

The Wide Adaptation of Green Revolution Wheat

by

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ABSTRACT

"Wide adaptation" is an agricultural concept often employed and seldom closely examined. Norman E. Borlaug, while working for the Rockefeller Foundation (RF) on an agricultural project in Mexico in the 1950s, discovered that some tropical wheat varieties could be grown over broad geographic regions, not just in Central and South America but also in the Middle East and South Asia. He called this wide, or broad, adaptation, which scientists generally define as a plant type that has high yields throughout diverse environments. Borlaug soon made wide adaptation as a core pillar of his international wheat program. Borlaug's wheat program rapidly expanded in the 1960s, and he and his colleagues from the RF heavily promoted wide adaptation and the increased use of fertilizers in the Middle East and India. These events led to the green revolution, when several countries rapidly increased their wheat production. Indian wheat cultivation changed radically in the 1960s due to new technologies and policy reforms introduced during the green revolution, and farmers' adoption of 'technology packages' of modern seeds, fertilizer, and irrigation.

Just prior to the green revolution, Indian wheat scientists adopted Borlaug's new plant breeding philosophy—that varieties should have as wide an adaptation as possible. But Borlaug and Indian wheat scientists also argued that wide adaptation could be achieved by selecting only plants that did well in high fertility and irrigated environments. Scientists claimed, in many cases erroneously, that widely adapted varieties still produced high yields in marginal, or resource poor, areas. Many people have criticized the green revolution for its unequal spread of benefits, but none of these

critiques address wide adaptation—the core tenant held by Indian wheat scientists to justify their focus on highly productive land while ignoring marginal and rainfed agriculture. My dissertation describes Borlaug and the RF's research program in wide adaptation, Borlaug's involvement in the Indian wheat program, and internal debates about wide adaptation and selection under favorable environments among Indian scientists. It argues that scientists leveraged the concept of wide adaptation to justify a particular regime of research focused on high production agriculture, and that the footprints of this regime are still present in Indian agriculture.

DEDICATION

To my parents.

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LIST OF ABBREVIATIONS

ALAD:	Arid Lands Agricultural Development
CGIAR:	formerly Consultative Group on International Agricultural Research, now known as CGIAR Consortium
CIMMYT:	International Maize and Wheat Improvement Center
CIP:	International Potato Center- affiliated with the CGIAR
CV:	coefficient of variation
DWR:	Directorate of Wheat Research
FAO:	Food and Agricultural Organization
FF:	Ford Foundation
GxE:	genotype by environment
IARI:	Indian Agricultural Research Institute
ICAR:	Indian Council of Agricultural Research
ICARDA:	International Center for Agricultural Research in the Dry Areas
ICRISAT:	International Crops Research Institute for the Semi-Arid Tropics
IFPRI:	International Food Policy Research Institute
IRRI:	International Rice Research Institute
MAP:	Mexican Agricultural Program
PAU:	Punjab Agricultural University
RF:	Rockefeller Foundation
SD:	standard deviation
TAC:	Technical Advisory Committee of the CGIAR

USAID: US Agency for International Development

USDA: US Department of Agriculture

INTRODUCTION

Introduction

Norman Borlaug is perhaps the most famous agricultural scientist in the modern world. He is remembered for saving millions of people from starvation in the 1960s and 1970s by introducing high yielding varieties of wheat. Starting in the 1940s, he worked for the Rockefeller Foundation's (RF) agricultural program in Mexico. In 1955, he developed a fertilizer-responsive variety of semi-dwarf wheat that would soon spread around the world. Five years later, he discovered wheat varieties with what he called "surprisingly broad adaptation" (known also as wide adaptation) (Borlaug, 1968, p. 8). These varieties could be grown not just across agro-climatic zones, but across continents. Borlaug soon realized that this wide adaptation was due to the genetic trait of photoperiod insensitivity.¹ Yet discussions of wide adaptation seldom centered on photoperiod insensitivity; they focused on vague claims of improvements in the inherent yield of wheat (high yield independent of environmental context) and harkened to the greater quest of feeding the world.

Becoming head of the RF's international wheat program soon after he discovered wide adaptation, Borlaug made wide adaptation a key part of his program. To Borlaug, wide adaptation became emblematic of his program's global reach and ability to affect radical agricultural change. Against the prevailing sentiment that "plant breeders must work in the place where their crop will be grown," (Hesser, 2006, p. 52), Borlaug argued

¹ Photoperiod insensitivity is a simple dominant genetic trait (*Ppd*) that allowed wheat varieties to grow at different latitudes and in different seasons, something that North American wheats could not do.

that wide adaptation was not only a tenable, but also a desirable plant-breeding goal. Through his publications, correspondence, and lectures, he influenced agricultural scientists around the world. Beyond this paradigm-shifting endeavor, however, Borlaug had a very mission-oriented reason for promoting wide adaptation: he wanted to transform agricultural production in developing countries from pre-modern to modern, and thought that widely adapted varieties were the most likely way to accomplish this.

In the 1960s, Borlaug quickly moved beyond the theoretical aspects of wide adaptation to its implementation. Borlaug and his colleagues saw wide adaptation as a way to speed up the process of adapting wheat varieties to countries with limited scientific resources to accomplish the task themselves (Rockefeller Foundation, 1964). He wrote to RF agricultural sciences director Albert Moseman in 1963:

Wheat varieties are much more flexible and plastic in their adaptation than has ever been recognized in the past. I feel that the materials that come from one broadly based wheat breeding program in which the objectives are a wide adaptation and resistance to the most important diseases of wheat, will be of value as varieties or lines that can be reselected for direct use in countries far distant from the location of the breeding programs. (Borlaug, January 18, 1963)

Borlaug proposed in 1965 that, “varieties and breeding lines with broad adaptation can be introduced rapidly and grown successfully in many areas of the world where expansion of food production is urgently needed. This is not possible with narrowly adapted varieties” (Rockefeller Foundation, 1965, p. 214). Borlaug realized that he could not only

transmit scientific knowledge to other wheat breeding programs around the world, he could directly transfer wheat germplasm.

In a plant-breeding context, adaptation means the relative performance (roughly, yield and disease resistance) of a plant variety under different conditions. Agricultural scientists define wide, or broad, adaptation as a crop that has high yields under many different environments and locations, also known as phenotypic stability. Scientists can measure variation of plant characteristics (such as plant height) to study the phenotypic responses, or adaptation, of plants to different conditions, but adaptation is typically measured in yield, or biomass per area. RF scientists wrote that yield “is the most meaningful measure of adaptation in terms of world food needs” (Krull et al., 1968, p. 1).

Despite the many possible advantages of widely adapted wheat varieties, there is significant evidence that Borlaug’s intended goal of wide adaptation—that is, wide adaptation across agronomic conditions—could be better described as adaptation across locations with similar agronomic conditions. Further, I present evidence that Borlaug so fervently promoted his intended goal that he overlooked evidence to the contrary. Borlaug and other proponents of wide adaptation argued that widely adapted wheat varieties were superior to local varieties (see Chapter 2) in both favorable and unfavorable environments, due to some “inherent yielding ability” (Stakman, Bradfield, & Mangelsdorf, 1967, p. 80). Further, they argued that locally adapted varieties were inferior under favorable environments because they could not utilize higher levels of fertilizer.

