful articulations rely principally on analyses of ideology, power, and economics to show how scientists have played into the interests of militarism, racism, capitalism, and other exercises of power. I refer to those analyses here for the theoretical insight they provide. Unfortunately, the response that they have generated in the scientific community has been polemical. Because of the contentious history of this topic, I do not rely on theoretical and historical arguments alone. Instead I take those works as a theoretical background and use them to analyze a contemporary problem in the science and practice of genetic resources conservation.

Genetic resources conservation is a scientific field concerned with the conservation of the diversity of life. This diversity is crucial to all agriculture, from the production of food, to biofuels, and fibers. It will only become more important to humanity's future as fossil fuel resources are exhausted, or become uneconomical to extract. A *diversity* of genetic resources is required because it is the physical basis that allows biological systems to adapt to new environmental conditions. Without diversity the agriculture that we are dependent on could not survive.

The problem at issue in genetic resources conservation revolves around a pair of analytical mistakes. First, scientists conflate long term and short term goals in genetic resources conservation and use. The long-term goal of conserving genetic resources is to support agriculture in perpetuity, which requires sufficient genetic diversity to respond to changing environmental conditions and human needs. The short-term concern is to supply genetic resources to industrial agriculture, organized principally around the pursuit of profit, to improve crop yields and prevent failures. These short-term concerns explicitly exclude human nutritional and cultural needs. They are primarily related to the industrial use of crops. The long-term goal is comparatively more resource intensive than the short-term goal because, though both use the same techniques, long-term conservation must be more extensive in time, geographical considerations, and variety of crops conserved.

Although the two are superficially related, scientists working in genetic resources conservation have noted that short-term uses of genetic resources undermine the long-term uses. As early as 1970 (Frankel and Bennett) researchers described, with great concern, the process of agricultural development whereby diverse landraces—crops that consist of multiple, interbreeding populations—are replaced by genetically uniform modern varieties. Since current models for agricultural development place no limitations on the replacement of landraces by modern varieties, short-term agricultural development and long-term conservation are clearly irreconcilable. Despite this conflict of interest, the same institutions are responsible for both conservation goals. These institutions pursue both goals under the banner of long-term conservation but find their active advocates mainly in agribusiness. This situation has led to a pursuit of short-term use to the detriment of long-term use and conservation.

The second analytical mistake at issue is the use of a definition of genetic resources based on outdated genetics theory. Since the 1970s, a genetic resource has been defined as a gene; plants with novel genes are seen as genetic resources. This is a pervasive definition and it implies that genes function atomistically—they are assumed to be basic building blocks that produce additive effects. Under this definition the whole is just a sum of its parts. Applying this definition reduces the diversity of qualities associated with genes into a single quantity—the number of genes. According to this definition, collecting two novel genes is twice as good as collecting one novel gene. Far more expeditions have gone to collect wheat in Turkey than any other target, in any other geographical location. Nevertheless, under this atomistic definition of genetic resources, it still appears appropriate to send expeditions to Turkey to collect wheat. If the qualitative aspects of genes are irrelevant and only the quantity of genes matters then 1) any amount of genetic material conserved for almost any length of time qualifies as conservation; and 2) the narrow pool of genetic resources needed for short-term conservation goals cannot be distinguished from the far broader pool of genetic resources needed for long-term goals.

This outdated theory encourages the conflation of short-term and long-term conservation goals. In the parent discipline of population genetics, many important developments have taken place in defining genetic resources since the 1970s. Significantly, most agriculturally useful traits such as yield are known to be the result of combinations of multiple traits, which are themselves the result of complex combinations of genes. Even many traits that are not as complex are known to be controlled by interactions between cellular processes and DNA, not simply the result of genes. Successfully conserving combinations of genes will require an increase in the amount of plant material in storage. This increase would clearly differentiate the amounts of resources required for short-term and long-term goals, and illustrate the inadequacy of the current system.

The problems of conflated conservation goals and outdated definitions of genetic resources have a logical resolution. If the definition of genetic resources were to encompass the complexity we know exists, we would need to collect, and store, that complexity in addition to individual useful genes. In this case, the projected resource requirements of long-term conservation would clearly rise. On the other hand, the resource requirements of short-term genetic resources use would remain the same. This is borne out by the fact that, under the current system, plant breeders are successfully increasing yields and staying ahead of pathogens. Such a distinction translated into terms of resource requirements would destabilize the relationship that has been established between short-term and long-term goals of genetic resources conservation. If the two sets of goals could no longer be conflated on solid scientific foundations, they would also be difficult to conflate in practice. The resource needs of long-term conservation would have to be considered on their own. That this scenario has not taken place, despite being the logical result of applying contemporary genetics to genetic resources conservation, shows that non-scientific influences are in play.

Ultimately, I argue that in the natural sciences, researchers must learn to apply social theories to understand, and manage, non-scientific pressures, if they are to control the development and application of their work. To understand the nature of these influences, I turn to two sets of social theories. The first, based on the work of Kuhn and Sarewitz, provides a framework for looking at science as a social activity. The second, based on the works of Marx, Levins, Lewontin, and Merchant, describes in detail the social processes that shape science.

In discussing how science develops there is a tendency to focus on the logical succession of ideas. This tendency is dangerous to scientists interested in controlling their work, since it hides the political and polemical struggles that occur whenever new ideas are presented to the scientific community and the public. Kuhn's major contribution is to articulate the distinction between how scientific ideas are currently logically related, and how they developed historically. As an example, the logical development of classical genetics can be explained by the discoveries of two people: Darwin and Mendel. Darwin contributed the understanding of how organisms are selected for by natural selection. This raises the question of how traits are transmitted between generations of organisms. Mendel's work addressed that issue by showing how traits could move as units between organisms. The succession of these two discoveries opened up the study of classical genetics—the study of how trait transmission between generations occurs and affects the constitution of organisms. From a historical perspective, this logical explanation is inaccurate since there is evidence that Mendel was not interested in the problem of evolution by natural selection at all. Since he was interested in an alternative mechanism of evolution, situating Mendel's work as one of the foundations of classical genetics is inaccurate. This inaccuracy also does a disservice to scientists because it masks the fact that, historically, there have been substantive disagreements over ideas now accepted as common wisdom. Kuhn's argument is that to understand how science develops we should examine both the logical fit of ideas, and the conditions under which scientists engage in their work. Disregarding the latter in favor of the former gives an inaccurate view of the political danger involved in developing new ideas.

In the contest over new ideas, some scientists have historically been very savvy in acquiring resources for their work. Particular fields, such as biotechnology, have been able to produce such compelling visions of the power of their research that they eclipsed multiple other disciplines—e.g., plant breeding, embryology. Sarewitz provides a framework for understanding this ideological process by likening it to a series of myths that scientists produce about their work. The clearest contemporary example that the predictions scientists produce about the power of their disciplines are myths is genetic engineering. Work in that field continues to be presented by scientists as both precise and predictable, and as the route to better crops and medicine. The first of these three is false since the field relies primarily on throughput—the ability to conduct thousands of trials—to find a successful experiment. The second must also be false since we do not know how most genes interact with each other and with cellular processes. Lacking that knowledge, even when we successfully manipulate a section of the genome, it is impossible to predict what the results will be. Finally, at least in agriculture, the predictions of improved crops have yet to materialize. Although genetic engineering's promise may be a myth, it is a very effective one. Despite producing few productive results, the science continues to have high standing within the academy and in the business world.

Both Kuhn and Sarewitz demonstrate how science is a social activity. Kuhn's focus is on the social activities—debate, laboratory work, publication, censure—that shape the historical development of scientific ideas. Sarewitz, on the other hand, focuses on how groups of scientists engage in building myths to create a space with the freedom and resources required for them to carry out their research. This first set of social theories, however, does not address in detail which social processes are most important in shaping science or how they function. I have already highlighted funding, and the arguments used to acquire funding, as social processes affecting science. Although there are more, I limit my discussion to funding and the rhetoric used to acquire funding for simplicity. I define these processes in detail in my method section. For insight into how these processes function, I turn to Marx, Levins, Lewontin, and Merchant.

According to Marx, intellectual production—such as science—within a society will be controlled by the class—people united by their position in the process of capital accumulation—that controls the capital. The basic argument is as follows: 1) science is a capital-intensive activity; 2) those who control capital can afford to support science; and 3) those who control capital will not support science that challenges them in a significant and sustained fashion. Therefore, although there might be some science which challenges the holders of capital, the vast majority will not do so in a significant way. This would be a straightforward theory two apply if the world were populated by the public, capitalists, and scientists, but it is not. Instead, scientific institutions—Universities, independent laboratories, the national laboratories—mediate the relationship between science and capital. Although the interaction is not direct, scientific institutions are under significant pressure from business, and that pressure is passed on to the scientists who these institutions support.

In addition to this indirect form of control, there are also issues stemming from the division of labor. Some science can be made a direct part of the production process. In that case, it functions as a part of capital accumulation. Levins and Lewontin argue that this integration of science and capital currently only happens partially, and that it can only happen when the science reaches the stage of generating predictable results. Because there are always unpredictable frontiers to any science, this integration can only be partial. The unpredictable science, even when it is necessary for the production process, is carried out by government institutions. In such situations, the class in control of capital is dependent upon state scientific institutions. However, the researchers in those institutions are not necessarily interested in supporting capital accumulation. Therefore, there is an ongoing contest over the control of state scientific institutions.

Marx, Levins, Lewontin are concerned primarily with issues of control of resources and institutions. These concerns leave out the struggle that scientists engage in to show the worthiness of their work to funding entities and society at large. Merchant shows that the rhetoric of a small group of people, in the right context, can drive a much larger ideological shift. Her argument is historical, and based primarily on the shift from an understanding of nature as organic and alive to an understanding of nature as mechanical and dead. Merchant argues that this shift was driven by a combination of three elements: 1) the language and imagery of a mechanical nature; 2) the convenience of such an image in the context of an explosion of extractive industries; and 3) the practical effectiveness of assuming that nature can be understood as a machine.

The fact that there are contests over ideology, division of labor, and economic control in the scientific establishment that supports contemporary agriculture, is best illustrated by Kloppenburg (2004, 101-129). In his discussion of hybrid seeds, he points out that it was public institutions that produced the first economically viable hybrid seed. Additionally public institutions, such as the University of Wisconsin, developed equipment for farmers to produce their own hybrid seed—and farmers successfully engaged in hybrid seed production which kept the price of hybrids low. This decentralized model of production meant hybrids were initially not an economic boon for seed companies.

Making hybrid seed the domain of unfettered capital was the result of ongoing political and economic pressure. By 1938, the seed industry began to advocate for a shift in public institutions towards fundamental research—that is away from competing with business. By 1941 the number of hybrids released by experiment stations dropped sharply as a result of such pressure. Finally, in the 1950s, lacking the support of public institutions, farmers disappeared from the business of producing hybrid seeds. In this same time period, seed companies following the principle of product differentiation, took publicly bred hybrids and illegally marketed them as their own, newly developed products. The result of these pressures is that yearly hybrid corn sale, which were around zero in 1934, rose to $70 million by 1944, and $6.4 billion by 1986 (Kloppenburg 2004, 101-126). The economic golden age of hybrid seed production was a product of both public science, and political pressure from seed companies.

Together the theorists I mention—Marx, Levins, Lewontin, and Merchant—provide a framework for understanding how social processes—at least those involving ideology and the institutional framework for science—affect science within, and beyond, the laboratory. Although these theoretical and historical arguments have been compelling to many people, this has not been the case within the natural sciences. For that reason I apply these theories to genetic resources conservation as a case study. I argue that the problems of conflating short-term with long-term goals, as well the problem of using outdated definitions of genetic resources, are caused by a combination of ideological and institutional pressures.

This case study approach allows me to address a contemporary conservation problem and demonstrate the importance of social theory in understanding how science is influenced by non-scientific pressures. More broadly this argument is applicable to the entire conservation field. Conservation cannot be about science and management alone. Scientists who do not deal with non-scientific pressures in conservation directly are liable to have their work at best ignored, and at worst manipulated to other ends. Since biological systems are likely to only increase in importance as oil resources are exhausted, conservation becomes more than an environmental issues, it becomes an issue of survival.

CHAPTER II

THEORETICAL FRAMEWORK

Science as a Social Practice

Using social theory to understand the natural sciences first requires defining science as a social activity. I rely on the work of Kuhn and Sarewitz to demonstrate how science can be understood as a social activity. Kuhn's argument is primarily historical whereas Sarewitz is interested in the social activities that scientists engage in to promote their work. Both argue that, to have an accurate view of how science is conducted, we need to think of it as a practice that people engage in, not as an intellectual process alone.

Thomas Kuhn is credited with first making the argument that science should be seen as a social activity. His primary insight was to turn historical analysis to science—a field that had, thus far, successfully escaped unscathed from such analysis:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure. (Kuhn 1970, 1)

Kuhn's initial insight was that histories of science had previously been based on the logical arrangement of well-established discoveries. Even when historical events suggested that some scientific achievements might not be related, they might be placed together because they were related logically. He derisively likens this type of history to the production of a tourist brochure and calls for a truly historical analysis.

The example of genetics can be used to demonstrate Kuhn's point. If one were to ask the textbooks, the answers about the beginnings of genetics are clear. The great luminaries are Darwin, who discovered evolution, Mendel, who discovered heredity, and Watson and Crick who discovered the molecular structure of DNA. Although the work of these men fits logically in a progression, there are at least two serious issues with that story.

First, it is very likely that Mendel was simply not interested in evolution (Bowler 1989a) when he carried out his work. Putting him into this progression therefore hides both Mendel's intent, and our knowledge of the work that Mendel felt he was contributing to. Second, though Watson and Crick may have written the paper that described the structure of DNA, it was Rosalind Franklin's crystallographic work that empirically demonstrated its existence. The theoretical paper would have been meaningless without her empirical work, but Franklin's name was never associated with the discovery. This demonstrates two additional issues. First, the logical story excludes Franklin because it is a story about concepts. Such a story ignores the practice of science and therefore gives a false picture of how knowledge is produced. Second, Franklin was also excluded from the story on grounds of her gender. This exclusion demonstrates the extent to which social norms can pervade science. Although creativity and the pursuit of novel discoveries are important parts of the scientific process, it is ideological and political conflicts that form the body of material scientific history. These conflicts must be understood to gain an accurate vision of why science has, and is, developing along its current trajectory. To understand them, Kuhn argues we must engage in historical analysis because purely logical connections between concepts hide important conflicts.

Perceiving science as a *social* activity implies that norms and dogmas exist in science—much as they exist in any other social activity. This perspective leads Kuhn to propose a cyclical model for scientific discovery with three phases: normal science, crisis, and revolution. During the 'normal science' period, most researchers work under the same paradigm and are primarily working out the details of an overall world view. Their choice of questions and experiments is based on the paradigm that they are working under. As these paradigm-dependent experiments are conducted, paradoxes that are inexplicable under the current paradigm tend to accumulate. Once the paradoxes accumulate to the point that they are obvious, they begin to wear down the credibility of the current paradigm, leading to what Kuhn terms a crisis. Ultimately, either the paradoxes are solved and the crisis avoided, or the crisis expands and opens up a creative space where a new paradigm can be suggested. If a better paradigm does come about, is compatible with current experiments, and explains some of the paradoxes, it can affect a scientific revolution or *gestalt* shift. Kuhn's theory can be easily critiqued for providing a totalizing and linear discourse on the fashion in which scientific discoveries occur. Nevertheless, it was an early work that made an important point: science is a social activity with norms and dogmas just like any other human practice.

Some of the dogmas that Kuhn mentions are especially relevant to questions of funding. To understand those dogmas, it is instructive to turn to Sarewitz (1996) who follows Kuhn's lead by conceptualizing science as a social activity, but focuses his attention on the myths scientists produce about how science interacts with society. Unlike Kuhn's proposed paradigms, these myths have survived multiple shifts in gestalt and, since World War II, have become part of the discourse used to secure ongoing funding for the natural sciences. Sarewitz identifies five myths:

1) Infinite Benefit - that more research will always produce more social (and individual) benefit;

2) Unfettered Research - that research is most productive when scientists simply pursue their curiosity in basic and applied research;

3) Accountability - that peer review and rigorous education in scientific practices and methods provide all the accountability necessary in the field;

4) Authoritativeness - that particular pieces of scientific understanding are solid, agreed-upon, and should not be questioned (i.e.- accurate DNA fingerprinting, gravity); and

5) The Endless Frontier - that research, basic or applied, in any particular field or direction will produce more social (and individual) benefit.

In Sarewitz's analysis, these myths are relevant to both scientists' survival and social welfare. For scientists, the myths are useful in creating a space where more expenses on science can be proposed and justified. Additionally, excess outside scrutiny is deflected by presumably rigorous internal mechanisms for accountability. The implication for society is that science is not always directed towards social welfare; often it is directed at military or socially unproductive applications. In this context, the perpetual rise in scientific expenditures can be completely socially unproductive.

Unproductive scientific expenditures can be illustrated using the example of health care. In the United States the biomedical sciences, particularly pharmaceuticals, tend to receive credit for our excellent health care. Sarewitz contends both that health care in the United States is not so excellent and that the biomedical sciences have not been responsible for it. The first part of the argument is straightforward: "The least affluent of the industrialized nations, such as Greece and Portugal, have basic public health indicators that equal or exceed those of the United States in areas such as life span, infant mortality, mortality of children under the age of five, and maternal mortality" (Sarewitz 1996, 143). Similarly, far from being dependent on biomedical technologies, the largest improvements in life expectancy were historically correlated with declines in pneumonia and tuberculosis; and not correlated with medical treatment. Many of the diseases which were later impacted by antibiotics and vaccines were in strong decline *prior* to the advent of these treatments (Sarewitz 1996, 149). Clearly, the massive resources that the United States have committed to the biosciences are not being socially productive.

Sarewitz's myths can be understood as the ideological tools that scientists deploy to strengthen the public position of their disciplines. Though he only mentions five common ones, historically there have been a wide variety, each applicable to a particular field. On the basic point of science as a social activity, Kuhn and Sarewitz concur. It is clearly a human practice with norms, dogmas, common assumptions, and the need to justify its existence. Additionally, Sarewitz points out how these norms and dogmas can be deployed to acquire resources. The second step in establishing how social theory can be used to address issues of control in the natural sciences, is to describe the social processes that affect scientific practice. To this end I rely on the works of Marx, Levins, Lewontin, and Merchant.

Social Processes Affecting Science

Scientific practice, considering the long-term nature of its goals, is a capital-intensive activity. For this reason two social processes are especially powerful in affecting scientific practice: 1) scientists are supported economically by public or private funds, meaning that what these institutions are willing to pay for affects research; and 2) since both public and private institutions have goals for their research dollars, scientists must always make the case that their particular research is the best for reaching these goals. These two processes function together to distort the shape that scientific practice might otherwise take.

In *The German Ideology,* Marx proposes a clear understanding of the relationship between science, or intellectual production, and availability of resources. In particular, he sees the production of ideas—whether through natural sciences or philosophy—as closely tied to the intellectual's class position. Those who produce knowledge relevant to the dominant class are, ultimately, better supported in their work.

> The ideas of the ruling class are in every epoch the ruling ideas: i.e., the class which is the ruling *material* force of society, is at the same time its ruling *intellectual* force...[insofar] as they rule as a class and determine the extent and compass of an epoch, it is self-evident that they do this in its whole range, hence among other things rule also as thinkers, as producers of ideas, and regulate the production and distribution of the ideas of their age. (Marx 1978, 172-173)

Marx's argument is not only that ruling class people—owners of capital—are the dominant intellectual force in society, but also that they influence the intellectual production of others. Ruling as producers of ideas also implies enlisting others—e.g., professionals, professors, teachers—to produce ideas sympathetic to your interests. This might happen directly or indirectly, through the provision of funding for institutes that apply certain paradigms. Initially, this picture of the relationship between class and intellectual production might seem too monolithic. However, Marx does not presuppose that the economic structure of society determines the mental production in the absolute. Instead he argues that divisions can develop and collapse within the group responsible for mental production and within their more passive followers (Marx 1978, 173). However hostile opposition may become between these groups, it is typically developed only to the extent that it does not destabilize the position of the dominant class. John Foster (2000) demonstrates the details of how this process of scientific discovery modulated by class concerns can develop.

Foster (2000, 21) begins the argument with a paradox in the work of Charles Darwin. Though Darwin recorded in his autobiography that he first conceptualized his understanding of evolution by competition in 1838 and elaborated it in writing by 1844, he left the work unpublished until 1859 (Foster 2000, 22). Based on Darwin's own notebooks, Foster argues that Darwin was both a "dedicated materialist" (Foster 2000,

22) in his scientific approach, and clearly aware of how heretical his materialism was. The argument for Darwin's materialism is that he did not invoke a higher force or ideology in his explanation of evolution. He limited himself to describing the progress generated by evolution in terms of random variation and natural selection. In contrast, the weakening but dominant perspective at the time was teleological. The world was though to be developing according to a divine plan and containing fixed species in a Chain of Being (Foster 2000, 22-23; Bowler 1989a, 59-63). Due to this instability mixed with dogmatic religion and backlash, this was a very powerful time for a theory like Darwin's:

> Materialism in Darwin's day was commonly associated in the public mind not only with atheism but also with the ideology of revolutionary France. There were laws on blasphemy and sedition acts aimed at radical freethinkers. Between 1837 and 1842 the newspapers were full of the notorious activities of the Chartists, Owenites, and others who espoused materialism in the cause of social reform. There were also radical materialists, particularly in medical circles, centered in London who embracing evolutionary ideas, but whose views were anathema to Darwin because of their extreme anti-church and anti-state character. (Foster 2000, 30)

In this environment Darwin, who did not believe that materialism and atheism were related, struggled to avoid giving weapons to those revolutionary elements. He did this by carefully crafting his position, holding off on the publication of his work on evolution, and publishing on the more controversial topic of the evolution of humans in a separate work in 1871.

Darwin was clearly aware of the class context of his scientific work, and he modulated his publication dates and narrative to avoid siding with his enemies. Because his ideas were a danger to his class position, Darwin did not force an intellectual issue that he was clearly interested in confronting. This narrative of Darwin's life therefore fits well into Marx's understanding of a thinker of the ruling class who manages his scientific activities so that they do not conflict excessively with the interests of his class. What Marx cannot describe is the shape that these interests have taken in recent, advanced capitalism in the United States. Contemporary science has become more closely integrated with commodity production than it was in Marx's day.

The biologists, Richard Levins and Richard Lewontin co-wrote *The Dialectical Biologist* (1985), which successfully updates the Marxist analysis of science. Their key contribution is to recognize that science has become progressively more commodified as capitalism has developed. This commodification has led to a division of labor where public institutions engage in research that is unpredictable and difficult to commercialize. On the other hand, private institutions follow the projects that are likely to lead to short-term profits. Levins and Lewontin use 18th century Europe as a basis for comparing current trends. In the 18th century, science could be considered an externality for the ruling class. Although it might be important at times, science was by no means predictable and there was no way to calculate what sort of returns might be achieved based on a particular investment. For example, though "Napoleon urged scientists to develop munitions, as well as a synthetic indigo dye to replace the imports from India", he certainly relied on neither as a critical resource that could be expected to influence his broader plans and chances for success (Levins and Lewontin 1985, 198). Although science has long been a component of military conflicts, it could not be expected to produce specific advances on a specific time scale. The beginnings of a change can be seen in the 19th century when "science enlarged its role as an externality of capitalist expansion. But science was not yet a commodity. Its application was still uncertain... its product still often an after-the-fact explanation of empirical innovations" (Levins and Lewontin 1985, 198). In the 19th century the process of scientific research could not reliably lead to new, useful discoveries. In the mid 20th century developed capitalist nations finally managed to commodify science in its current form. Currently even complex research, such as the development of new medicinal drugs, attracts steady investment. Although some of these ventures end in bankruptcy, the chances of a successful investment are known.

Although it is tempting to see science as an exception, Levins and Lewontin argue that it should be studied among the other social practices impacted by capitalism: "The commoditization of science... is not a unique transformation but a natural part of capitalist development. And we discuss it not to express outrage but to examine the consequences of this change for scientific activity" (Levins and Lewontin 1985, 199). Following Levins and Lewontin, I want to emphasize that the goal is also to examine the consequences of this integration for scientific activity, and how the effects of capital can be managed or resisted.

The move from science as a social activity to science as a commodified social activity was based in part on regularizing science so that it fit into the accumulation cycle. Although the path that the development of a new drug takes is difficult to predict, it is possible to predict that given a set regulatory framework and a particular market, a successful new drug can be marketed in 10 years for $10 million of research spending (arbitrary numbers). Under this situation it is possible to determine which fields of research can be profitable and science can become an investment (Levins and Lewontin 1985, 201). Science that is not profitable at a particular time is relegated to the public sector: "for example, in the development of new [plant] varieties in agriculture [State experiment stations] develop lines, which are then released to certified seed producers. The lines then become general property and are taken up by seed companies who 'fine tune' them and sell the results to farmers" (Levins and Lewontin 1985, 201). The 'fine tuning' process is predictable and therefore amenable to capital accumulation. This has had the effect of turning scientists into 'manpower' and rationalized their production (education). As 'manpower,' scientists are afflicted with narrow measures of the costs of production, they can be fired when outdated, and their behavior can be strictly controlled by managers (Levins and Lewontin 1985, 202). According to Levins and Lewontin, this transition is far from complete:

> The transitional condition of scientists as a stratum of professional intellectuals who are in the process of being incorporated into the structure of capitalism exacerbates the contradictions in their ideological positions and their social action. These vary from defiant assertions of individual responsibility and dissent, through cautious criticism, and studied indifference, to servile sycophancy; from elitist resistance to being bureaucratized and proletarianized to realistic or enthusiastic participation in the new order, to alliance with other alienated sectors in the struggle against capitalism. (Levins and Lewontin 1985, 204)

Levins and Lewontin argue we should avoid trying to 'look back' to how science used to function before commoditization. Instead we need to begin from the point at which history has left science and find the best way to use it to improve social welfare. The contribution of Marx, Levins, and Lewontin to studies of epistemology is to place science within the political-economic realm which has been forcefully driven by capitalist accumulation. In this view, intellectual production is necessarily coevolving with the historical and material context of a particular science practiced in a particular institutional framework. This literature gives us a view of science as a social activity, influenced by norms, dogmas, and capitalist accumulation.

Ideology in Social Processes

According to the works of Marx, and Levins, Lewontin, scientific practice is influenced by a series of social pressures. However, scientists themselves also shape how this interaction occurs through the creation of the myths Sarewitz discusses. Finally, scientists also influence this interaction through the way that they publicly articulate the importance of their work. This last interaction between scientific and social processes is explored in the work of Merchant (1979).

Merchant's work, *The Death of Nature* (1979), presents a broad argument related to the fundamental nature of modern scientific work. There she argues that around the time of the Industrial Revolution the imagery used in depicting nature changed. Initially it had been seen as a dynamic contest between the nurturing and wild sides of an organic nature. That view shifted to one where nature was a passive thing that could be mechanically manipulated—this was Merchant's concept of nature's death. Merchant argues that this shift was driven by a combination of three elements: 1) the language and imagery of a mechanical nature used by scientists such as Francis Bacon; 2) the convenience of such an image in the context of an explosion of extractive industries; and 3) the practical effectiveness of assuming that nature can be understood as a machine.

Francis Bacon's writings were a particularly rich text for Merchant, as he frequently imagined the scientific process to be an inquisition of female nature. She—nature—had secrets and his tools would help him find them (Merchant 1979, 168). Merchant also points out Bacon's references to physically binding, disrobing, and torturing nature. In all cases the common image is of nature as female, passive, and subject to mechanical manipulation. At the same time this conceptual shift was taking place, extractive industries, such as clearing forests for making charcoal, were expanding. With the shift from an organic to a mechanical perception of nature, Merchant argues that people, conveniently, lost the ethical injunctions that had been a part of previous worldviews. Finally, Merchant argues that this mechanical worldview became established because it was extremely powerful in the practical sense.

With little understanding about how nature actually worked, the selective application of simplifying concepts gave scientists the tools to exert enormous power:

> The brilliant achievement of mechanism as a world view was in reordering of reality around two fundamental constituents of human experience—order and power. Order was attained through an emphasis on the motion of indivisible parts subject to mathematical laws and the rejection of unpredictable animistic sources of change. Power was achieved through immediate active intervention in a secularized world. (Merchant 1979, 216)

The mechanical worldview assumes that all things, on some basic level, are the result of the motion of indivisible parts. The motions of those parts can be calculated and therefore understood and predicted. This understanding was applied to harness nature in pumps, mills, mines, and factories. It was effective at manipulating the world and therefore economically productive.