Critics against wide adaptation have argued that widely adapted crops were *only* widely adapted to favorable environments, and thus did not benefit farmers who face variable and non-optimal environments.² My dissertation research, both quantitative and qualitative, tends to support this position. In the 1950s, Borlaug specifically adapted wheat varieties to high fertility and irrigated environments, by selecting varieties under those conditions. After he discovered broad geographic range of his wheat varieties that was possible due to their photoperiod insensitivity, he then began to make arguments that his varieties could also be adopted in rainfed and low fertility environments. Despite the evidence against Borlaug's claims, Borlaug's ideas have had lasting consequences in the Indian and international wheat research programs (the International Maize and Wheat Improvement Center [CIMMYT] and the Indian Agricultural Research Institute [IARI]), leading to a system that is biased towards breeding and testing varieties under favorable conditions.

By the 1970s, Borlaug canonized wide adaptation in his narrative of the green revolution. In his Nobel Lecture, Borlaug spoke of the Mexican semi-dwarf varieties he developed:

It is the unusual breadth of adaptation combined with high genetic yield potential, short straw, a strong responsiveness and high efficiency in the use of heavy doses of fertilizers, and a broad spectrum of disease resistance that has made the

² Or, in the words of James C. Scott, that “every effort is made to transform and homogenize field conditions so that the field meets the genotype’s specific requirements” (Scott, 1999, p. 302).

Mexican dwarf varieties the powerful catalyst that they have become in launching the green revolution. (Borlaug, 1970)

While many scholars have studied the green revolution, this dissertation will examine the critical yet overlooked history of Borlaug's wide adaptation and its impact on international wheat research.

A Brief History of Adaptation Studies in Agriculture

The science of plant adaptation is historically and conceptually tied to plant collection and introduction. In the twentieth century and earlier, the science of plant introduction and adaptation was based on trial and error as well as using climate analogues to predict areas of good adaptation (Wilsie, 1962). When farmers or scientists introduce a plant to a new location, they test its adaptation to the new environment, or the “transposition of a genetic entity from an environment to which it is attuned to one in which it is untried” (Frankel, 1958, p. 338). While *ad hoc* collection schemes occurred earlier, in “1827 President John Adams instructed the foreign consuls to collect seeds and rare plants and send them to Washington, DC” (Baranski, 2013, no page number). Mark Alfred Carleton at the US Department of Agriculture (USDA) started procuring wheat varieties from around the world to test them in the US around 1894 (Loegering & Borlaug, 1963). The USDA created the Office of Foreign Seed and Plant Introduction in 1898 (Baranski, 2013).

Adaptation has been, and continues to be, a rather fluid term. In the ecological and evolutionary sense, it means a plant variety's reproduction and production of biomass based on its physiological tolerance and requirements of temperature, soil composition,

moisture, disease, sunlight, wind, species competition, etc., which vary between regions. Through evolution and natural selection, as well as artificial selection (by farmers and plant breeders), plants are assumed to be specifically adapted to their region of origin (the place that they evolved in—though recent studies have shown that plants are not necessarily optimally adapted to their local conditions). Scientists use specific, or narrow, adaptation to refer to a variety that only thrives under a specific set of environmental conditions (Annicchiarico, 2002).

Early studies of plant adaptation drew from the theories of Charles Darwin. Cittadino (1981) has shown that German botanists became interested in adaptation in the last two decades of the 19th century. Around 1895-96, Danish botanist Eugenius Warming coalesced the study of plant adaptation into a field he called “ecological plant geography” (Coleman, 1986). In his study of plant geography, Russian plant scientist and explorer Nikolai Vavilov created his well-known theory of the centers of origin of cultivated plants, based on his travels around the world as a plant collector in the early 20th century (Baranski, 2014). Vavilov wrote on the adaptation of wheat: “Although wheat in general appears to be a plant with varieties which are comparatively specialized, nevertheless, in many ecological types there is observed a high degree of ecological plasticity” (Vavilov, 1951, p. 193). After Vavilov, scientists used a variety of theories such as plant tolerance, ecotypes, plasticity, and genotype by environment interactions to explain plant distribution and adaptation (Wilsie, 1962).

In the mid-twentieth century, organizations like the Food and Agricultural Organization and the Rockefeller Foundation facilitated an enormous global movement

of plant germplasm. Research and development programs such as the RF's Mexican Agricultural Program focused on both the collection of wheat and maize germplasm, and the testing and breeding of those varieties. The growth of international plant germplasm exchange and introduction was tightly linked to increasing interest in the adaptation of crops to diverse geographies.

Wide adaptation existed in the lexicon of agricultural scientists in the 1960s, but only in the margins of agricultural science (Finlay, 1968). Since at least the mid-1800s, agriculturalists have used the term wide adaptation to describe the climatic and edaphic range of horticultural varieties in America, but wide adaptation was not viewed as a particularly desirable goal, rather as one of many possible plant characteristics (New York State Agricultural Society, 1856; Prentiss, 1866). The conventional wisdom of plant breeding in the early 20th century was that crop selection should occur in the target environment, creating varieties with specific adaptation to the local conditions. Even a 1954 annual report from the RF's Columbian Agricultural Program stated that, "it is axiomatic in agricultural research that an improved crop variety, to be commercially successful in a given region, must be developed and tested in that region" (Oficina de Estudios Especiales, 1954, p. 10). In other words, agriculture was a "site-specific science" (Perkins, 1997, p. 12), and most cereal breeders viewed wide adaptation with little more than skepticism (Finlay, 1968).

Until the mid-1960s, plant breeders believed that crops should be specifically adapted to local conditions. But as Borlaug's research program rapidly expanded from Mexico to the Middle East, Africa, and Asia, the international conversation shifted. Paulo

Annicchiarico (2002) wrote that in the first half of the 20th century, plant breeders in England and Italy focused on “understanding and exploiting specific adaptation effects in order to raise crop yields in their respective countries” (p. 9), and then in the second half of the century, breeding goals shifted to improving yield potential through selection in favorable environments. To date, existing regional and national plant breeding programs have diverse goals that range between specific adaptation and wide adaptation. But some countries, like India, are solely focused on wide adaptation of specific crops. This is a legacy of Borlaug’s involvement in Indian wheat breeding starting in the mid-1960s.

Wide Adaptation in India

This dissertation traces Borlaug’s ideas around adaptation and climate from the 1950s and through several decades and countries. I start with Borlaug’s discovery in Mexico, and move through India, Turkey, and the Middle East. My primary narrative focuses on the introduction of wide adaptation to India and the subsequent embedding of wide adaptation in the structure and practices of Indian wheat research. This focus on India is due not only to the availability of archival information in India, but also the landscape of post-Independence India and the involvement of RF scientists, particularly Borlaug’s interest and participation in reshaping Indian wheat research.

Many scholars have examined how Borlaug and the RF promoted a specific package of seeds and agricultural inputs that led to the green revolution. Most of these accounts, however, overlook wide adaptation as a critical part of this package. In retrospect, many critics point to the uneven distribution of green revolution technologies in India as evidence of a capitalist agenda. But Indian scientists utilized Borlaug’s

rhetoric around wide adaptation to promote food production and support equity throughout India's agro-climatic regions. While wide adaptation is not inherently problematic to any agricultural research program, in India in the 1960s, wide adaptation was used to justify a research regime focused on irrigated and fertilized conditions. Modern efforts to support regional and localized research are largely ineffective due to the continued reliance on old narratives and institutional structures set up during the green revolution. My primary research questions are as follows:

- 1) How did wide adaptation become central to Indian wheat research?
- 2) Why did some scientists so fervently promote widely adapted varieties in light of scant or contradictory evidence?

I also explore to a smaller extent, that in light of the historical connections between wide adaptation and plant breeding under favorable agricultural conditions, what does this mean for present-day discussions of poverty alleviation through agricultural development?