The broader ideology of nature was essentially transformed through human action—through both theoretical arguments and practical technologies of control. Although this change was also a broad social process, it was driven by particular capitalists and scientists. In terms of expanding the broader critique of social influences in scientific processes, Merchant's contribution is to point out just how important the ideological constructs of particular individuals are. Even now, we still use the genetic "engineering" trope to discuss biotechnology, and the systems paradigm to understand the interactions of organisms. The worldview of Francis Bacon is clearly visible in these examples despite the fact that the sciences themselves have evolved beyond simple mechanistic approaches. Nevertheless, there is more to it than men making ideas. First, these particular ideas—the mechanistic understanding of nature—have been useful to the practitioners of multiple scientific paradigms from physics, to chemistry, to molecular genetics. Those disciplines have in turn made themselves useful to states and other long-lasting institutions. Mechanistic philosophies have also allowed science to be somewhat predictable. If complexities are simply ignored and only main effects considered—common practice from drug development work to bids on contracts for new weapons systems—then, as Levins and Lewontin argue, science can become a part of the cycle of accumulation.

In sum, although the influence of small groups or individuals might last long and travel far, it is typically carried by forces beyond their influence. Those forces include scientific institutions in the business, academic, and government context; these institutions must address the interests of those in power to survive in the long-term and maintain. They are therefore very selective about *which* ideas they help propagate.

CHAPTER III

METHOD

Overview

Attempts to apply historical analysis and social theory to the natural sciences have not typically been well received. Even when articles in this area are written especially for interdisciplinary audiences—i.e., researchers in both the natural and the social sciences—there are often major miscommunications. For example in one article on global warming science, Demeritt (2001a) mentions that science might serve a social function. The respondent, Schneider (2001), takes this as an insinuation that scientists engage in their work with some sort of nefarious purpose. Demeritt, in his response (2001b), seems to be unable to address Schneider's misconception on this, and other major points. Although it is a step forward that the two authors agree that social issues ought to be considered, there is not even discussion as to how. That level of discussion, where only generalities are agreed one, is clearly unacceptable. My method is to rely on a combination of a theoretical approach with a simple case study to foreground the detailed application of the theory. The goal of this approach is to allow the reader to accept parts of the argument, and come away with a specific understanding of why he or she disagrees with other parts.

I begin my method with a description of how, in spite of powerful social influences, valid knowledge about the world can be produced. This is important for two reasons. First, it serves to show that considering social influences on science does not invalidate a scientific approach to knowledge production. This point is key to opening a debate with researchers in the natural sciences. Second, this approach to the production of knowledge is the explicit method I use for my study. Although I combine perspectives from a variety of disciplines, I do not claim to understand each discipline as a whole. Instead, I make my claims based on the validity of the individual arguments I present. After this first step, I move on to outline the aspects of social theory and genetic resources conservation that I consider in this thesis. Essentially, I only consider one intersection of the two topics. The scope of my work is limited to the United States National Plant Germplasm System (NPGS) which applies *ex situ* (out of context, gene-banking) conservation methods. Within that context, I consider most explicitly how theories of sampling have been have been shaped by social pressures at work within the NPGS. This discussion of limits is followed by a definition of explicit research questions and terms.

Producing Valid Knowledge

Donna Haraway (1991) in her essay entitled 'Situated Knowledge' put the goal of her quest for an epistemology succinctly: "I would like a doctrine of embodied objectivity that accommodates paradoxical and critical feminist science projects: feminist objectivity means quite simply *situated knowledges"* (Haraway 1991, 188). This goal is qualitatively different from the debate between objectivity and social construction. In her essay, Haraway goes on to critique the appropriation of the discussion on possible ways of knowing by objectivism on one side, and social construction on the other. She argues that this dualism offers no way out except, purportedly, moderation. Moderation between two extremes is an unsatisfactory solution as it perpetuates the understanding of science as diametrically opposed paired notions.[1]

Haraway's requirements for valid knowledge are that it should be a locatable and partial account of reality subject to ongoing re-evaluation. She argues that an account that is non-locatable must be questioned because every person writes from a particular context. If a knowledge claim is made by someone hiding their location or ignorant of it, the claim is not likely to be useful. When the location—institutional, class, cultural, economic, etc.—is unknown to the audience, understanding how the claim is partial, and how it is accurate, difficult. Using scientific communities as an example, understanding locations is also part of good practice since particular institutes, groups, or types of institutions carry out their research with specific concerns. The validity of drug safety studies in the United States has been questioned based on the industry practice of not releasing some negative studies. Understanding who carries out drug studies becomes a critical piece of information for evaluating their results. Following this argument, each location, if it is well identified, can be a valid place for producing knowledge despite any social pressures that might apply.

[1] See Harding (1991) for a detailed discussion of this analytical mistake.

Haraway also argues that an account that is totalizing must by necessity be false because no one person can successfully see the world from all places. No single scientist or group understands all subfields of their own discipline, meaning that the knowledge they produce is partial. Even a whole scientific community can rarely claim an understanding of the global state of knowledge within a discipline, again meaning that their knowledge is by definition only partially representative. This is part of the reason why journals and international conferences hold such important positions in most scientific fields—they allow people who carry partial knowledge to connect. These connections, between people with partial knowledge, are what allow for the construction of valid, and broad, worldviews.

Haraway's final requirement is that knowledge claims be subject to ongoing reevaluation. On the surface level, this is well in line with the requirements of standard scientific method, but her emphasis on this point is important. Knowledge that is aligned with powerful institutions has a penchant for turning into tropes that are simply deployed as true without consideration. For example, in biology, this is the case for the idea of genetic 'engineering,' where a mechanistic view of how genes determine organisms has become enshrined as an idea of great explanatory power. Despite being questioned within biology, the notion of genetic 'engineering' is regularly presented to the public as fully valid. Following this argument, it is important to revisit basic ideas so that, even if they are extremely convenient in the social sense, they do not escape reevaluation.

First, Haraway requires that knowledge be both partial and locatable. Second, she emphasized that knowledge must regularly be revaluated to assess its validity. The beauty of this approach is that, although it comes from a radical, theory-driven, feminist perspective, it can be reconciled with the best of the processes of normative science. Peer review is a limited acknowledgment of a single person's partiality. The need to replicate novel experiments similarly accepts an individual's partiality. In a chemical study one lab might carry different contamination from another and therefore produce different experiments. Each one produces knowledge valid to its place, but if the context is not understood, the knowledge produced might not be meaningful. The encouragement of creative competition is also an acceptance of partiality since one cannot know beforehand which researcher might be better positioned to make a particular discovery. Accepting these explicit limitations as ever-present does not mean rejecting the production of valid knowledge. Instead, accepting partiality allows for the production of valid knowledge despite the social pressures that continually affect it.[2] Much like peer review, the production of partial knowledge is a social process, as well as an intellectual one.

Limits, Scope and Definitions

My approach to method rests on Harway's theory but, to approach this complex topic, it is also helpful to restrict a number of other definitions. Genetic resources conservation is, in practice, a field where work is carried out in the absence of much necessary information and in the midst of national and corporate economic interests. As a result, it has been a political minefield of intellectual property rights, national sovereignty, piracy, and North-South conflict. Although these issues are broadly related

[2] For a more detailed discussion of the theory that describes how knowledge can be produced under Haraway's framework, see Appendix A.

to the one I take up here, I do not consider them explicitly. My goal is to use a specific aspect of genetic resources conservation to show how social theory can be fruitfully applied to a problem that is otherwise considered part of the natural sciences. Considering the many well-established problematic aspects of genetic resources conservation would only confuse this issue. To further simplify the topic, I start out from a normative position in regards to the state and goals of genetic resources conservation following Hawkes (2000):

1. Genetic resources are not being conserved to the extent necessary to assure long-term agricultural stability.
    a. The issue is greater for 'minor' crops than for 'major' ones such as wheat.
    b. Much of what *is* conserved is poorly described and therefore difficult to use.
    c. Much of what is conserved needs to be regenerated and the resources are lacking.
2. A key goal of genetic resources conservation is agricultural stability and improved production.
3. The genetic resource is initially defined broadly as 'plant diversity.'

Each of these points could be problematized but they are common working assumptions. Within these broad goals and definitions, theoretical approaches to sampling based on population genetics have always been applied in order to optimize the expenditure of resources between three categories when collecting plants: 1) seeds from a plant; 2) plants from a population; and 3) populations from a broader area. Theoretical approaches to sampling were not considered necessary when the goal was simply plant collections. When the paradigm shifted to conservation, the balance between mission and available resources shifted; following this shift underfunding became a key issue. I address the paradox in genetic resources conservation theory through two questions.

1. What ideological commitments from the parent discipline of biology encourage the maintenance of the paradox?
2. What aspects of the institutional or material framework for genetic resources conservation might perpetuate the paradox?

In this thesis, ideology refers to the ontological and epistemological commitments that scientists might make as a group and place beyond the reach of most ordinary investigations. For example, some Marxists have made ideological commitments to class as the primary category of analysis, the EPA has ideological commitments to atomistic risk assessment as a toxicological epistemology, and many economists are ideologically committed to free trade. In the work to improve genetic resources conservation, it is important to understand what ideological fossils genetic resources conservation might have to contend with. These are the various norms, dogmas, and paradigms that once may have been part of the state of the art, but are now outdated. Though they do not have scientific functions anymore, many are still important in the social arena. Ideology is important because it is often the level at which science is translated into public awareness and legislative action. It is important because it shapes the education of new scientists, and it can restrict which questions and which methods are deemed appropriate. All of these things have direct impacts on how the NPGS is funded and the ways that scientists argue for the importance for their work.

The material foundation of the NPGS is just as important since, underfunding is, essentially, an economic issue. Material foundations refer to everything from the work of the janitors at the National Seed Storage Laboratory (NSSL), to the liquid nitrogen tanks devoted to the storage of seeds there. The NPGS requires the cooperation of hundreds of people to succeed and much of the organization therefore occurs at the large institutional scale. Understanding the evolution of these institutions and how they are situated within the context of state and capital requires historical perspective and that is how I examine them here.

The questions I address are focused on ideology and institutions because these are two key, persistent phenomena that have coevolved to produce the current material foundation of genetic resources conservation. They also embody the social influences that I discussed in my theoretical framework. Briefly, the ideology of genetic resources conservation is relevant for three reasons: 1) it showcases the paradigms that research in the field is conducted under, 2) it operates to produce the myths—in Sarewitz's sense—that organize the importance and effectiveness of genetic resources conservation in the public mind, and 3) it is the product of particular ideas from biology that have been acquired by individuals and institutions promoting conservation. The institutions of genetic resources conservation are similarly important since they are the material foundations of any conservation work. After all, conservation does demand large, long-term, commitments of resources beyond the ability of any individual. Ideas themselves will not conserve anything.

Having made these considerations explicit, I combine them with a scientific analysis of the theoretical and technical literature on sampling genetic resources for conservation. The argument is iterative, first I consider the technical case study alone, then I consider the ideological and historical arguments, and return with those perspectives to the technical issues. This narrowly focused case study allows me to demonstrate in detail how social theory can be applied to a specific problem that would, otherwise, be considered to reside firmly in the realm of the natural sciences. It is also an important case study because the National Plant Germplasm System represents a serious investment in a mission-driven conservation institution by the United States government. The success of this institution will have a big impact on what the future of American agriculture looks like.

CHAPTER IV

CASE STUDY

The Field of Genetic Resources Conservation

Broadly speaking, my case study is the field of genetic resources conservation. This field exists at the nexus of plant breeding, biogeography, classical genetics, and molecular genetics. The global perspective in field currently called genetic resources conservation was developed first by De Candolle (1855)—his works are entitled *Rational Plant Geography* (1855), *Plant Geography* (1880), and *The Origin of Cultivated Plants* (1882) and each one is meant to evoke the sense of a global understanding of patterns of plant diversity. In the context of genetic conservation, when resources are discussed, they are not considered as the property of individual nations, but as the common heritage of humanity. This results in a focus on the areas of the globe that, historically, have had the most genetic diversity of a particular crop. For wheat this is Turkey; for corn this is Mexico. Even though corn was cultivated across the entire North American continent when Europeans arrived, the United States is not considered to have significant corn genetic resources. The National Plant Germplasm system is therefore always an outward-looking institution, dependent on collecting the genetic resources of other nations to complete its mission.

In addition to this global understanding of genetic resources, it is also important to understand how crucial institutions have always been to genetic resources conservation. After De Candolle, the next systematic attempt at developing a global understanding of the centers of origin and domestication of cultivated plants was carried out by Nikolai Ivanovich Vavilov (1887-1943). As a Soviet Academician and Director of the All-Union Institute of Plant Industry (VIR), Vavilov brought vast institutional resources to plant biogeography. His papers were based on his own research as well as hundreds of research expeditions that his institute organized and funded. As with De Candolle, Vavilov's work was thoroughly international in scale and attempted to create a global account of the origin of "cultivated crops" in general. Despite the vast resources available to him, Vavilov was unable to include locally important crops—there were simply too many.

These facts bring up two lasting features of genetic resources conservation. First, institutional resources have always been key to genetic resources conservation. Though Vavilov was an outstanding scientists he could never have accomplished his work in isolation. Second, within this global context, local variation, particularly when it is culturally foreign to Europe, is relegated to secondary importance. This decision is as much ideological as institutional. Part of it is a decision about what is worth eating—and whose genetic resources are worth keeping. Part of this decision is also based on knowledge about which foods are most important to the apparatus supporting the conservation institutions themselves. Vavilov, for example, focused on wheat simply because it was the major source of calories for the Soviet Union. Bread might not be very nutritious, but it can quell food riots.

The Mendelian revolution is the scientific event that distinguishes Vavilov from De Candolle and it began in 1900—the year that Mendel's work from 1865 was unearthed and reinterpreted. Prior theories of heredity postulated blending of parental traits, or the production of new species through hybridization. Following either mechanism, the only genetic resources that were useful to plant breeders were exotic crops. Neither mechanism made the wild plants related to crops useful to plant breeders. However, under the Mendelian framework, particular traits could be transferred from one plant to another, as long as sexual reproduction could be arranged between the two plants. Keeping this mechanism in mind, plant exploration could be broadened in scope to include wild relatives of cultivated plants that carried interesting traits—for example drought tolerance or disease resistance. The wild relatives could then be bred with current crops to produce new crops with the desired traits. This was the new understanding that justified the massive plant exploration missions launched by the VIR, and expansions of plant collecting missions from the United States. The reason for pursuing plant collections was economic and a scientific shift in paradigm was the driving force behind the expansion.