My dissertation relies heavily on the historical and theoretical groundwork laid by historians Nick Cullather (2010) and John H. Perkins (1997) in their respective books, *The Hungry World* and *Geopolitics and the Green Revolution*, as well as the recent contribution of Madhumita Saha with her 2012 dissertation, *State policy, agricultural research and transformation of Indian agriculture with reference to basic food-crops, 1947-75*. Perkins and Cullather map the connections between the Cold War, agricultural development, and the Green Revolution in Mexico and India (examined briefly in Chapter 1). These works also situate agricultural scientists in the context of state identity

and authority, such as the RF's connections with US foreign policy officials and advisors. In Saha's dissertation, she breaks down the dualism between "indigenous" and "modern" technology, showing the multifaceted reasons why scientists and political planners advanced certain scientific agendas post-Independence.

India became a major site of international intervention in agricultural development in the 1950s. This was due to the Bengal Famine of 1943 (which followed a string of famines in the late 1890s), India's Independence in 1947, and growing international fears of overpopulation and starvation in India in the 1950s. In the 1950s, the US Government began food and technical assistance programs in India, and various foundations also became involved in India's agricultural development. The RF, because of their program in Mexico, was invited to advise on Indian maize and small grain programs in the 1950s. In 1963, the Government of India invited Borlaug to advise on India's wheat research program, and Borlaug's semi-dwarfs introduced to India that same year.

Then in 1964 to '66, the Indian wheat research rapidly changed. The All-India Coordinated Wheat Improvement Program was formed and headquartered in New Delhi in 1965. That same year Benjamin Peary Pal, a wheat scientist, became head of India's agricultural policy organization, the scientist to hold this role. Finally, in late 1964 the RF hired R. Glenn Anderson to join the RF staff in New Delhi and to co-coordinate the All-India wheat program. Anderson and Borlaug worked with Indian scientists such as Pal and the famous wheat scientist M. S. Swaminathan to introduce not only new wheat varieties, but also the new plant breeding philosophy of wide adaptation. In addition to

these administrative changes, India was in the midst of an ecological and political crisis: a war with Pakistan, a failed monsoon season and looming famine, President Johnson's "short tether" approach to food aid as a negotiation strategy, and the untimely deaths of successive Prime Ministers Jawaharlal Nehru and Lal Bahadur Shastri (Perkins, 1997). Concurrent with the changes in the research landscape, the Indian government approved several agricultural policy changes including seed and fertilizer imports, a minimum support price for wheat as well as ongoing land policy reforms such as abolishing absentee landlords.

While Anderson coordinated the RF's Indian wheat program from Delhi, Borlaug organized and proselytized an emphasis on increased fertilizers concentrated in irrigated, productive areas, which at the time formed only twenty percent of cultivated land. High-level Indian agricultural scientists such as Pal, Swaminathan, and others supported Borlaug in this. These scientists used the concept of wide adaptation to justify that the concentration of research efforts on irrigated and highly fertilized land would spillover into rainfed and less fertilized areas, due to the claimed superiority of widely adapted wheats. But wide adaptation became controversial among scientists in India. A wide adaptation-based agenda meant that testing for adaptation was only done post-hoc, meaning wheat varieties would not be developed *in situ* or for location-specific conditions. Critics of wide adaptation questioned whether the system of plant breeding and testing for high production areas would benefit rainfed and marginal farmers in India.

To understand the connection between fertilizers and wide adaptation, one must put Borlaug in the context of his work in Mexico with the RF. When Borlaug started

working in Mexico in the 1940s, his task was to develop a wheat variety with higher yields and more disease resistant than the present varieties. He realized that more fertilizers were required to improve yields, but conventional varieties, when grown under higher fertility, would fall over due to the weight of their grain. Borlaug, working with Orville Vogel in Washington, thus developed a semi-dwarf wheat adapted to Mexican conditions in 1955. At that same time, Borlaug started testing varieties under exclusively high fertility conditions. Borlaug was convinced that fertilizers would soon become more available and affordable, and that fertilizer-responsive varieties were needed to increase global grain production. He also believed that testing under favorable conditions was the best and most efficient way to assess the genetic yield potential of varieties. Borlaug believed that an ideal genotype would be more powerful than environmental variation, and that this was the key to raising yields in developing countries. Borlaug's wheat program was considered successful in Mexico, as farmers quickly adopted his wheat varieties, though these were mostly commercial farmers in the Sonora who had access to irrigation and fertilizers. When Borlaug discovered wide adaptation around 1960, his wheat research program was solely focused on selection and testing under favorable conditions.

By the time Borlaug became involved in Indian wheat research, he was already highly invested in his model of widely adapted, fertilizer responsive wheat coupled with higher levels of fertilizer. Although the RF realized this did not reach marginal farmers, they were more concerned about overall global food production than equity. In India,

Borlaug's model appealed to scientists, especially those who worked in the mostly irrigated area of northwest India.

But some scientists, and especially India's social planners, were against this model (Saha, 2012). While Borlaug wanted to concentrate fertilizers on irrigated land, social planners favored evenly distributing fertilizers throughout the country. Borlaug lobbied to increase fertilizer production and imports, with marginal success. Against these pressures, Borlaug had to prove that concentrating on fertilized and irrigated areas was for the greater good. He did this through wide adaptation.

Around 1965, Borlaug began promoting wide adaptation on the basis of not just adaptation to location, but adaptation across agronomic conditions. While Borlaug was in the midst of planning an Indian green revolution, he argued that widely adapted varieties could equally benefit farmers under varying conditions. He claimed that widely adapted varieties would still produce high yields in less productive areas, which despite being ecologically dubious, became a central part of his argument. Borlaug's influence coincided with a greater shift in Indian agricultural policy starting in 1965 (see Chapter 3). In 1969, India's fourth five year plan radically updated the previous focus on equity to "betting on the strong" by concentrating resources on irrigated areas (Cullather, 2010, p. 199; Saha, 2012).

In light of the evidence I have collected throughout my research, I believe that Borlaug promoted a fundamental misinterpretation of wide adaptation that either he himself believed, or that he promoted in spite of evidence because of his ideological motivations. That is, I argue Borlaug's widely adapted wheats were so because of their

photoperiod insensitivity and fertilizer responsiveness, and not any genetic or inherent wide adaptation or yield, as Borlaug argued. But, in part due to his connection with India at the time, Borlaug fallaciously promoted wide adaptation as adaptation across agronomic conditions. It is clear that Borlaug, however noble his intentions were, was focused on a specific set of farmers who had access to irrigation and fertilizers, and used wide adaptation to deflect criticisms of bias. Regardless of how the story is framed, there are no easy explanations for why Borlaug held onto the idea of the widely adapted genotype while simultaneously attempting to make farmers' fields look more like experiment station fields.

While Borlaug's argument that selection in favorable environments leads to wide adaptation across conditions has been challenged over the years (Annicchiarico, 2002; Ceccarelli, 1989; Simmonds, 1991), one cannot simply dismiss Borlaug as wrong—one needs to understand history to see just how deeply his ideas have been engrained in modern research. Unfortunately, many of Borlaug's mid-century ideas became solidified in Indian wheat research.

In the late 1960s, India's wheat production rapidly increased in what was heralded as the green revolution. But by the early 1970s, it was clear that the green revolution was not the miracle everyone had hoped. Questions about the impacts of the green revolution were rampant. Although wheat production did increase, there were storage and distribution problems, and further, a string of good agricultural years was succeeded by bad weather in the early 1970s. In retrospect, some scholars have suggested that the rapid gains in wheat production from 1966 to 1971 were more likely a result of good weather

and extensification in response to price supports rather than the genetic benefits of Borlaug's widely adapted, semi-dwarf wheat (Cullather, 2010; Sen, 1974). India was once again importing grains in the mid-1970s, and the spread of green revolution varieties was slowed down as rising energy prices limited the availability of external inputs such as fertilizer and irrigation (Cullather, 2010). In 1973 to '74, wheat scientists noted, "farmers were inclined to revert to local wheats because of input shortages and better prices for the local wheat and straw" (Rao, 1974, p. viii).

Considering the criticisms against the green revolution, several organizations (including the RF) hired external reviewers to assess the food situation in India. The US Agency for International Development (USAID) commissioned Francine Frankel, and the World Bank sent Wolf Ladejinsky to India in the late 1960s (Cullather 2010). Both scholars released reports in 1969 that strongly critiqued the unequal impacts of the green revolution on different socio-economic classes of farmers (Frankel, 1969; Ladejinsky, 1969). Frankel, Ladejinsky, and other contemporary scholars (Abel, 1970; Cleaver, 1972; Sen, 1974; Wade, 1974) all pointed out the shortcomings of the green revolution, including that adoption of new varieties was limited mostly to irrigated areas, and that farmers with more capital benefitted more from new technologies, despite that many scientists claimed that the seeds and technologies were "scale neutral."

Then in the early 1970s, scientists became concerned about the "genetic erosion" of agricultural crops that were vulnerable to biotic and abiotic stresses (National Research Council, 1972). These claims, which still exist today, tend to ignore that northwest India was already a high production area and ruled by only a few varieties before the green

revolution (Smale et al., 2008; Wood & Lenné, 1997). Yet the spatial diversity of wheat varieties remained the lowest in this area of 2001, due to the dominance of a few varieties and a slow replacement rate of these varieties (Mohan et al., 2001). It is less clear how the green revolution has affected agrobiodiversity in northeastern India, where wheat was not a primary crop.

A 1973 USAID report found that the green revolution had not impacted the availability of cereal grains compared to population in India. Even the RF staff in India wrote, “India has made no real progress in improving her people-food equation in the decade of 'the green revolution' and there's no new agricultural technology on the drawing board as glamorous and promising as was the HYV's [high yielding varieties] at the beginning of the decade” (Rockefeller Foundation Indian Agricultural Program, 1973). This was despite the clearly rising production of wheat (Figure 1), which was considered more successful than the introduction of green revolution rice.³

Past a slight nadir in the 1970s due to the slowed adopted of new varieties and rise in energy prices, wheat production in India has continued to rise at a roughly linear rate (Figure 1). This corresponds with fertilizer production and consumption that have also increased to several times over the levels in the 1960s (Figure 2), and also the expansion of irrigation throughout India. Yet net foodgrain availability per capita per year has not increased since the green revolution, and availability of protein-rich pulses has declined and while wheat has increased (Figure 3). While the adoption of green revolution

³ Rice varieties developed in the Philippines were not locally adapted, and were susceptible to pests (Kalirajan & Shand, 1982; Sen, 1974)

varieties, technologies, and policy changes has led to an increase in food production that has kept pace with population growth, hunger and malnutrition remain pervasive problems in India due to regional disparities in production, poor distribution systems, and wasted grain due to improper storage.

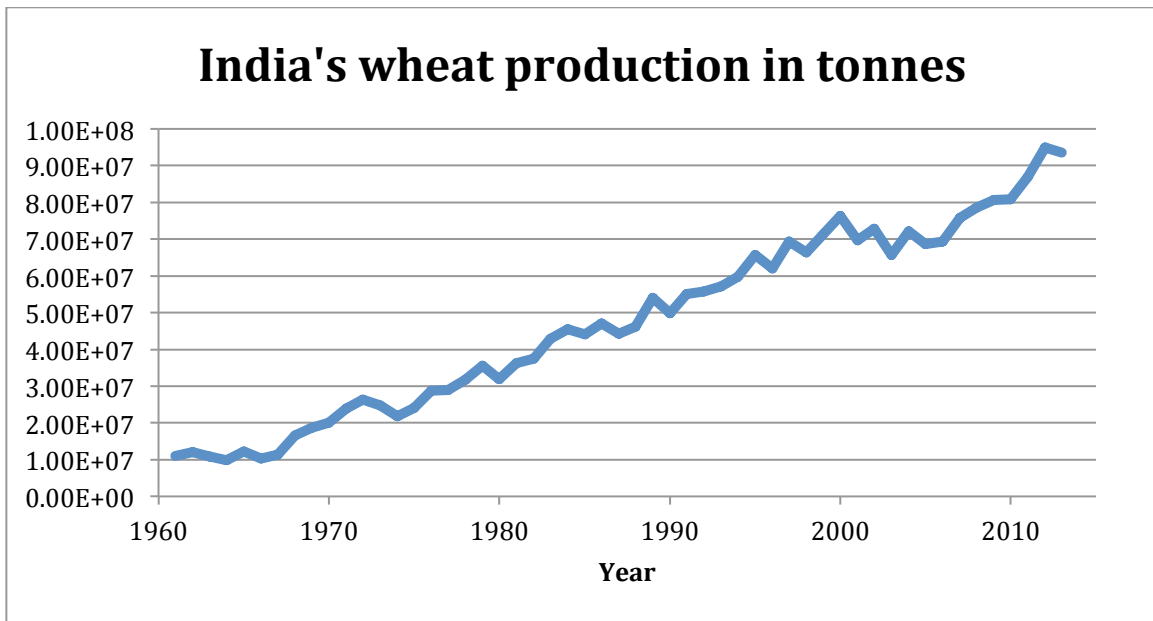


Figure 1. India's wheat production in tonnes, from 1961 to 2013. Data retrieved from FAOSTAT.

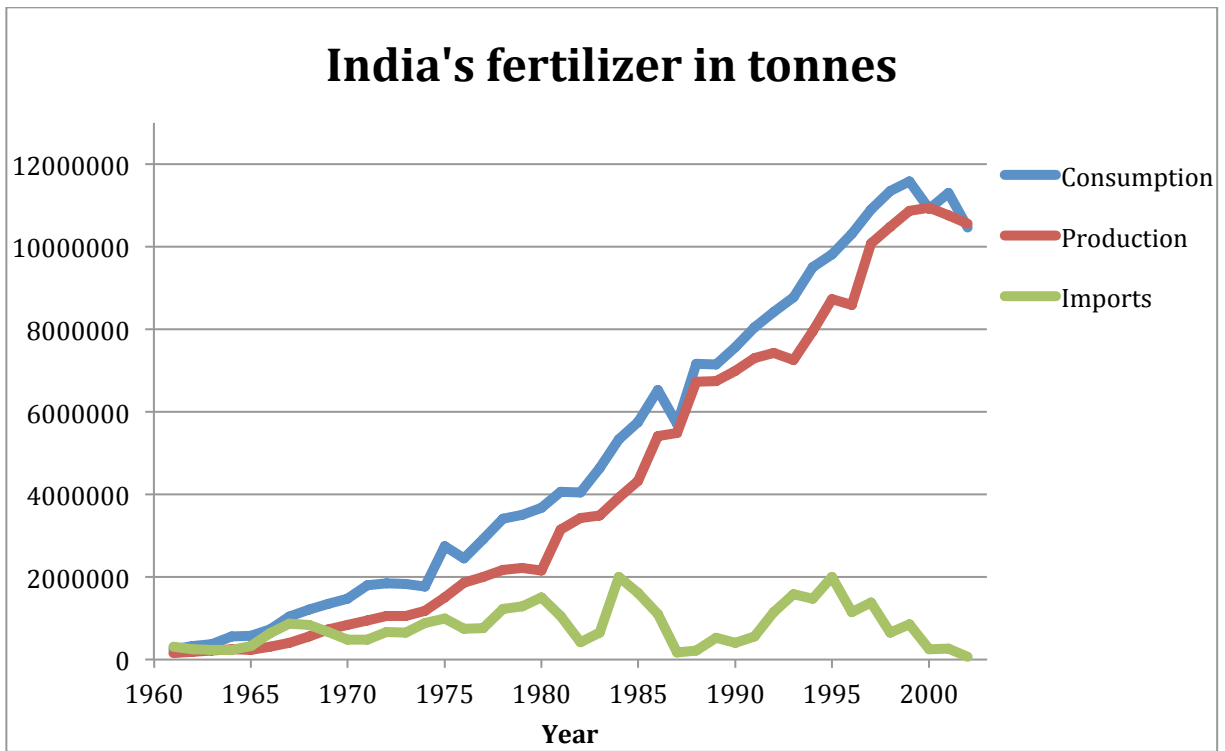


Figure 2. India's fertilizer consumption, production, and imports in tonnes from 1961 to 2002. Data retrieved from FAOSTAT.