After World War II, the Green Revolution went international. At that time, plant explorers were the first to notice that the places they went to find useful traits were being, figuratively and literally, paved over in the process of modernizing agriculture. The primary source of genetic erosion was the replacement of locally developed varieties by inbred, uniform, "High Yielding Varieties"—plants that are now accurately characterized as High Response Varieties (HRV) since they only produce high yields when plied with pesticides and fertilizers in an agro-chemical package. Secondary causes of genetic erosion were the destruction of crop relatives by the expansion of fields to previously fallow areas, and the use of herbicides to destroy weedy crop relatives growing in the fields (Frankel and Hawkes 1975, 1; Hawkes 1983, 108-113, 131-130). In light of these developments, the response of plant collectors and biologists was more effective and extensive collection tied to gene banking. Following the established Mendelian and Darwinian understanding, this was a movement for *genetic* resources conservation.

This movement, led by plant collectors, established that economically valuable genetic resources were at risk due to the twin forces of economic development and the replacement of landraces by commercially produced varieties. The first organizing around this issue happened in the 1960s through the U.N. Food and Agriculture Organization (FAO) and its primary accomplishments were in establishing a unified discourse in genetic resources conservation. Hawkes (1983, 131) cites 1967 as the year that the phrase "genetic resources" was coined and the discipline of genetic conservation established as field. The initial international consensus on genetic conservation was focused entirely on plant exploration and gene banking—development was the process implicated with causing genetic erosion, but it was assumed to be an inevitable part of progress and therefore not questioned. With that move, the genetic conservation movement first established a key pattern: it would not address important economic patterns directly but only work around them.

This pattern has been destructive to the movement since it internalizes the conflict between conservation and development into the genetic resources conservation movement. Now the genetic resources conservation movement must deal with the fact that, although it might have a broadly supported mission, it may never be given the resources to accomplish that mission. In fact, this resource crisis is so intense that suggesting any approach other than gene banking has been politically dangerous. The gene-banking institutions are almost uniformly underfunded (Guarino, Rao, and Reid 1995, xiii) but they are the dominant institutions in the field. Advocacy of other methods has historically been seen as advocating a shift of funds; it has often triggered defensive polemics.

The result is that the works published on genetic conservation have been almost exclusively about *ex situ* (out-of-context; gene-banking) conservation as opposed to *in situ* (in-context; on-farm; bioreserves) conservation. *In situ* conservation only became appropriate fare for genetic conservation in the 1990s and the first extensive book on the topic was published by Nigel Maxted in 1997. The explosiveness of the topic can be felt in the highly diplomatic preface and introduction to that work (Maxted 1997, xix-14) which takes pains to distance itself from vulgar proponents of the *in situ* approach who "may not comprehend fully the scientific bases of genetic conservation" (Maxted 1997, xxi). The work also assures the reader that *ex situ* conservation—gene banking—is still a very good approach. Often the topic of *in situ* conservation is simply closed to discussion without serious consideration as in the following.

> Whereas some problems remain with the current gene-banking system, much goodwill has been nurtured by it. International collaboration in germplasm acquisition and exchange is a reality. We consider that efforts would be better spent on improving the current system with increased financial support and training opportunities [than criticizing it]. (Plucknett 1987, 189)

The critics referenced indirectly in these passages are the likes of Pat Roy Mooney, the Rural Advancement Foundation International, and other groups more interested in North-South issues than with advocating gene bank funding. Again economics is the elephant sitting at the table. Because this issue is so acute, questions of conservation efficiency are perpetually at the forefront of research in genetic resources.

The National Plant Germplasm System

In the United States this network of scientists and seed banks was labeled the National Plant Germplasm System (NPGS) in the 1970s and retains that name unofficially since being subsumed into the National Genetic Resources Program (NGRP) at the end of the 1990s. Since moving from the plant collections mission to the conservation mission, the constituent institutions of the NPGS have been in a perpetual state of underfunding. This is not due to a decrease in funding, but rather due to their vastly expanded mission. The gap has only become exacerbated since the program was broadened, a second time, to include non-plant life forms in the 1990s.

To address this lack of funding, researchers in genetic resources conservation have been working primarily on technical solutions that would make the storage and maintenance of genetic resources more efficient. To their credit, there have been amazing improvements in the state of the art. Especially powerful techniques are cryogenic preservation in liquid nitrogen, which holds the promise of storing some seeds indefinitely, and tissue culture, which allows for the storage of crops that produce recalcitrant seeds.[3] Nevertheless, there are parts of the NPGS operation that are not amenable to technical optimization. One example of such a process is seed regeneration—the process of taking old seed of low viability and growing a fresh batch. This process requires highly trained staff, multiple geographical locations, and controlled pollination. Each of these skill intensive and knowledge intensive requirements make the usual industrial approaches to efficiency unworkable. The result is that there remains a qualitative difference between the work that the NPGS is funded to pursue, and the work that the NPGS is supposed to pursue according to its mission.

Unit of Analysis: Independent Alleles

The question of how to quantify the sampling of genetic resources was not a major issue prior to the notion of genetic resources conservation. At that time it was only important to collect enough diverse plants to satisfy the needs of plant breeders. The need to quantify only surfaced with the publication of *Genetic Resources in Plants* (Frankel and Bennett 1970) which was a result of early genetic resources conservation conferences. This need was not addressed in theoretically grounded quantitative terms until the publication of a follow-up volume in 1975 (Frankel and Hawkes 1975) which included a paper (Marshall and Brown 1975) on sampling genetic resources. This established the allele as the definition of a genetic resource. That definition has been in

[3] Recalcitrant seeds are those which are not compatible with long-term, low-temperature storage.

use for the last 30 years and I will show that it is outdated and out of line with current population genetics theory. This use of an outdated theory in a field that is, otherwise, well connected to genetics, is a scientific phenomenon can be best addressed through social theory. Here I will define alternative theories and show why the one used in genetic resources is outdated.

The original approach to sampling genetic resources, developed in the practice of plant collections, was to collect plant diversity. Where micro-climates, ecosystems, or the plants themselves were more abundant, the collector was instructed to collect more plants (Allard 1970). That concept proved unwieldy when the work moved to a conservation paradigm and concerns of efficiency were foregrounded. Initially the issue was qualitative and plant explorers sought to collect some unknown amount of diversity, but one that was proportional to the amount of diversity present. Under the conservation paradigm, this qualitative approach became unacceptable.

Explorers knew that genetic resources were being paved over and plowed under on a daily basis. Time wasted collecting any more of one resource than was necessary meant that other resources were unnecessarily lost. While working with plant diversity as a guiding concept, it is easy to drown in the riches. Plant diversity as a concept is difficult to quantify, and therefore a weak guide for determining the amount of material that needs to be collected from a single patch of plants. To understand how much plant diversity we have, what we have, what we need, and what a successful collecting mission is, requires a more detailed definition of diversity. Therefore, conservationists have tried to define diversity more precisely so that it could be quantified.

Ultimately, the amount that can be conserved, using whatever definition, is limited by the capacity to collect, store, and maintain a collection. Although each limit is important, the way that these limits function in an institutional setting makes collection a primary concern. Maintenance expenses are incurred decades after the plants are collected. These are the funds required to regrow old seeds and produce new, healthy seeds for storage. In a resource-poor institutional environment, these resources do not receive sufficient consideration since the maintenance process can always be delayed. For example, at the NPGS, the queue for regeneration of accessions is currently a decade long in some cases. Storage has become extremely efficient in recent decades. For example, Native Seeds/SEARCH collection, which is currently using about 15 acres and a summer crew of ten to maintain its collection, stores its entire collection in five freezers on approximately 400 square feet of floor space. Storage is therefore a comparatively minor resource consideration. Collection has, by default, become the step where the attempt to optimize has had the most impact. It is one place where it is necessary to have a strict definition to be able to measure efficiency.

In a 1975 paper, Marshall and Brown defined the goals of genetic conservationists as the collection of at least one copy of each of the different alleles[4] in a target species. These authors define a genetic resource as a single allele. Then they categorize alleles according to two binary characteristics: the first characteristic distinguishes whether an allele is broadly geographically distributed or restricted to a particular place—endemic.

[4] An allele is a variation on of a genes. If there are two plants, and they are identical genetically except for one gene, where plant one plant has allele A and the other allele B, they two plants might be different colors.

The second characteristic is the frequency at which an allele occurs within a population—Marshall and Brown define common alleles as those present at a frequency of 0.05 or greater in the population. Rare alleles are those present at any lower frequency. According to Marshall and Brown (1975), common and rare alleles, so long as they are broadly present, will be collected simply because each collecting mission for a target species has the chance to gather them. Similarly, rare, endemic alleles should not be a collection target because they require excessive resources to collect on a large scale. According to these definitions and exclusions, the common but endemic alleles form the group explicitly targeted for collection. They can be collected with a reasonable amount of effort, but they must be targeted if we are to stand a chance of collecting them.

Following Marshall and Brown's modeling, a random sample of fifty gametes—reproductive cells which typically have one copy of the plant's genome—will have, with 95% certainty,[5] one copy of each allele (Marshall and Brown 1975, 65). These authors also postulate an extreme case that bounds how much is worth collecting even if time is relatively abundant:

> ...even in the limiting case of twenty alleles with frequency 0.05, a random sample of about 120 gametes will include with 95 per cent certainty one copy of each allele. Consequently, it seems safe to conclude that a random sample of 50-100 would be more than adequate under most circumstances. (Marshall and Brown 1975, 65)

Although the original paper uses an upper limit of 100 or 120 gametes to collect, this limit has not remained as the model in the literature. Although J.G. Hawkes, another prominent conservationist writing on collecting in 1980, uses the range 50-100, in a later

[5] The number 95% is arbitrary, but common in statistical analyses. Marshall and Brown justify it by pointing out that collecting more extensively produces a drastically diminished benefit to cost ratio.

publication (Hawkes 2000) he recommends only 50 individuals. In a rewritten edition of their original paper, Brown and Marshall (1995) suggest at least 50 individuals. Writing about collecting wild plants for conservation, Guerrant et al. (2004, 423) refer to Brown and Marshall (1995) but use 50 individuals as the *upper* limit for the number of individuals to collect. Overall, the bottom limit of Marshall and Brown's (1975) recommendations is being applied. This would only be marginally problematic if the initial recommendations were on solid ground, but they are not.

There are multiple definitions of genetic resources. Marshall and Brown (1975) apply the definition of a single, independent allele that carries a trait as the basis of their theory. An alternative definition, one which Marshall and Brown only considered by substitution, is the notion of genetic complexes. A genetic complex can be understood as a set of alleles associated with a trait. According to Namkoong et al. (2004, 856) the vast majority of agriculturally relevant traits are the results of genetic complexes:

> …it has been found that the classical multi-locus model for quantitative traits is usually correct. [Quantitative Trait Loci] mapping of loci relevant to such characters as yield, size, speed of development and continuous variation in particular morphological traits have typically found very large numbers of loci segregating each with small effect.

These authors cite Edwards et al. (1987) who, for 82 quantitative characters, demonstrated that 90% of loci affecting quantitative traits had an effect of less than 2% on the trait. Individually, the alleles present at these loci would not be a useful resource to a plant breeder because too many seasons would be required to assemble them together to a useful extent. Clearly the assumption that a genetic resource can be usefully defined as an independent allele is problematic. Nevertheless the argument is not straightforward since the two definitions I have presented actually overlap in some situations. Marshall and Brown (1975, 57) did consider genetic complexes to be the "real targets of genetic conservation", but they dealt with these complexes by substitution only. That is, they argue that if their definition is used, genetic complexes will be included in the sample as well. I will now show that this argument does not hold and therefore constitutes a contradiction between the theory of sampling in the genetic resources conservation field and the population genetics field.

Marshall and Brown (1975) have argued for the substitution of independent alleles for genetic complexes as the unit of analysis based on two arguments.[6] The first argument was based on theoretical considerations and second on experimental considerations. On the theoretical side, they argued that the difference between alleles and complexes was only apparent. They argued that "co-adapted gene complexes can be regarded as 'alleles' at a 'super gene' and in the context of sampling strategies are no different in principle from alleles at a locus" (Marshall and Brown 1975, 57). A ‘super gene’ is a set of genes that are all associated with a trait. Co-adapted sets of genes are also associated with a single trait and, Marshall and Brown argue, they do not behave differently in the context of sampling. If this were entirely accurate it would dismiss the concern over gene complexes, but it is not.

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[6] Frankel, Brown, and Burdon (1995) have also argued that genetic complexes which are lost by fragmentation can be regained in the process of breeding. Although this is true in principle, it is also impractical in the context of genetic resources conservation for agriculture. Conservation work is carried out with the purpose of breeding agricultural crops. The work of reconstituting genetic complexes would add years to the development of new cultivars—an entirely impractical proposition.

If a gene complex is composed of two alleles and each allele occurs at a frequency of 0.1—10%—in the population, then the complex, if it is adaptively neutral,[7] will only be present at a frequency of 0.01. Under Marshall and Brown's strategy, the complex carries a greater than 5% chance of being lost even though each of its component alleles might be collected. Apparently, the assumption in making this substitution is that important genetic complexes are always adaptive—that is, they are associated with a trait that is important for the plant's survival in its current habitat (Frankel, Brown, and Burdon 1995, 141). Under that assumption they would be common and easy to collect, making the 'super gene' assumption workable. Nevertheless that assumption is a weak one. Important complexes can be part of a response to cyclical stresses—drought, disease, flooding—and during the low-stress years they would decrease in frequency. i.e., a plant would no longer need a specific combination of ten alleles to survive, so those offspring which only had some of the alleles would not be selected against. The proportion of plants that carried only some of the alleles would increase, meaning the complex would become less frequent and more fragmented. Therefore, conceptualizing a genetic complex as an allele at a 'super gene', does not alter the chances of collecting it unless the complex is an adaptive one.

Since the real goal is to collect genetic complexes—currently adaptive or not—as long as they occur above a frequency of 0.05, Marshall and Brown's theoretical argument for a substitution does not hold. Their second, experimental argument is just as problematic. They argue that gene complexes function in experiments as single genes do:

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[7] Adaptively neutral meaning that it is neither selected for nor against.