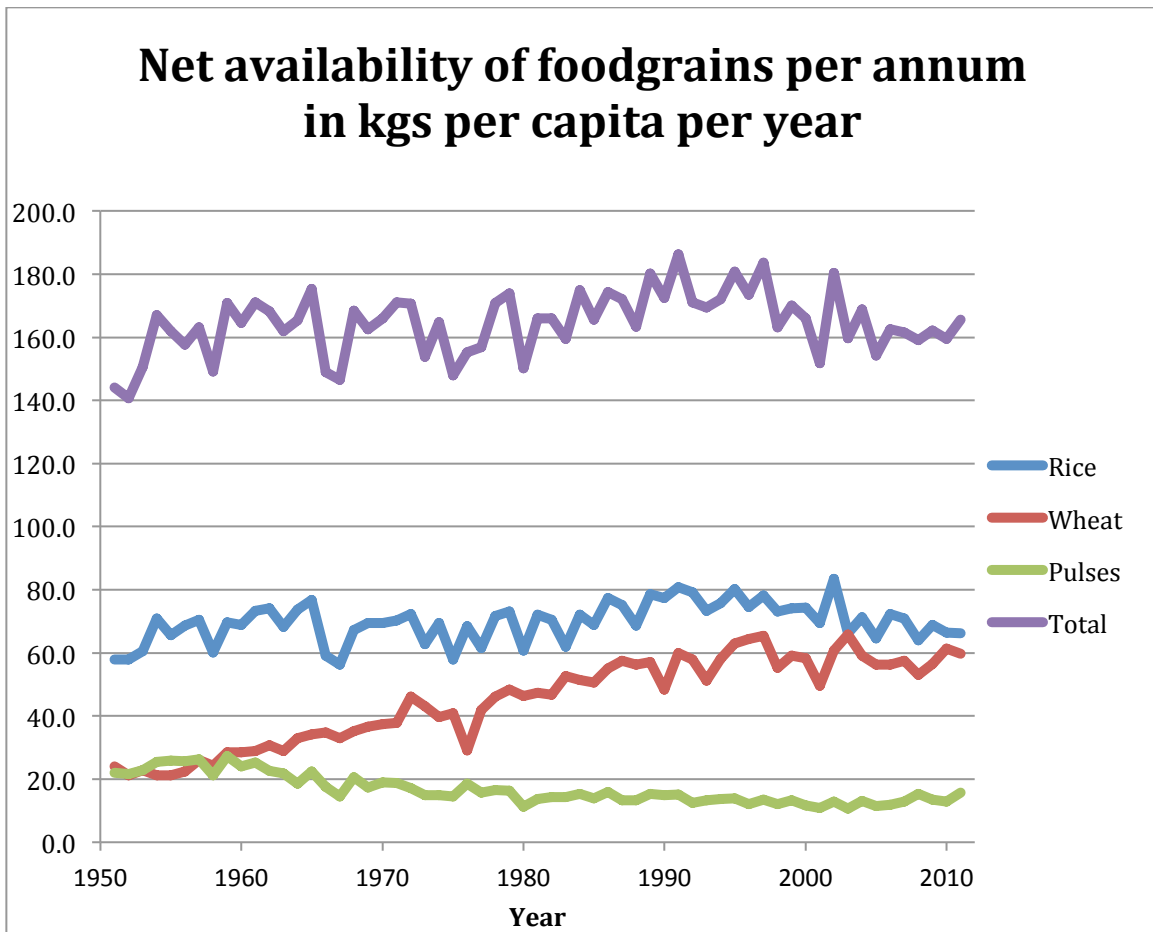


Figure 3. Net availability of foodgrains in India for rice, wheat, pulses and total. Data retrieved from Directorate of Economics and Statistics (2006) and (2013).

Theory: Initial Conditions of Research Trajectories

Borlaug discovered wide adaptation over 55 years ago, and despite a rich literature challenging the philosophy of wide adaptation in plant breeding, several research organizations dogmatically hold wide adaptation theories and practice. These include the IARI as well as CIMMYT, which emerged from the RF's Mexican Agricultural Program. To explain the dominance of wide adaptation in these organizations, I invoke the work of several scholars who highlight the importance of the initial conditions of an organization's development in determining its evolution. As a

result of being set up to address the specific problem of overall food production in the 1960s, these organizations have struggled to address new paradigms of agricultural development, particularly ecological problems, social equity, and participatory research. In short, these organizations suffer from a type of institutional path dependency, relying on outdated narratives, models, and institutional norms established during their formation, and which makes outdated concepts like wide adaptation invincible to critique.

This theoretical orientation turns wide adaptation into a science policy question that should concern administrators in India, the World Bank, and USAID. Forty years ago, the economist Keith Griffin stated:

There is no reason why plant research cannot be directed toward developing improved varieties which reflect the factor endowments and ecological conditions which most Asian farmers confront. The desired physical properties of plants can be predetermined by science policy advisors, and the research biologists can then be asked to design such a plant... In other words, science policy could be used to push technical change in a 'peasant-biased' direction. (Griffin, 1974, pp. 78–79)

Indeed, science policy could be utilized to seriously address the dominance of wide adaptation. But because wide adaptation is so embedded in research trajectories, it seldom comes up for discussion. This is very much relevant to current policies and research investments towards agricultural adaptation to climate change.

Both CIMMYT and the Indian wheat research program emerged in the 1960s, and were strongly shaped by the social influence of Borlaug and Cold War rhetoric around

population growth and hunger. In both wheat programs, Borlaug canonized wide adaptation. The specific conditions under which the research systems were formed still influence the trajectory of current programs. The relative success of the green revolution in increasing aggregate wheat production has allowed both research programs to continue along these same pathways: focusing on improving wheat germplasm through a centralized breeding system, and then post-hoc testing for adaptation. As discussed, both the breeding and testing systems are biased towards favorable environments, but wide adaptation is held up as an ideal to deflect criticism. Indeed, many of the wheat varieties developed by CIMMYT and its predecessor organizations did have an impressive global spread. But as stated, this was due to the convergence of photoperiod insensitivity and fertilizer responsiveness in new varieties. RF-sponsored breeding programs for maize and rice have not been as successful at developing a so-called universal variety.

Recent literature has discussed the green revolution narrative as a useful explanatory framework to examine modern agricultural research (Cullather, 2010; Patel, 2013; Thompson & Scoones, 2009). Cold War historian Cullather wrote, “Development should be analyzed not as a process or an outcome but as a narrative strategy” (Cullather 2010, p. 183). In his 2010 book, Cullather examined the power of narratives and models of agricultural development as a tool of modernization, writing that, “A characteristic feature of modernization studies is *modeling*, the dissection of case studies with the aim of revealing generalizable principles that can be applied in other circumstances” (Cullather 2010, p. 645). As Patel (2013) and Thompson and Scoones (2009), among others, have shown, narratives around the green revolution are still incredibly pervasive

and influential in the international agricultural research system. Emerging during the Cold War, green revolution technologies became an important symbol of international development and geopolitical intervention (Westad, 2005). Thus, Chapter 1 will focus on the Cold War context of the green revolution, the emergence of green revolution narratives, and Borlaug's wheat program as a model for other international agricultural programs. Chapter 2 focuses on some of the individual actors who played important roles in creating the green revolution narratives around widely adapted wheat.

Other scholars have analyzed narratives not just for their pervasiveness in modern agricultural development, but also for their ability to limit alternative innovation pathways. Leach, Scoones, and Stirling wrote about the “pervasive tendency... for powerful actors and institutions to ‘close down’ around particular framing, committing to particular pathways that emphasize maintaining stability and control (Leach, Scoones, & Stirling, 2010, p. 5). Their aim, in examining the mid-century framings of international development, “is to open up debate about the array of socio-technical trajectories” (Leach, Scoones, & Stirling, 2010, p. 161). My dissertation holds a similar goal of re-opening debate around wide adaptation in order to examine plausible alternatives.

Hall *et al.* more explicitly examine the limits of present-day agricultural innovation systems (2000). They wrote:

At the risk of over-simplification, the green revolution was concerned with increasing the productivity of agriculture in order to increase the aggregate food supply. This was seen as a way of reducing hunger and the poverty associated with it. The institutional arrangements to achieve this goal were consistent with

prevailing ideas concerning the organization of science and its relationship with innovation and economic production; namely that centralized scientific research institutes could solve the generic problem of increasing the biological potential of important food crops and that this would lead to increased food production. The task to be achieved was conceptually quite simple and all the actors in the system charged with achieving it held a similar clarity of purpose. However, as the policy agenda has moved away from articulation in broadly scientific terms to one articulated in more developmental terms, existing research structures have had increasing difficulty making satisfactory contributions... Often these new agendas actually conflict with traditional internally driven policies and beliefs of the research sector, particularly where these remain focused on production and productivity and continue to reflect the food security concerns of an earlier period (Hall et al 2000, p. 72 & p. 81)

My research results are consistent with this conclusion, and would add to this the pervasiveness of “black boxed” historical concepts like wide adaptation that limit innovation pathways.

Obviously, the narrative of the green revolution is important to current agricultural research. But how do narratives, models, and technological trajectories become embedded into an organization over time? Several factors are important, including: physical infrastructure, norms and values, and bureaucratic regimes and rules. One theory that unifies these factors is the idea of technological momentum as described by Thomas P. Hughes (1994). Hughes developed the concept of technological

momentum to explain the evolution of large-scale technological systems (1994). Large-scale technological systems contain both social and technological—and human and non-human—actors (Hughes, 1994). These systems initially develop to reflect political and social influences, but eventually acquire momentum of their own due to the passage of time and added physical infrastructure. According to Hughes, older systems are more immune to outside forces because of the momentum they have acquired. Technological momentum helps explain why older large-scale socio-technical systems appear to act in technological determinist ways.

If we can view the Indian wheat research system as a large-scale socio-technical system (I believe we can), technological momentum helps explain why, despite so many calls for reform, the Indian coordinated wheat system has remained a top-down, centralized, and monolithic system of varietal research and testing that favors wide adaptation and excludes potentially complementary location-specific research.

Physical infrastructure. CIMMYT's and other international agricultural research centers' strong historical reliance on plant breeding, for example, presents a force of technological momentum as well as institutional path dependency (McGuire, 2008).⁴ The historical pathway of agricultural innovation through plant breeding gained momentum by creating a system of seed banks and research labs that profoundly influences future conceptions of technologies for climate change adaptation. Future technologies are limited due not only to the scientific training, funding mechanisms, and research infrastructure of agricultural systems, but also the continued reliance on narratives about

⁴ For another example, see Sally Brooks, et al. 2009 book: *Environmental change and maize innovation in Kenya: Exploring pathways in and out of maize*.

the green revolution. These factors ultimately limit conceptions of what adaptation to climate change.

Norms and values. In Hughes' theory of technological momentum, systems are most influenced by social and political forces in their infancy. This is consistent with my analysis earlier in this section that the initial conditions of a system's development are critical to its later development. We know that in the agricultural research community, narratives of the green revolution and Borlaug are prevalent. An important question here is why did Borlaug's ideas become so dominant? I believe this is due to the particular qualities of Borlaug's background and personality as well as the socio-political context he operated in. For example, Borlaug possessed a "missionary zeal" that inspired his "wheat apostles," and he worked in the post-WWII era of American interest in modernization and development of poorer countries through science and technology (Shiva, 1991).

Bureaucracy. Hughes wrote that "as a system matures, a bureaucracy of managers and white-collar employees usually plays an increasingly prominent role in maintaining and expanding the system, so that it then becomes more social and less technical" (Hughes, 1994, p. 106). This is supported by the various failures of attempts to diversify the Indian wheat research system over time. For example, the Ford Foundation's Package Programme in India started in 1961 (see Chapter 3 for more information). According to its second report, it aimed to "be a tailor made programme to suit the needs of a particular area which can be adjusted by the local authorities promptly and effectively, as and when the situation changes" (quoted in Sen, 1969, p. 66). But the Package Programme was limited in part because, "the main concern of the Indian

administrative system has been to lay down general patterns of conformity to which the areas must adjust rather than otherwise and leave the least possible discretion to the authorities lower in the hierarchical structure” (quoted in Sen, 1969, pp. 66–67). Indian economist Samar R. Sen found the Indian bureaucracy to be the main barrier to implementing research “directed to local situation at the field level and in the various agro-climatic regions” (1969, p. 123).

In light of the preceding theoretical discussion, this dissertation largely focuses on the initial conditions of green revolution agriculture because of the importance not only historically, but also to present-day science policy debates. Chapters 1, 2, and 3 will examine international and Indian wheat programs up to 1970. Then, Chapter 4 examines CIMMYT’s international wheat program in light of shifting development concerns, including a focus on equity and rainfed areas that began at the end of the 1960s. Finally, Chapter 5 addresses present-day Indian agricultural science policy.

Methods

Historical research. The majority of my dissertation relies on historical documents that I collected at various sites in 2013 and 2014. Documents were selected based on their relevance to research on the agro-climatic adaptation of wheat, the institutional history of relevant organizations, and correspondence between relevant actors. Initial research was restricted to the time period of 1950–1970, but at the Rockefeller Archive Center I collected documents from the 1970s.

Indian archives. In India I drew materials from several agricultural research libraries at the IARI in New Delhi, the Directorate of Wheat Research (DWR) in Karnal,

Haryana, and Punjab Agricultural University in Ludhiana, Punjab. While these libraries did not host any archives, they contained both primary and secondary sources. In particular, the IARI library hosts a wealth of annual reports and conference proceedings from mid-century (and earlier) to present. Sources included published and unpublished material including annual reports, national and international wheat conference proceedings, journal articles, and published books. The annual wheat research workers' workshop proceedings were collected from both the IARI library and the DWR library.

Historical wheat multi-location trial yield data. Due to the unavailability of historical yield data segregated by variety or area covered by different varieties, I relied on reports of the All India Coordinated Wheat Improvement Program to approximate this information. I collected data for every year available from 1965 to present. This data was limited to the Advanced Varietal Trials (formerly Uniform Regional Trials) and for what is currently considered the North West Plain Zone and North East Plain Zone.