> ...in population studies, it is impossible to study single loci independently of closely linked loci. It follows that where we use experimental data from single loci to draw inferences about collecting strategies, these inferences refer to the linked segments marked by the individual loci rather than the marker gene themselves. (Marshall and Brown 1975, 57)

Closely linked loci are those places on the genome that tend to stay together through sexual recombination. The argument here is that we cannot distinguish closely linked gene complexes from individual genes experimentally so where we use experiments to calibrate sampling strategies we will be calibrating them towards the collection of individual genes as well as complexes. This argument is convincing, but it relies on a condition—that we use experimental evidence in deciding on sampling strategy. Despite this condition, the rest of Marshall and Brown's paper is a work of theoretical genetics and statistical calculations. Therefore, this argument is not relevant to Marshall and Brown's particular recommendations. Furthermore, recommendations for sampling based on the specific genetics of individual crops are not common in the literature outside of a few drastically different crops.[8] The manuals that cite the Marshall and Brown (1975, 1995) papers accept the numbers provided without the caveat that experimental data should be used to consider gene complexes. In practice, Marshall and Brown's condition of using experimental data has not been applied—this means that gene complexes may not be sampled effectively.

Part of the power of Marshall and Brown's approach is that it is purely theoretical—their recommendations provide sampling benchmarks for any population regardless of the state of our understanding about the population's genetic structure.

[8] Crops, such as potatoes and garlic, which are propagated as clones have vastly different population structures than corps maintained as seed. Their population structure has long been recognized as different.

However, despite the authors' statements to the contrary, these recommendations should not be construed to apply to gene complexes.

If one were sampling for genetic complexes, 50 individuals from 50 populations would be insufficient to collect complexes occurring at a 5% frequency with acceptable certainty. The changes that would be required to theories of sampling genetic resources if a genetic complex approach were taken are significant. Minimum sample sizes might have to increase dramatically to achieve the same type of conservation goals that Marshall and Brown laid out in their paper. Under a framework that considers genetic complexes, the resource requirements of genetic resources conservation are clearly higher. As I have argued above, there are good scientific reasons for including genetic complexes in sampling. Gene complexes cannot simply be treated as 'super-genes,' they require a modified theoretical approach to sampling, and they cannot simply be reconstructed as needed due to resource limitations. In light of these fairly basic arguments, it is surprising that genetic resources conservation is still using a theory of sampling developed in the 1970s.

As indicated by Namkoong et al. (2004) population genetics has progressed far beyond the theories in use in genetic resources conservation. Specifically, sampling that targets individual alleles is no longer an appropriate strategy for collecting genetic complexes. The mismatch between theories in these two closely related fields constitutes a contradiction from a purely scientific perspective. The continuing mismatch leads me to believe that the ideological and institutional pressures operating within genetic resources conservation might be significant contributors to the stability of this contradiction. Addressing this possibility provides the opportunity to understand how such processes might impact scientific practice on a broader scale.

CHAPTER V

THE PRIMACY OF THE GENE

Genes Without Deoxyribonucleic Acid

In theories of sampling genetic resources for conservation, the independent allele—or gene—has been the dominant unit of analysis since the 1970s. It has maintained that position despite the developing sciences of population genetics and plant breeding that suggest a more complex understanding is needed. Understanding this paradox requires an understanding of where the notion of independent genes as units of life originated, how those ideas have since been propagated, and for what reasons. This information is essential to learning what ideological commitments stabilize the current understanding of independent genes, and broader questions about the role of ideologies in the natural sciences. My analysis starts briefly with Darwin and Mendel since the reinterpretation of these two figures into the coherent framework of classical genetics is one of the foundations of an atomistic understanding of genes—an understanding that is focused on individual parts and ignores complexities. Therefore, I begin by contesting how these figures are understood. Next, I consider the rhetoric of atomism within classical and molecular genetics to demonstrate how it forms an ideology distinct from its scientific merits.

When the genetic resources movement coalesced in the 1960s, the gene was the ascendant unit of analysis in biology. Since biology in general, and population genetics in particular, would be the foundation of sampling theories, the initial importance of the independent gene concept in sampling theories is expected. The molecular structure of DNA had been worked out in 1953, lending an atmosphere of exactness that the science had previously lacked, and researchers proceeded to work on understanding the meaning of the genetic 'code.' The gene was the next hot thing and the movement to conserve agriculturally useful plants was framed squarely in terms of the conservation of *genetic* resources. The history of how scientific and political contests ultimately made the gene the new rising paradigm in biology is crucial to understanding the ideological baggage that genetic resources conservation—and sampling—are saddled with. Often this history is reduced to naming the discoverers of various ideas. Following Kuhn, I see the work of early figures important to genetics such as Darwin and Mendel as part of a different paradigm than current work. In light of that observation it becomes important, as Haraway suggests, to understand what intellectual and material context their work was accomplished in. That context offers a contrast for the later reinterpretations of their work. It is also important to understand how the work of these founding figures was shaped by its context since similar processes operate today. Finally, these figures have been reinterpreted by others and they are a part of the mythology of genetics. It is therefore crucial to know how their work was subsequently received, and used.

In the pre-1900 era, biology as a field had accepted that evolution occurred, but it did not accept any particular mechanism for that evolution. Darwin's *Origin of the*

Species was published in 1859 and was only partially successful in making new converts. Bowler (1989) argues that, although Darwin's work was powerful in helping promote an evolutionist world view, the mechanism of random variation and natural selection was basically ignored until the end of the nineteenth century. This initial reception was due to two factors. On one hand, the precedent for evolution came in J.F. Meckel's 1821 "law of parallelism," which Bowler (1989, 43) presents as follows:

> [Meckel] argued that in the course of its development the embryo in effect ascends the hierarchy of animal forms, passing form the lowest to the highest levels of organization we see elsewhere in the animal kingdom. The human embryo passes through phases in which it is first a fish, then a reptile and finally a mammal... This announcement was followed by a growing recognition among paleontologists that the sequence: fish, reptiles, mammals, represents the order in which the classes were introduced in the course of the earth's history.

The notion that new species appear and are related had precedent before Darwin published his ideas. The pre-Darwinian assumption was that the Creator had devised a plan where the species would become established over time and "progress, not adaptation, was the key to the development of life" (Bowler 1989, 49). This theory of evolution was firmly connected to a comfortable, purposeful world. Darwin's theory of the mechanism of natural selection did not allow for the assumption of a divine plan and was therefore ignored by most of his contemporaries—even those who defended him.

Part of Darwin's troubles[9] resulted from the lack of an established mechanism for the inheritance of traits. His initially proposed mechanisms involved the blending of parental traits in the offspring—a mechanism that could be easily construed to disallow evolution by drowning out adaptive traits in reproduction (Bowler 1984, 61-62). The

[9] For a more complete discussion of the troubles of Darwinian theory, see Bowler (1983).

effect was that, although evolution quickly enjoyed wide currency in the scientific community, the mechanism of natural selection was not broadly accepted until the 1940s in Europe (Bowler 1993, 3) and somewhat earlier in the United States.

Mendelian heredity, also crucial for classical genetics, followed a similar trajectory. Prior to the Mendelian understanding of heredity, there was no notion that an individual trait might be moved from one plant variety to another related variety by crossing the two. Instead crosses were seen as either reproducing one or the other parent, or as blending—and therefore diluting—traits of both parents (Bowler 1989b). When change was produced, it was thought to occur due to saltations, or instant and broad changes in a plant's form, habit and vigor. These were thought to provide the only means to evolution. An article by Mendel, published by the Brno Natural History Society in 1866, is said to have laid the foundation for the modern understanding of heredity.[10] This reconstruction of history has been challenged on a variety of grounds, the most powerful being that Mendel was apparently not interested in the problem of heredity (Branningan 1979; Olby 1979). Some historians have even suggested that Mendel was opposed to the entire notion of evolution:

> An even stronger statement of this thesis comes from L.A. Callender (1988), who insists that Mendel's intentions have been misinterpreted because of the widespread assumption that he favoured the general theory of evolution. Callender sees Mendel as an opponent of evolutionism who was trying to defend Linnaeus' alternative theory of the production of new species by hybridization... The fact that Mendel had a theory of the production of new species by hybridization is confirmed by his reference to the struggle for existence—a struggle he conceived not in Darwinian terms, between individuals, but as a

[10] Mendel crossed two homozygous pea plants and measured the ratio of parental traits in the hybrids. This experiment can be reinterpreted to show that offspring can be a mix of parental traits, and that the parental traits will occur in offspring in specific proportions. This is the insight classical genetics took from Mendel's experiments when they were rediscovered in the 1900s.

> process taking place between the new species formed by crossing. (Bowler 1989, 103-105)

These understandings of Mendel are important here primarily to disrupt the notion of some sort of intellectual succession between Darwin, Mendel, classical genetics, and molecular genetics. Regardless of what Mendel thought about his experiments, when they were 'rediscovered' in 1900, it was in the context of heredity as the basis for a new science (Bowler 1989, 113). The Mendelian approach quickly gained some champions, but unresolved issues delayed its general acceptance. The traits that Mendel had chosen to study were controlled by one or two major genes. Due to this, they were likely binary, or if not binary then they only possessed a few states—e.g.-round seeds, elongated seeds, pointed seeds. There are also traits that are controlled by dozens of genes and display much more gradual variation. Although both forms can be currently understood under a Mendelian framework, this was not the case in the early 1900s. This unresolved discrepancy delayed the acceptance of Mendelian heredity till the 1940s in Europe and the 1910s in the United States. Once the new science of heredity was accepted in plant breeding, it produced an important shift in how plant breeders and collectors viewed their tasks.

In the pre-Mendelian climate, plant breeding relied primarily on 1) finding well-adapted crops by exploration, and 2) sowing a crop in a particular place and choosing the individuals that performed best for seed—essentially waiting on evolution to work by some undefined mechanism. This slow but effective method is now called simple mass selection. With the establishment of Mendelian trait inheritance—between 1890 and 1910 in the United States (Kloppenburg 1988, 68-72)—came the possibility of moving traits between plant varieties through hybridization. The Mendelian experiments provided a mechanism that could theoretically be used to manipulate plants like machines, moving separate parts from one to another.

> The plant breeder's new conception of varieties as plastic groups must replace the old idea of fixed forms of chance origin which has long been a bar to progress... Since the science of plant breeding has shown that definite qualities may be produced and intensified as required, it is no longer necessary to wait for nature to supply the deficiency by some chance seedling. (American Breeders Association 1905, 204, emphasis added)

Much like the mechanical conception of nature described by Merchant, this new understanding produced a more effective science via an ontology of life that reduced complexity. Although plant breeders initially did not understand how the process worked, they were able to focus on the few traits that this process was applicable to. In practice applying Mendel's discovery meant crossing a wild plant that held a useful trait with a cultivar. Out of the hybrids, the most vigorous plants that also carried the required trait were kept and again crossed with a cultivar. Each time the hybrid was crossed with the cultivar, some of the new hybrids would carry the desired trait but the undesired traits from the original wild plant were diluted. The process could be repeated until the result was practically the same as the cultivar, plus the new trait. This practice, called back-crossing, was time-consuming, but also useful for adapting crops to new environments and therefore capable of revolutionizing agricultural economies.

When Mendelian genetics did find its contemporary place within the Darwinian notion of evolution, plant breeding had gotten hold of an essentially modern understanding of its biological basis—minus the DNA. Plant breeders no longer sought to find usable exotic germplasm for adaptation to the United States. Instead they sought to find exotic plants that could be hybridized with established varieties so that particular exotic traits could be included (Kloppenburg 1988, 79-80). This was the beginning of the, essentially modern, idea of exchangeable biological parts transmitted through sexual reproduction. According to Kloppenburg (1988, 80), this shift in understanding was practically complete by 1925 and impacted plant exploration:

> It naturally implied a different strategy for germplasm collection activities. K.A. Ryerson (1933:124), plant explorer with the USDA, summarized the new perspective: "Species and varieties which in themselves have little or no intrinsic value become of the first importance if they possess certain desirable characters which may be transmitted through breeding."

In the pre-Mendelian approach, plant collectors sought to find only exotic cultivated plants—those that showed promise as crops in the Untied States. Plant domestication, wherever it happens, is a genetic bottleneck—farmers often find only one or two genotypes that might be edible whereas the wild plant is unpalatable. Those few plants often become the basis for the new crop. The amount of genetic diversity available from crops is therefore only a small part of the diversity available in wild plants. Using the Mendelian framework, explorers no longer needed to look for *cultivated* plants with desirable traits, they could find *weeds or wild plants* with the desirable traits. This shift quickly increased the effectiveness of collections efforts.

> Though the number of expeditions remained relatively constant over time, the actual number of accessions per five-year period nearly doubled between 1925 and 1930 and has continued to grow, save for a hiatus in collection during World War II. (Kloppenburg 1988, 80)

Back-crossing techniques were required to use this new wealth of plants. Since these techniques required the use of controlled, economically unproductive crosses, they would be difficult for farmers to use (Simmonds 1999, 158-160). Plant breeders at Land Grant

Universities and Agricultural Experiment Stations had found their qualitative distinction from farmers in techniques that allowed for the complex, conceptually atomistic, manipulation of crop evolution. Although this was an ideological transformation it had material impacts on plant exploration, its institutions, and breeding expenditures. At this point the gene was only a word and an atomistic way of thinking applied to biology.

The Gene Materialized

Through the work of Franklin, Watson and Crick in 1953, the gene finally materialized into a molecule—deoxyribonucleic acid. In the immediate sense, this molecule would serve to carry the Mendelian traits that geneticists and plant breeders sought to manipulate. More importantly, the gene emerged as an object that could carry useful ideologies—especially the compelling idea that life had a larger purpose and that genetics would allow us to control it. Since Darwin's mechanism of evolution through natural selection had put the notion of a purposeful world in question, the stage was ripe for this ideology.

Evelyn Keller (1995) argues that the teleological baggage—the notion of life developing with a purpose—was injected into the gene in two stages. In the 1920s the form of that baggage was the notion of 'gene action' and, introduced in the 1940s, was the notion of a 'genetic program'. Gene action can be defined as the idea that the basis for life is solely in the genes. Genes were the carriers of purpose and they directed the development of the organism. Looking at a fertilized egg in sexual reproduction, geneticists in the 1920s might see that the chromosomes where the genes resided were only a small part of the cell, but they were perceived as the most important part. The genes were seen as active and the rest of the cell was seen as essentially passive.