Rockefeller Archive Center. I spent three weeks at the Rockefeller Archive Center in April and May 2014. While there, I focused on three topics: the RF's international wheat programs in the 1960s (focused on the office in Mexico), the RF's Indian agricultural program in the 1950s and 60s, and RF's international wheat programs in the 1970s (focused on the Middle East). For the RF's international wheat programs in the 1960s, I was interested in the progression from the Mexican Agricultural Program to the Inter-American Food Crop Improvement Program, then to the International Center for Corn and Wheat Improvement, to eventually the International Maize and Wheat Improvement Center (CIMMYT). To these ends, I examined records from the project

files (RF, Record Group 1.2, Series 300D and 323, and Record Group 1.3, Series 105), administration, program and policy files (RF, Record group 3, Series 915 and 923), Mexico field office (RF, Record Group 6.13, Series 1), officer diaries (RF, Record Group 12), oral histories (RF, Record Group 13), and the RF Agricultural Science Program Annual Reports.

I also focused on the RF's involvement in Indian maize and wheat improvement. Towards this end, I used project files (RF, Record Group 1.2, Series 464D, and Record Group 1.6, Series 464D) and the New Delhi field office records (RF, Record Group 6.7). Finally, I explored the RF's expansion into wheat research in the Middle East in the 1970s, utilizing again the project files (RF, Record Group 1.3, Series 105) and some recently added archival material from the Ankara, Turkey, field office (RF, Record Group 6, Series 19).

Iowa State University Library Special Collections and University of Minnesota Library. Due to the particular importance of Norman E. Borlaug to my dissertation research, I drew from two collections dedicated to Borlaug's papers. The Minnesota archive was only accessed online, thus my browsing was less thorough and restricted to Borlaug's correspondence in the 1960s and his 1967 oral history. At the ISU Library Special Collections, I focused on Borlaug's correspondence with Charles F. Krull and Keith W. Finlay, as well as Borlaug's and other RF scientists' involvement in various FAO programs.

Fieldwork in northern India. Much of my dissertation research was conducted in northern India from January to July 2013. I was financially supported by an NSEP

Boren Fellowship and I was officially appointed as a Research Fellow with Bioversity International's sub-regional office in New Delhi under the guidance of Dr. P. N. Mathur. In accordance with the Boren Fellowship, I completed approximately 200 hours of Hindi language study from the HindiGuru language institute as well as a private tutor.

While in India I traveled to several different agricultural research institutions involved in wheat research. These include: IARI Regional Research Station in Pusa, Bihar (established in 1905, it is the birthplace of wheat research in India, and the former center of IARI until 1934), the Directorate of Wheat Research in Karnal, Haryana (established at Karnal in 1990), Punjab Agricultural University (PAU, established in 1962), and the IARI headquarters in New Delhi (established in 1936). I stayed at each institution for about one week each, (except for the IARI, New Delhi), and spent 2-3 days at each place conducting interviews. The majority of my time in India was spent in New Delhi.

Scientist interviews. From February to May 2013 I interviewed 47 agricultural scientists at four major agricultural research institutions in India. Thirty-two of these interviews were with practicing scientists involved in wheat improvement and extension programs. Main fields of the scientists interviewed included: plant breeding, biotechnology, genetics, plant protection (pathology), quality, agronomy, and extension science (see **Error! Reference source not found.**). Participants were chosen through snowball sampling. At each field site I had a local scientist host who would introduce me to the other scientists based on the criteria I had laid out of scientists involved in the Wheat Improvement Programme and extension scientists. The scientists I interviewed

ranged from junior level plant breeders to senior level project directors. On a whole, my data may be skewed towards more senior scientists since that was often who I was directed to talk to.

Scientist interviews generally lasted between 15 and 60 minutes, and followed a structured questionnaire (see Appendix A). Agricultural scientists were given a specific set of questions, while extension scientists were asked a separate set of questions. Interviews were conducted and recorded in English, and transcribed by the author. A few interviews were not recorded based on the informant’s preference. Transcriptions were not completely verbatim, but reflect the main themes of the conversation. Direct quotes were transcribed from key statements. IRB approval was obtained for all interview questions (including for farmers and administrators), and all interviews were confidential (no names were recorded, and no identifying information will be used in this dissertation).

Table 1

Number of Recorded Interviews with Wheat Scientists, by Field and Institution.

	IARI Delhi	IARI Pusa	DWR	PAU	Total
Plant breeding	2	1	4	3	10
Biotechnology	1	1	4	0	6
Plant protection	1	0	1	3	5
Wheat quality	1	0	1	1	3
Agronomy	0	1	2	0	3
Seed Production	0	1	0	0	1

Extension	0	2	0	2	4
Total	5	6	12	9	32

When circumstances made the questionnaire format incompatible with the situation (such as time constraints, interviews which became off-topic, scientists whose research did not apply to the questionnaire), I would ask unstructured questions that were based on the main themes of the questionnaire. Despite this, the majority of the interviews were structured and most of the questions could be answered to a satisfactory degree.

Based on completeness of the structured interview questions, twenty-five interviews were analyzed and coded. Answers to structured interview questions were put into a spreadsheet and analyzed for keywords and themes. Themes included wheat breeding for location, breeding for agro-climatic conditions, research on stress tolerance, microclimatic factors, and the structure of Indian research. Key quotes were pulled to reflect these themes, including themes to represent opposing viewpoints.

Research administrator and retired scientist interviews. Eight current and retired agricultural research administrators were interviewed. Due to varying time allotments for interviews with administrators, these varied greatly in both questions and content. Administrator interviews were semi-structured and centered around Indian agricultural science policy in general and specifically related to wheat improvement in north India. Two retired wheat breeders at IARI in New Delhi were also interviewed. These interviews followed a similar format as the administrator interviews. Transcripts

from the interviews with research administrators and retired scientists were analyzed qualitatively. Administrators and retired scientists were identified through my local research networks.

CHAPTER 1
THE ROCKEFELLER FOUNDATIONS INTERNATIONAL EXPANSION OF
STAPLE FOOD CROP IMPROVEMENT PROGRAMS, 1940–1970

“*World peace must be based on world plenty.*”
–J. B. Orr, Nobel Peace Prize lecture, 1949

Table 2

Timeline of Chapter 1 Events.

Year	Event(s)
	* Starred points all share institutional origins with the Mexican Agricultural Program (MAP).
1941	Rockefeller Foundation (RF) sends scientists to survey Mexican agriculture
1943	RF and Government of Mexico begin the Mexican Agricultural Program (MAP) under the Office of Special Studies*
1944	Norman Borlaug hired to work with MAP
1948	Fairfield Osborn’s <i>Our Plundered Planet</i> and William Vogt’s <i>Road to Survival</i>
1949	Truman’s Point Four Speech
1950	RF’s Columbia Agricultural Program started
1952	Dean Rusk comes from State Department to become president of RF
	RF sends scientists to survey Asian agriculture
1954	Public Law 480 enacted
	Hugh Everett Moore’s Population Bomb pamphlet
	Central American Corn Improvement Program begins*
1955	MAP starts a research station at Ciudad Obregón, research is focused on ideal conditions
	RF’s Chilean Agricultural Program started
1956	RF and Government of India sign a memorandum of understanding
	RF starts a worldwide maize testing program, expanding to South America, India, Indonesia, and the Philippines
1959	Inter-American Food Crop Improvement Program started*
	RF and Ford Foundation (FF) sign memorandum of understanding with Government of Philippines
1960	Dean Rusk becomes Secretary of State, George Harrar becomes RF president in 1961
1961	PL-480 becomes “Food for Peace”
	MAP transferred to Mexican government and National Institute for Agricultural Investigations is formed*
1962	International Rice Research Institute officially formed
1963	RF and FF form the International Center for Corn and Wheat Improvement*
1965	An El Niño event causes monsoon to fail in India; new varieties of dwarf rice and wheat are released there and rapidly adopted in irrigated areas
1966	Centro Internacional de Mejoramiento de Maíz y Trigo (CIMMYT) / International