> When Muller identified the gene as *the* basis of life, he was claiming for it both ontological and temporal priority. First the gene, then the remaining protoplasm (the cytoplasm), which appears as a *by-product*, the only function of which is that of a facilitating environment, to *foster* the gene... The concept of the gene invoked here is Janus faced: it is part physicist's atom and part Platonic soul—at one and the same time a fundamental building block and an animating force. (Keller 1995, 9-10)

Though this language developed to the point where it became natural to discuss the actions of genes, basic questions about meaning were left unanswered. Keller (1995, 10) points out that, at the time, there was no explanation of what genes actually *do*, *how* they do it, or *what* they are. Currently it is still difficult to say that genes *do* anything (Namkoong 2004). They may interact with other cellular processes to create a certain biochemical and structural environment within the growing organism, but they do not act independently. The notion of gene action was always limited in its explanatory power by this problem. It was also never the basis of a better understanding of how genes function.

Gene action did, however, allow geneticists to promote their field by positing a simple framework where their object of study was the alphabet for controlling life. Such an argument carries the implications that once the alphabet was understood, organisms can be acted upon piece by piece. Much as Mendelian trait inheritance was supposed to allow plant breeders to move groups of traits between plants, the new genetics might allow scientists to move any traits and perhaps even redesign them. Importantly, the notion of gene action could accomplish this without answering the most complex questions about how genes acted on the organism. The strategy was very effective in securing resources as genetics came to overshadow the field of embryology, its historical counterpart, and is still in ascendance. It is a very successful scientific ideology in the same way that the mechanistic worldview was a successful ideology for Francis Bacon. It reduces the broader problem of how life develops to independent parts that we can—supposedly—manipulate. This reduction is inaccurate, as Namkoong et al (2004) point out, but that fact is sidelined by the power of the new worldview based on genetics.

The development of this ideology did not end with the notion of gene action. Keller (1995, 18-19) argues that Watson and Crick's overlooked contribution to science in their article on DNA, was calling the baggage carried by DNA "information".

> Geneticists and molecular biologists were euphoric: there, surely, must be the answer! DNA carries the 'genetical information' (or program), and genes 'produce their effects' by providing the 'instructions' for protein synthesis. DNA makes RNA, RNA makes proteins, and proteins make us. It was without doubt, one of the greatest milestones in the history of science… but still, one might ask (although few people did at the time), what kind of answer is this? What, in fact, do *information, program, instruction* or even the verb *makes* actually mean? (Keller 1995, 18)

Although the notion of genes as information carriers was based on little evidence, it was again an excellent ideology. With the terms of discussion set, it became easy to justify research in the bureaucratic setting without actually answering very fundamental questions. This move updated 'gene action' with a modern paradigm of codes and computers that would direct work on genetics well into the 1990s.[11] Here the distinction between gene action and genetic program is not as important as the fact that these concepts maintained a mechanistic view of the gene's function and the primacy of the gene as the actor.

[11]See Kay (2000), and Oyama (2000), for more detailed analyses of the transformation.

In the 1960s, when genetic conservation was established as a field, the language of the gene was already well developed. At the time, the notions of gene action, genetic program, and the gene as *the* basis for life were fully established. At the same time, the information that would lead to the destabilization of the gene by highlighting the role of other cell parts was still unavailable. In that context, when biologists discovered that the centers of diversity were disappearing and a rescue mission needed to be launched, genes as the focus of conservation were a natural choice. Still, genes are not natural units. Their identity as units was produced partially through disciplinary conflict between genetics and embryology. Additionally, the genes' primacy as actors was also produced, partially, in the pursuit of funding. The choice of individual genes over whole plants or plant traits was both historically and ideologically conditioned.

In the 1970s the notion of genes as independent actors, on reasonable scientific grounds, was enshrined into genetic resources theory and practice by Marshall and Brown (1975). These authors defined a genetic resource as a single, independent allele. Since the 1970s genetics has evolved to the point where the independent-allele model is outdated. This discrepancy is especially surprising since the two fields, genetics and genetic resources conservation, are otherwise very close. I suggest that the usefulness of the gene-as-actor ideology in the pursuit of research funding has helped keep the outdated theory in place. In the sense that Kuhn defined it, the primacy of the independent gene has become a paradigm of scientific research in genetic resources conservation. In the sense that Sarewitz suggests, the independent gene has also become part of the mythology that helps secure ongoing funding for some genetic resources work. For both these reasons the gene is a touchy subject and a critique of it is dangerous to genetic resources conservation work. At the same time, it is crucial for genetic resources conservation to move past this ideology and address the deeper contradictions such as the lack of a distinction between short-term and long-term conservation goals.

This argument is only part of the story. From the perspective I discussed in my methodology, this has been a weakly materialist argument. It is important because it presents the partial perspective that helped to center the gene in the conservation discourse. This argument also establishes that the independent gene has been both a scientific concept and a form of ideology. The materialist perspective, which I address next, adds to the analysis by revealing how the application of biology to genetic resources conservation was supported institutionally. The atomistic notion of genetics, focused on the independent gene as the most important part of a living organism, has been widely accepted in genetic resources conservation institution; that prominence has only made it more entrenched as an ideology.

CHAPTER VI

MATERIAL FOUNDATIONS OF THE U.S. NPGS

Overview

The second step in my synthesis perspective on the NPGS is a case study of its institutions, carried out in order to understand what institutional political-economic pressures might impact genetic resources conservation science. Following Levins and Lewontin (1985), I conceptualize science as a fragmented process that is partially regularized in terms of results and partially co-opted by capital accumulation. I argue that genetic resources conservation has been particularly important for the agricultural industry and co-opted through that relationship.

As I argued earlier, the NPGS's role in agriculture cannot be understood as monolithic. On one hand, the NPGS is meant to dispense genetic diversity in the short- to medium-term so that the production of major crops under conditions of industrial monoculture can be stabilized. On the other hand, the NPGS is supposed to conserve the genetic heritage of humankind. Although the general techniques applied to meet both responsibilities can be the same, the quantities of resources needed for the conservation mission are vastly larger. The goal of this chapter is to answer the question about what institutional commitments exist within the NPGS to each part of its mission. There is clearly an imbalance between the two parts of the mission, and I propose that the relationship with industrial agriculture is primary.

A history that's useful to this end has not been written, primarily because the ideologically driven story is so powerful and obvious that it is difficult to approach critically. The often-repeated, idealized origin story of these institutions takes place in the early 1970s. In 1970 and 1971, the U.S. lost 15% of its corn crop nationally to southern corn leaf blight. In some affected areas half the crop failed and some farmers lost entire fields. The USDA booklet *Seeds for Our Future*—a brief summary and explanation of the National Plant Germplasm System—passes on this common story as follows:

> In 1970, a disease called Southern corn leaf blight swept through cornfield after cornfield from the southeastern United States into the Great Plains. This epidemic cost farmers 15 percent of the corn crop that year (about 700 million bushels). It happened because nearly all the corn being planted was genetically susceptible to the fungus that caused the blight. To prevent similar epidemics from recurring in later years, seed companies and breeders reverted to hybrids that lacked the susceptibility but otherwise had the desirable genetic makeup of the affected corn.
>
> The 1970 epidemic reminded us of how vulnerable modern agriculture has become. It reminded us of the mid-19th century Irish potato famine, which happened for similar reasons and cost not just money, but hundreds of thousands of lives... [The epidemic] was partly responsible for the formalization of the National Plant Germplasm System. (USDA 1990)

Though this is only one story from one program booklet, it is the most common one told in response to questions about *why* we need a genetic resources program. The specter of famines comes up again and again—famine in India, famine in Ireland. That story, however compelling, is not why we have an NPGS. The NPGS and all its precursor institutions have been key to expanding the economic power of U.S. agriculture.

Although the NPGS meets both functions to some extent, I argue that the balance has been and continues to be tipped towards economic concerns.

This chapter discusses three distinct pressures that have contributed to a system that is very effective at nurturing industrial agriculture, but less competent at conserving genetic resources. The first pressure is the historical relationship to agribusiness as a user community. The conservation context for the National Plant Germplasm System in the United States only developed in the late 1960s and 1970s. For most of their history, the institutions that now make up the system were primarily involved in supporting farmers, and, by the 1950s, agribusiness. The second pressure is the *de facto* system of advocacy for gene banks based on the economic power of the user communities. The most economically powerful users are able to advocate most effectively, meaning that the 'major crops'—corn, wheat, soybean—receive a larger share of funding whereas biologically important but economically 'minor' crops—berries, fruits, vegetables—receive less funding.

The third pressure is the schism between the narrower goals of the system's advocates, and the complete set of goals of the system's managers. The users of the germplasm system have long been, and continue to be, private and public breeders who, by the 1950s, focused their work on delivering new varieties to industrial agriculture. For breeders, access to and evaluation of germplasm resources are the key parts of the NPGS. Since the system perpetually both acquires and looses larger numbers of accessions than a breeder could possibly put into use, the actual conservation of all acquired germplasm is a secondary concern to the work of breeding. Similarly, it is broadly acknowledged that the plants most useful to breeders are those that are closely related to currently cultivated varieties (Goodman 1990). The conservation of distantly related plants is therefore less a of a user priority than the conservation of land races and "obsolete" cultivars (Shands 2005).

These pressures distort the biological goals of genetic resources conservation, but they are common in the practice of gene banking. Not all these pressures were present at all times in the history of gene banking in the United States. They developed in an uneven but mutually reinforcing fashion over time. The second two can be seen as elaborations on the first and they developed in the order presented: 1) the historical relationship to agribusiness as a peripheral user community; 2) the *de facto* system of advocacy for gene banks based on the economic power of the peripheral user community; and 3) the schism between the narrower goals of the system's advocates, and the complete set of goals of the system's managers. Whereas a strong organizational structure might have allowed the NPGS to pursue its mission, that mediating influence was missing in the NPGS. Its weak structure has played an important part in allowing these pressures to drive the overall trajectory of U.S. gene-banking practice.

This chapter will show how this practice has developed in three historical sections. The first runs section runs through the 1950s when the dominant paradigm was exotic plant and then exotic trait acquisition and storage. The second section encompasses the development of the conservation paradigm in the 1960s and 1970s. The third section covers the 1990s when the NPGS was reorganized into the National Genetic Resources Program (NGRP) and given responsibility for additional life forms: forest trees, animals, aquatics, insects, and microbes. Although this expansion of responsibilities did not fix the system's financial woes, it did add the other bioindustries to the mix: silviculture, meat production, aquaculture, and microbial genetic engineering.

Gathering the World's Plants

The National Plant Germplasm System (NPGS) did not exit prior to the 1970s. The Plant Introduction Stations and National Seed Storage Laboratory that were ultimately tied into that system existed for decades under a context that was much more focused on the development of the economic power of American agriculture than on conservation. This section will show the historical roots of that context and how closely it was tied to the developing industrial agriculture of the 1950s. The tips of these roots reach to the colonization of the Americas and the subsequent nation-building project. Nelson Klose's (1950) romantic history of crop introductions to the United States begins with the founding myth of the United States—the Discovery of America. In this story, *America's Crop Heritage* (Klose 1950) crop introductions begins with Columbus' second voyage. The sugar cane he introduced was apparently a vast success. Following this success, oats, peas, and rye were among the introductions made to feed colonists and provide them with culturally recognizable food. The colonists probably dismissed Native American foods as quickly as Klose who says simply that "the Indians cultivated few crops in comparison with the wealth of plant life which the immigrants brought with them." (Klose 1950, 1). Colonial powers and private companies also made introductions of sugar cane, and silk with purely monetary and political gains in mind.

Because settlers and plantation owners steadily claimed new territory for cultivation of European crops, they also regularly introduced new varieties—the varieties of oats, peas, rye, and wheat that first succeeded in maturing and producing seed on the east coast of North America did not necessarily grow further inland.

> The spread of crops to new areas was made possible only through the adoption of a few varieties without which the move would not have been possible. Between 1839 and 1859 the center of wheat production moved from western Pennsylvania to western Ohio. (Kloppenburg 1988, 56)

These exotic introductions to the United States and their ongoing breeding were part of individual capitalistic projects, but they were also a fundamental part of American economic nation-building. It is not surprising then, that starting in 1836 the U.S. Federal Government became involved in the introduction process through U.S. Patent Office Commissioner Henry Ellsworth (Kloppenburg 1988, 55) who began sending exotic seed out to farmers for testing. Ellsworth organized that initial effort himself. In 1839 he was funded to carry out the activity, and with his retirement in 1849 the function was institutionalized in the U.S. Patent Office under the Department of the Interior. Although many introductions of exotic plants were made by corporations, government officials, or individual capitalists, the work of testing and breeding varieties stayed in the hands of farmers well into the early 1900s. These farmers risked a crop failure every time they tried out a new variety—it might mature late, lack appropriate pollinators, or be unable to flower at a particular day length. New varieties also held the possibility of higher yields, better reliability, and better resistance to cold or disease.

In 1898, Secretary of Agriculture James Wilson made the first changes in how exotic plants would be introduced into and disseminated in the United States. His predecessor, Sterling Morton, acting as a champion of the seed industry, had tried to kill the USDA's Seed Division distribution program. Since the Seed Division was one of the day's pork-barrel programs, the episode resulted in Morton quickly being put in his place by Congress. However, Wilson took a much less confrontational approach by dividing the program. One part would remain in the business of distributing high-quality seed as long as Congress was interested in paying for it. The other became the Section of Seed and Plant Introduction which Wilson sought to make into a more rational scientific enterprise:

> A staff of professional botanists was employed who were able to recognize plant diseases and pests and assess the agronomic value of varieties. With N.E. Hansen's 1898 journey to Russia in search of hardy alfalfas and forage crops was launched the 'golden age of plant hunting' that over the next quarter century would see some 48 expeditions scour the world for useful germplasm. Accessions were, and still are, recorded in the *Inventory of Foreign Seeds and Plants*. The first entry is a cabbage variety, 'Bronka,' collected near Moscow. (Kloppenburg 1988, 67)

Although these changes seem like commonsense basics, they were both revolutionary and very incomplete. The changes were revolutionary in the sense that one bureaucracy would now track introductions, and decide who could test the introductions. The new bureaucracy relied on professional scientists to make these judgments, a move that standardized the methods used to choose plants for introduction. The same move specified the training necessary to make plant introductions decisions. Though promising varieties must have been excluded from introduction in the process, scientists in this bureaucracy mediated the risk that new crops might fail entirely.

These were the beginnings of rationalized exploitation of exotic plants. The changes, brought about through the Section of Seed and Plant Introduction, were also incomplete because once plant material passed on to growers there was no insurance that a variety would survive. This was the beginning of the establishment of the material basis of genetic resources conservation in the U.S. but it also highlighted a basic contradiction by omitting the storage of introduced seed. It created only a halfway house for incoming seed. As a result of these deficiencies, approximately 90 percent of introductions made prior to 1950 have been lost (Busch et al. 1995,92). The creation of the regional and national Plant Introduction Stations in the 1940s and 50s partially resolved the issues of tracking incoming accessions:

> The establishment of the first of our regional plant introduction stations in 1948 in Ames, Iowa, was a significant step in germplasm conservation. Three additional regional plant introduction stations were established at approximately the same time at Geneva, New York (1948); Experiment (now Griffin), Georgia (1949); and Pullman, Washington (1952). (Busch 1995, 94)

Although Busch refers to these stations as an advance in conservation, that context did not really exist in the 1940s and 50s. The focus of these institutions would be entirely on plant introductions—they stored germplasm only so it could be made available to breeders in the pursuit of short-term goals. Two important issues that positioned the Plant Introduction stations in the U.S. plant breeding system were the replacement of farmers by professional breeders and the regional focus of the stations.

Farmers had finally lost their place in breeding by the 1950s. With complex breeding methods differentiating between full-time plant breeders applying scientific methods and farmer-breeders, the USDA Section of Seed and Plant Introduction passed most of its exotic imports to personnel at Land Grant Universities, Agricultural Experiment Stations, and private breeders in the service of agricultural corporations.

Breeders now mediated the connection between exotic germplasm and farmers; as time went on an increasing proportion of these breeders worked in the service of private companies instead of public institutions. Farmers were no longer the dominant group of users for the Plant Introduction Stations. This transition is significant because Plant Introduction Stations, part of the Land Grant Universities[12], now explicitly served agricultural capital and not simply the farmers themselves. After the Second World War, plant breeding turned increasingly to satisfying the needs of industrial agriculture such as uniformity, determinate fruiting, and the consumption of cheap synthetic nitrogen fertilizers (Kloppenburg 1988, 118) available after the wartime bomb-making industry shrunk to peace-time size. The results were High Response Varieties (HRV's) that produced increased yields as promised only as part of an agro-chemical package. That package included HRV's, synthetic fertilizers, irrigation, pesticides, and farm machinery (Simmonds and Smartt 1999, 347-355). This overall system was part of a very particular, industrial vision of agriculture and both breeders and Plant Introduction Stations participated in it.

That vision and its identity as the only route for the progress of agriculture has persisted to the present day. The second issue is that the Plant Introduction Stations were primarily regional institutions: "These stations were established primarily to meet the germplasm needs of plant breeders and other scientists; their responsibilities were based principally on the concern of agriculture in each of their respective geographic regions"

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[12] Since the Land Grant Universities were created for the purpose of serving the public and are bound by law to do so, this is clearly a problematic position for them to be in.

(Busch et al. 1995, 94). Due to the focus on the agriculture of specific regions these, they were positioned firmly as institutions working in support of established economic crops.

By this time the relationship between agricultural businesses and the Plant Introduction Stations was very well established. Although it had started as an indiscriminate mass distribution of exotic seed, plant introductions had been rationalized through the USDA Section of Plant and Seed Introductions and, later, through the Plant Introduction Stations. Throughout this period, the understanding of plant introductions came to focus less on plants, and progressively more on finding useful traits, and useful genes (Kloppenburg 1988, 69). These stations primarily supported breeders who increasingly worked for private and not public institutions. This realignment of plant introductions with an industry as opposed to the mass of farmers meant that the future 'user community' was becoming better defined, more cohesive, and more directed in its goals.

Farmers had always been on the lookout for tasty, disease resistant, productive crops. Private seed companies were interested primarily in the productivity of varieties, and the profits that they could bring in. Agriculture was no longer simply a system that produced food, it became increasingly geared to produce profit. This work was, of course, carried out under the guise of providing more food for the nation. These shifting associations set the stage for future conflicts of interest between users and managers of U.S. germplasm.

From Plant Introductions to the Conservation Paradigm

The change from the introductions paradigm to one of conservation started globally in the 1960s. In 1936 one article by Harlan and Martini (1936) had mentioned the possibility that elite varieties might displace more diverse varieties traditionally maintained by farmers. However, it took the collective experience of multiple plant explorers to trigger international concern and U.N. action:

> A transformation came in the mid 1960s, from a variety of sources... warnings of a greatly accelerated rate of displacement of primitive crop varieties by locally selected or introduced cultivars came from scientists with experience in important gene centers, e.g. H. Kuckuck (Iran, Turkey, Ethiopia), J.R. Harlan and D. Zohary (Near East), E. Bennett (Turkey), J.G. Hawkes (Latin America). (Frankel and Hawkes 1975, 1)

This process privileged specific explorers to speak for the condition of biodiversity in particular regions of the world. On one hand this privilege led to quick international action—there was no need for a careful argument based on masses of data. On the other hand this expedient approach handicapped conservation efforts by masking the actual conditions of landraces as well as wild and weedy relatives of crops. Although some crops did disappear as scientists feared, many landraces have persisted alongside newly adopted modern varieties. This means that where scientists perceived a complete loss there were significant opportunities for *in situ* conservation. Conservationists working in this context were working from a framework where plants were to be collected for centralized control and distribution. Additionally, they followed the pattern of subordinating conservation to economic development—regardless of the form. The result was that conservationists ignored these *in situ* opportunities. Instead they turned to the UN to provide them with the resources for more intensive collection work.

It was not until 1971-72 that Frankel and Hawkes (1975, 99) carried out the first survey of genetic resources and erosion—it was primarily a sketch relevant just to the major crops:

> Limitations of time and finance restricted the scope of the enquiry and are responsible for some of the gaps—such as the omission of cassava, of fruit species in Iran, or of all vegetable crops: and for deficiencies due to the limited number of experts who could be consulted. (Frankel and Hawkes 1975, 100-101)

The experts saw that the situation was urgent and tasks had to prioritized. Nevertheless, it is important to remember that this sketch was organized and executed primarily by scientists working in the service of American, European, and Australian industrial agriculture. Their goals were to survey the major food crops first, and anything that was especially threatened or easily accessible. It is not surprising that wheat, potatoes, and corn made it into the survey; it is also unsurprising that the initial survey did not include cassava.

Hancock (2004, 212) associates the cassava firmly with the Global South both economically and culturally: "*Manhiot esculenta* Crantz (manioc, cassava, or yuca) is a critical food source in tropical areas of South America, Africa, Asia and several oceanic islands... It is grown mostly by peasant farmers as a staple, but is also used to produce industrial starches, tapioca and animal feed." This omission is indicative of a particular focus in the genetic resources conservation movement. That focus is culturally Western and certainly economically Western. The influence that experts such as Otto Frankel, and J.G. Hawkes had on genetic resources conservation is not far removed from the effect that Merchant assigns to Francis Bacon. In each case, the powerful ideas of some individuals were propagated and made durable by the institutional resources they were able to gather. This parallel provides a partial answer to the question of institutional commitments in genetic resources conservation. Even before the movement affected the United States, it had taken a Western concept of food crops, and a focus on those crops that provided primary calories—not necessarily other nutrition.

The urgency that the experts felt was clearly articulated through the U.N., but the message did not arrive in the United States until the 1970s. When genetic resources did become an issue in the U.S., it was on the heels of the economic devastation caused by the 1970-71 Southern corn leaf blight. Losses to consumers and farmers totaled billions of dollars and companies that manufactured hybrid corn seed escaped class action lawsuits only by "legal obstructionism" (Kloppenburg 1988, 122). The corn blight was a particularly powerful indicator of the problematic aspects of American breeding programs. First, the blight was caused not by the slow narrowing of the genetic base of corn over time, but by the introduction of a specific set of genes into almost all corn grown in the United States, in a very short period of time. This uniformity was practically engineered into corn populations. Second, the same genes that made corn vulnerable to blight, allowed for the production of hybrid corn seed without detasselling and were an economic boon for seed companies. Together, these two facts pointed to a level of economic rationality exerting genetic control at the crop population level that was unheard of before. Farmers planted entire fields and entire agricultural landscapes as monocultures.

The U.S. southern corn blight alone did not give birth to a conservation paradigm. It merely focused attention on the genetic makeup of American crops. The National Research Council (1972) took up both the task of understanding the foundation of the 1970-71 corn blight, and assessing the genetic vulnerability of other major crops. That work surveyed corn, wheat, sorghum, pearl millet, rice, potato, sugar beet, sweet potato, legumes, vegetables, and cotton—except for rice, it found them all standing on questionable genetic bases. Although the report mentions the "germ plasm bank or gene pool" (NRC 1992, 297-298) as a "device being used more and more by geneticists and plant breeders to meet the challenge of uniformity," it was apparently written in ignorance of the budding medium-term gene banks of its time—i.e.-National Seed Storage Laboratory which had been in operation since 1958—since the report only mentions breeders' collections. It was in this context that the NPGS was formed in 1974 by tying together the Plant Introduction Stations and the National Seed Storage Laboratory into a national network under the weak supervision of a 'special coordinator' (NRC 1991, 91).

A later report (NRC 1978) concluded that more diverse, non-commercial, genetic stocks available in ecosystems, fields, and botanical gardens were crucial if plant breeders were to continue responding to new plant pathogens. This report placed the NPGS as a key institution safeguarding the American economy and food supply. In light of this conclusion the report indicated the nation should:

> Support the aims and activities of the National Plant Germplasm Committee to: (a) develop a plan of repositories for clonally propagated plants; (b) provide facilities, staff and funds for USDA Inter-regional and Regional Plant Introduction Stations; (c) establish a facility for growing genetic collections of tropical plants; (d) set criteria as to the [responsibilities] of curators of specific genetic crop collections; (e) select such curators, provide for their support... (f) characterize major collections, with computerized data on plant genetic collections; (g) identify and rectify gaps in major collections; and (h) support research on techniques for maintaining collections. (NRC 1978, 97)

Two issues are especially significant: First, the transition from merely holding part of the introduced diversity to permanently storing it required new resources. The NPGS would need a vastly higher level of funding for this task. The system never received the needed money. Essentially, it was only funded to function as a halfway house to hold important material that could be moved when it was no longer useful. Although it could be argued that federal agencies always request more funds, the NPGS has been cited as having fewer resources than it needs to perform its basic tasks on a regular basis by a wide variety of authors (NRC 1978; OTA 1987, 233-235; Goodman 1990, 11-16; Busch et al. 1995, 96).

Second, crops that are clonally propagated instead of being stored as seed still did not have repositories in 1978. These crops—fruit trees, berries, grapes are economically minor crops. This fact ties back to the founding purpose of plant introductions in general and the Plant Introductions Stations specifically—that purpose was initially purely economic with considerations made for nation-building. The National Research Council (NRC 1991, 3-4) succinctly summarizes the mission that drives these efforts: "Germplasm activities in the United States have been largely driven by an unofficial policy of national self-sufficiency that calls for comprehensive collections to reduce dependence on other nations or institutions." All these things point to an institutional system oriented towards economic self-sufficiency. In practice, this has meant prioritizing the genetic resources that provide bulk calories—grains, corn, and potatoes—and ignoring the rest. This hoarding approach to genetic resources is unworkable as a broad genetic resources conservation strategy since the task of conserving genetic resources is too vast to carry out in the gene banks of a single nation. On the other hand, if the goal is to safeguard the nation's ability to feed itself in the short term—provide bulk calories for its citizens—then a botanical hoard need only encompass major economic crops, such as corn and wheat. These institutional histories, contradictions, and formally unarticulated but acknowledged goals have interacted to give make the NPGS focused only on major Western crops and service-driven as opposed to mission-driven.

The underfunding of the system can also be seen through the lens that Levins and Lewontin (1985) provide. To use that lens, we have to ask what parts of the work of using genetic resources conservation have been regularized as part of the system of capital accumulation and which parts are set aside as an unpredictable government function. In plant breeding the predictable use of genetic resources is for: 1) improving crop yields; and 2) combating current pest or disease issues. The unpredictable needs are for 1) long-term genetic variability within crops; and 2) the future impact of diseases. The essential difference in genetic resources conservation strategies for these two sets of needs is in time scale. The predictable needs currently require germplasm be stored on the order of decades. The unpredictable needs require storage in perpetuity. Since funding is available only for the short to medium term, the institutional commitment of genetic resources conservation appears to be primarily to resources used for capital accumulation.

Although the mission of germplasm conservation—saving as much as possible of as many crops as possible—has kept the NPGS involved in the conservation of crop plants in general, it is clear that the system in total has pursued its stated mission in an uneven fashion. This unevenness is manifest in two areas: 1) economically important crops are conserved far better than minor crops—an economic unevenness manifest in the genetic unevenness across genera in the NPGS collections; and 2) activities needed by the user community in the short term are pursued more than long-term conservation activities. The difference in resources devoted to the preservation of major and minor crops is most easily seen in the extent of collections for particular crops. As a baseline for comparison, the system's flagship institution, the National Seed Storage Laboratory (NSSL), currently keeps over 460,000 accessions. This laboratory is meant to be the long-term storage facility for all other accessions in the system and it currently holds a large portion. The NSSL is not responsible for regenerating any accessions and typically does not send out seed to users.

With that baseline, we can examine two institutions at different ends of the economic scale. First, the National Small Grains Collection (NSGC), responsible for wheat, barley, oats, rice, rye, *Aegilops,* and triticale, currently keeps over 128,000 accessions, or about 1/4 of the amount at the NSSL. The NSGC is the largest collection outside of the NSSL and it keeps more accessions than any of the Plant Introduction Stations despite being responsible for only five major crops, the triticale hybrid, and one wild species. It is also the most used collection in the NPGS, distributing over 100,000 samples in 1995 (Busch et al. 1995, 98). For contrast, the largest National Clonal Germplasm Repository, responsible for 26 genera—groups of species—holds only 13,000 accessions. Due to a lack of resources, many of those accessions are a single plant in a single pot in a single greenhouse—a situation that puts them at a very high risk of loss. These are the minor crops—hazelnut, strawberries, mint, pears, berries, and hops among others.

This may be a travesty from the perspective of genetic resources conservation, but Congress was not following that ideal in implementing the NPGS. Instead, Congress built the NPGS by bringing together institutions that had been charged with rationalizing the plant introductions system for the development of the American industrial agricultural economy. This institutional structure has created clear pressures on the implementation of genetic resources conservation and arguably its science.

Beyond a program focus on major economic crops, the NPGS also pursues the parts of its program responsibilities in an uneven manner. This is the result of a powerful user community with goals other than long-term conservation. This piece of political economy is best demonstrated by highlighting discrepancies between resources allocated to various activities. The same institutions are responsible for acquisition through exploration or exchange, characterization of the plants, evaluation of particular traits, distribution, and regeneration. This last function is not immediately relevant to users but it is crucial for the long-term mission of genetic resource conservation. As the NRC (1992, 28) astutely observed, "seeds do not live indefinitely." Soon after harvest seed germination rates can be around 95%, and successful storage techniques can keep germination rates high for years or decades. Regardless of the time frame, germination rates fall as some seeds die, and by the time that the test germination rate falls to around 65% even the live seeds start to be genetically unstable. Regeneration is the process of growing seed out to plants and harvesting fresh, healthy seed for storage.

Regeneration is also regularly one of the most expensive and underfunded parts of NPGS operations (NRC 1992, 28; OTA 1987, 235). The result is that the survival of large parts of the NPGS collection is in question:

> Viability data collected between 1979 and 1989 indicate that 29 percent of the accessions in the [NSSL] collection had germination rates that were either unknown or less than 65 percent. In addition, 45 percent of accessions contained fewer than 550 seeds... there are no effective plans for regenerating certain seed collections held at the NSSL that are unadapted to conditions anywhere in the continental United States or Puerto Rico, even though the NPGS has accepted international responsibility for several of these base collections. In short, like other nations, the United States has emphasized storage at the expense of regeneration and evaluation. (Busch 1995, 96-97)

In terms of genetic resources conservation this is completely irrational since if we are storing seeds we might as well make sure they are alive. However, it is rational from the point of view of most users, whose interaction with the germplasm occurs on a timescale shorter than a decade. These are researchers and breeders, who need material for current or upcoming projects, so the future loss of material is a lower priority concern.

In the case of the NPGS, the Crop Germplasm Committees (CGCs) are key players in determining priorities. To allow for detailed expertise, there is one CGC for each crop or group of crops—there are 40 in total. Their role is to "advise the Department on the acquisition, preservation, and information needs of the germplasm collections" (GAO 1997, 1). The GAO (1997) surveyed these CGCs and found that even though the collections were in poor or unknown condition (GAO 1997, 45), the CGC's placed regeneration eighth on the list of priorities (GAO 1997, 28). CGC members prioritized regeneration lower than acquisition, evaluation, characterization, enhancement/prebreeding, documentation, active storage/preservation, and molecular biology research.

The fact that there are mixed priorities and perceptions based on relationship to the germplasm collections is also made clear in the report. Although most CGC members did not put a high priority on regeneration, curators for 15 of 38 crop collections—the people actually responsible for preserving the collections—reported that regeneration is insufficient (GAO 1997, 48). Since the CGC's are primarily made up of users, with only one or two curators per committee, the curators are effectively silenced in the committee system. Institutional decision-making operates to shift resources away from conservation and towards the more economically remunerative utilization activities.

In contrast to these powerful economic and user pressures, the organizational structure of the NPGS itself is very weak. Congress created the NPGS in 1974 after the Agricultural Research Service had been reorganized into a set of regional structures. That reorganization disconnected the national staff from regional offices and left budgeting, staff selection, and decision making to the regional managers. The NRC's (1991, 89) report situates the national staff's authority as follows: "Their responsibilities are described as programmatic (i.e., program planning), which is to say, advisory." The situation for the NPGS was only slightly better since the regional stations and the NSSL were under the authority of a "special coordinator"; nevertheless, the authority to decide whether a program will be implemented, and how it will be implemented stayed with regional offices (NRC 1991, 91). In 1984 this special structure in the ARS was lost and the national program leader for the NPGS became a part of the National Program Staff. This leader's authority is comically diluted:

> The program leader now chairs a Germplasm Matrix Team (GMT) comprised of other National Program Staff members. Only with concurrence from the GMT and the cooperation of the area directors can the program leader make recommendations on appropriating funds, hiring staff, and coordinating activities. The program leader's success in executing a national germplasm program depends largely on his or her ability to persuade or to cooperate with the eight area directors and the GMT. (NRC 1991, 91)

Not only is the national leader's role advisory, he or she needs to convince an entire committee to make recommendations. If that were not complex enough, there are conflict of interest questions. The members of the GMT have primary interests and responsibilities outside the NPGS but they compete for the same funds (NRC 1991, 91). This creates a perverse incentive to interfere in the effective implementation of NPGS programs.

Weak national coordination of the NPGS means that the distorting pressures coming from economic interests and the system's users are not significantly mediated through a focus on the overall mission. Although in some situations the diffuse framework of authority might be more efficient, in this case it fails to meet the challenges of a system beset by multiple demands diverging from its specific stated mission.

At the end of the 1980s, the NPGS was fully formed as an institution. Its elements included the National Seed Storage Laboratory, the Clonal Repositories for nonseed crops, the National Small Grains Collection, and the Regional Plant Introduction Stations. The southern corn leaf blight episode in 1970-71 had refocused the attention from simple plant introductions to genetic resources and their conservation. In the short-to-medium term, this system of agricultural stations continues to acquire, document, and make germplasm available to plant breeders and researchers. The NPGS is fulfilling its old role—plant introductions—as well as it ever has. However, it is far from fulfilling its role in conserving plant biodiversity for American agriculture. The system's weak organizational structure within the ARS, and through the CGC's, means that it is often subject to institutional inertia. In the case of the NPGS that inertia is significant and tends towards the plant introductions mission and serving the major economic crops. In the early 1990s, Congress again reorganized the NPGS to expand its mission from plant conservation to include animals, aquatics, bacteria, and invertebrates.

The Rationalization of Genetic Resources Conservation

As indicated above, the success of the National Plant Germplasm System in 1990 was questionable from the viewpoint of genetic resources conservation, and moderately good from the viewpoint of plant breeders. From the perspective of the government's auditors, the NPGS was merely an administrative mess (Busch et al. 1995, 103). Without resolving these issues, Congress reorganized and expanded the system's responsibilities:

> In the 1990 Farm Bill lawmakers established the National Genetic Resources Program to provide for the collection, preservation, and dissemination of all genetic material of importance to American food and agricultural production. It is divided into six program areas: plants, forest trees, animals, aquatics, insects, and microbes. In addition, the U.S. Congress has mandated that ARS conduct an assessment of the projected needs over the next 10 years for this new comprehensive program. (Busch et al. 1995, 103-104)

Although the ARS completed that mandated report and it asked for a quadrupling of the total funding (Diversity 1991, 23), none of the money was forthcoming (Shands 1994, 52). When the NPGS received an increase of $3.0 million for FY 2001, it was hailed as "the largest annual increase in the NPGS budget in 20 years"; most of this increase was used on building repairs and deferred improvements (Diversity 2001, 8).

Although expanding the system without funding makes little sense from the viewpoint of genetic resources conservation, it is a rational extension of the plant germplasm system to other life forms. This economic rationality has little to do with conservation as it focuses on the needs of industrial agriculture. Such a form of rationality, applied to a system of genetic resources conservation is a distortion, but it is powerful and entrenched. Economic and user-relations concerns will likely continue to drive the NPGS more than its stated genetic resources conservation mission. These relations are unlikely to result in more conservation funding since the NPGS is already functioning well as a short-term supplier of genetic resources.

This entrenched set of relations needs to be challenged if conservation goals are to be achieved. At the same time it exerts a powerful pressure on scientific work within the genetic resources conservation field. In this context of radical underfunding, basic problems such as building maintenance and the regeneration of seed accessions predominate. It seems that there is no time for deeper questions such as how we define genetic resources. Nevertheless, it is those deeper questions that might allow us to challenge the anemic state of this system on scientific grounds. As I argue in chapter four, even a basic reconnection between sampling theories and contemporary theories from the field of population genetics would force us to distinguish between the needs of conservation and the needs of industrial agriculture. Such a scientific distinction may not be enough to revive interest in conservation, but it is required if the needs of conservation are to be evaluated on their own merits. Otherwise, the mission of the NPGS will always be in the shadow of the needs of industrial agriculture.

CHAPTER VII

HYBRIDS: POLITICS, ECONOMICS, GENETIC RESOURCES

According to social theories on the practice of science, both ideological and social pressures may impact the way that scientific institutions operate, how scientific theories are formed, and the practices of science. First, since research requires long-term commitments and institutions, scientists are always dependent economically on public or private funds. Second, since both public and private institutions have goals for their research dollars, scientists must always make the case that their particular research is relevant to these goals. Both of these influences have shaped how genetic resources conservation has developed.

First, the institutions incorporated into the NPGS, and later the NGRP, are closely tied to agribusiness and plant breeders as their user community. That user community is involved in both decision making and the acquisition of funding. This economic dependence has meant that, within its resource limits, the NPGS has become user-driven. Since it successfully delivers the genetic resource needs of its user community, there is no broader effort, from the user community, to actually increase the funding available to the NPGS. In practice, the system achieves short-term goals, and falls short of long-term goals, but the rhetoric about why we need an NPGS is based on arguments for long-term goals. The effect of an underfunded system aimed at long-term conservation is short-term conservation.

Second, the scientific theory of genetic resources conservation—particularly the theory relating to sampling—seems to be caught up in these economic influences. Initially, the need to quantify genetic resources forced Marshall and Brown (1975) to define a genetic resource in enough detail that it could be counted. At the time, they defined the genetic resource as an allele. In the thirty years since then population genetics—the science that Marshall and Brown based their paper on—has produced more complex understandings of genes. In particular, the theory suggests that at least genetic complexes ought to be included in the sampling strategy. Nevertheless, genetic resources conservation continues to rely on the 1970s definition.

Social theory suggests the following framework for this story. The gene may initially have attained its position as an atomistic unit on reasonable scientific foundations. Since then, it has remained a powerful theory primarily because of its utility in explaining the social relevance, and economic importance, of genetics. This places it in the realm of ideology first, and science second. Following Marx, it is necessary to see this ideology as existing in combination with economic pressures. Within the NPGS, I argue that institutional and economic pressures have stabilized the atomistic approach to genetic resources. The NPGS has been radically underfunded and effectively focused on short-term goals. In such an environment, the definition of genetic resources seems irrelevant since the system is barely keeping up with its current responsibilities. The current definition of alleles as genetic resources also feeds the short-term focus by allowing for the short-term and long-term conservation goals to appear to have the same resource requirements.

This might seem like a situation without a clear exit, but I suggest that the best method for exposing the problem of underfunding in the NPGS is to separate the short-term pursuits from long-term goals. This can be accomplished most effectively by again taking up the issue of how genetic resources should be defined for agriculture. In my fourth chapter, I demonstrated that a simply reconnecting genetic resources sampling to contemporary population genetics would allow us to distinguish between short-term needs and long-term goals. Such a new definition ought to at least consider how genetic complexes—which Marshall and Brown (1975) named as the true goals of conservation—can be collected, stored, and maintained. This move would provide a scientific basis that could be used to distinguish conservation from short-term concerns, such as supplying industrial agriculture. If the two could be distinguished, the opportunity would exist to evaluate the importance of each goal on its own.

Although this move might be difficult, there is precedent for it in the NPGS. In 1898, Secretary of Agriculture James Wilson first divided the exotic seed distribution program into the original pork-barrel program—which kept constituents happy by supplying seed—and the USDA Section of Seed and Plant Introduction. The latter program went on to become a professional, mission-driven agency which sought to improve agriculture by applying scientific methods. A similar division might be necessary currently. One program could be a fee-funded halfway house for genetic resources required by plant breeders. The other would be a mission-driven genetic resources conservation program which dealt with long-term issues. At least such a division would allow us to clearly evaluate our priorities.

CHAPTER VIII

CONCLUSION

If one considers the concerns articulated in magazines such as *Diversity* and the *Plant Genetic Resources Newsletter*, there is a significant amount of debate within the genetic resources conservation field over what agriculture ought to look like. Nevertheless, the uniting concerns voiced explicitly in those publications are for the safety and sustainability of agriculture in general. These concerns reach back to the global perspective that have been important to work on plant diversity since the writings of DeCandolle and Vavilov. Concerns of safety and sustainability are necessary to reconcile the single most important problem in genetic resources conservation—that the development of new varieties currently undercuts the very foundations that makes plant breeding possible. These are also the concerns that genetic resources conservation will need to address, if it is to break its destructive relationship with purely industrial agriculture. Social theory, and the understanding of science as a social activity, are both crucial if scientists are to understand and manage how genetic resources conservation interacts with social—ideological and institutional—pressures.

I have argued that there are two significant social processes at work in shaping the conservation of genetic resources in the United States. First, the genetic resources conservation field developed at the time when an atomistic form of genetics was on the rise and adopted understandings of genetic resources that were similarly atomistic. In time these atomistic understandings proved to be well suited to the institutional pressures operating within the NPGS. The institutions that constitute the U.S. framework for genetic resources conservation have historically served the agricultural industry, not conservation. Through a combination of weak, conflicted governance, institutional inertia, and underfunding, the actual function of these institutions has continued to be mainly economic.

The fact that social influences have, essentially, reached into the laboratory and shaped theory does not invalidate scientific knowledge. Beyond the genetic resources context, it indicates that social theory, particularly historical and ideological analyses, can generate an improved understanding of how scientific practices are affected by social pressures. Social theory can be important in understanding both why a field has followed a certain pattern of development historically, and why certain paradoxes within the natural sciences are permitted to exist long after they have been noted and analyzed through the usual paradigms. This form of analysis is especially appropriate to key topical issues of Environmental Studies such as agriculture, environmental justice, 'development,' global warming, wildlife management, and water resources to cite a few. In each case scientific processes are joined with ideological, economic, and cultural issues that beg for a synthetic analysis. Analyzing each one independently would be as foolish as applying single-variable calculus to a three-dimensional physics problem, or discussing the behavior of individuals according to categories such as the American People or the New Yorker.

A synthetic approach to analysis is needed for these complex issues. Researchers must be willing to combine relevant analyses from the natural sciences, social sciences, and humanities. In this context, the contributions of each perspective must be accepted on its own. This means accepting that a conclusion based on philosophical or ethical argument might be applicable in a scientific paper. This context also means acknowledging that results from a paper in the natural sciences might be an important part of an analysis of political economy. In any case, comfortable disciplinary boundaries can no longer be the limits we set for what counts as knowledge relevant to a problem. The danger in losing control of the social processes that impact conservation is not purely environmental. The biological systems at issue are the basis of the human food supply, medicine, cultural life, and, in the coming decades, they will have to become the basis of our energy supply. We cannot afford to ruin them.

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