	Maize and Wheat Improvement Center formed, RF and FF provide funding but CIMMYT is more autonomous from RF*
1968	“Green revolution” is coined

This chapter examines the origins of the Rockefeller Foundation’s (RF) agricultural programs in the 1940s up to the 1970s. I examine the geopolitical motivations for US-based foreign agricultural assistance in the post-WWII era, including fear of communism and the population problem. Then I discuss the formation of the RF’s Mexican Agricultural Program (MAP), which served as a model for other RF programs. I trace the evolution of MAP into cooperative and international programs for wheat and maize. The next section examines RF-supported rice research in Asia, focusing on the international research center and also the impact of new rice varieties in South Asia. Table 2 shows a timeline of the major political and institutional events that are mentioned in this chapter.

The RF was a major force behind global agricultural development in the mid-twentieth century, and helped establish international research centers and networks. RF-affiliated programs, especially for wheat and rice, not only transformed agricultural landscapes in the developing world, but also set a new standard for international agricultural development based around a program of crop genetic improvement. In agricultural programs aimed to aid tropical and semi-tropical developing countries, RF scientists challenged many of the existing dogmas in plant science. Throughout my dissertation I focus on the RF’s philosophy of wide adaptation, meaning a variety that produced high yields over wide geographic locations and/or agro-climatic conditions (though I will debate the latter claim). This philosophy emerged in tandem with the RF’s

focus on breeding varieties for ideal conditions (high fertility and controlled irrigation), leading to a research regime and technologies that problematically benefitted larger, wealthier farmers. But RF scientists assumed that through wide adaptation, they could overlook socioeconomic or agro-climatic factors and develop varieties embedded with inherently high yields. This perspective is closely tied with high modernist beliefs that prevailed at the time in the RF and in countries like Mexico and India (Scott, 1999).

To understand how RF scientists adopted wide adaptation as a main goal of their international plant breeding program, this chapter will discuss the history of the RF-sponsored programs in Mexico, the Philippines, and India for the crops of maize, rice, and wheat. These countries and crops were major foci of the RF's agricultural research program in the 1940s through 1960s. I will use primary historical sources to demonstrate the motivations of RF administrators and officers, specifically on concepts related to agro-climatic adaptation.

Green Revolution Theory and Terminology in Agriculture

In 1968, US Agency for International Development (USAID) administrator William Gaud declared the "green revolution" as an alternative to a socialist Red Revolution. In retrospect, the green revolution is typically defined as the rapid transformation of agricultural technologies and practices in the 1960s and 1970s (and even up to the 1990s) in Latin America and Asia. Political ecologist Keith Griffin, however, pointed out that those recalling the green revolution usually refer to either the "broad transformation" in agriculture or "specific plant improvements" without differentiating between the two (Griffin, 1974, p. 2). Historian Deborah Fitzgerald argued

that economists and policymakers tend to refer to the former, but that this perspective ignores the RF's agricultural program in Mexico that began in 1943 (Fitzgerald, 1986). Because my dissertation primarily focuses on the research, rather than the impact, of the green revolution, I adopt Fitzgerald's perspective that the green revolution research-era spanned the 1940s to the 1970s, starting with the RF's involvement in Mexican agriculture. I examine the "specific plant improvements" as not necessarily endogenous technological drivers of change, but as part of a system where personal and political motivations as well as socio-economic and biophysical contexts also play a role. While the green revolution has had impacts beyond the 1970s (Evenson & Gollin, 2003), the 1940s through 1970s were characterized by a rapid expansion of agricultural modernization programs and the diffusion of new packages of agricultural technologies.

There are many fields of scholarship that address the green revolution. Among these are agricultural economics, agricultural science, political economics, critical political ecology, sociology, anthropology, geography, history, development studies, and science and technology studies. This chapter will draw primarily from critical political ecology and the history of science. These literatures help contextualize the expansion of the RF's international agricultural program in the 1960s.

Setting the Political Backdrop to International Agricultural Assistance

Agricultural research profoundly transformed during the 20th century. The rediscovery of Mendelian genetics, the invention of hybrid maize, and the advent of synthetic fertilizers all restructured the very business of agricultural research. These technological achievements fueled the ideology of international development through

modernization of agriculture, held by many RF administrators as well as US politicians and policy advisors in the 1960s. The modernization ideology replaced previous modes of development such as New Deal-style progressive schemes and Tennessee Valley Authority-type technical projects (Cullather, 2010).

The ideological backdrop of modernization and other mid-century sociopolitical contexts is important to understand how a small group of American plant breeders, affiliated with the RF, became *de facto* diplomats of statecraft and agricultural interventions in several developing countries. The esteem in which US policy makers and advisors held these scientists, and the many ties between the US foreign policy community and the RF, are also critical to explain this particular era of plant scientists as state-builders. Before getting into that part of the story, some more background is necessary.

The emergence of agricultural modernism began after World War II, although the ideological roots of modernism can be traced much further back (Adas, 1989; Cowen & Shenton, 1996). In the immediate post-WWII era, the US entered into international development through policies such as the Marshall Plan to help rebuild post-war Europe. Counter to previous isolationist policies, the US now involved itself in international food politics as a form of development. Before WWII and in the 1940s, historian John Perkins wrote that, “no analytical framework existed to see how agricultural science and technology and modernization of agriculture fit into the overall scheme of international relations and power” (Perkins, 1997, p. 103). Food aid and technical assistance programs were *ad hoc* and not unified by any national policy. Yet, the programs did have obvious

political implications, as Cullather stated, “the construction of postwar order began with food” (Cullather 2010, p. 34).

Globally, agricultural development shifted from an imperial to a geopolitical regime. Countries like India and the Philippines gained mid-century independence from their colonial rulers. Simultaneously, the RF and other agencies, working with local governments, rapidly proposed agricultural modernization schemes for these countries—the so-called developing world. These agricultural development schemes rested on assumptions that other countries needed to “develop” along a “social evolutionist teleology” called modernization theory (Adas, 1989, p. 412). Modernization theorists held that history could be “sped up” and that societies could become modern given the right set of knowledge and inputs (Latham, 2011). This perspective heavily relied on technology as a tool of development (Westad, 2005).

Scholars widely cite President Harry S. Truman’s inauguration address of 1949 as the beginning of formalized international scientific assistance. Referred to as Point Four, Truman stated, “Fourth, we must embark on a bold new program for making the benefits of our scientific advances and industrial progress available for the improvement and growth of underdeveloped areas” (Truman, January 20, 1949). This fourth point of his speech highlighted the use of scientific and technological assistance as a tool of democracy. Thus international food aid, primarily through grain exports, gained attention in national political discourse, and was implemented through policies such as the Eisenhower administration’s Public Law 480 in 1954 (the Agricultural Trade

Development and Assistance Act, or PL-480).⁵ President John F. Kennedy renamed PL-480 as “Food for Peace” in 1961.

Modernization theory was closely tied to the west’s anti-communist agenda. Economist Walt Whitman Rostow’s 1960 famous book on modernization theory, *The stages of economic growth: A non-communist manifesto*, made this explicit (Westad, 2005, p. 33). In the Cold War battle of western values versus communism, the fate of developing countries became symbolic of their respective ideologies. Historian Odd Arne Westad wrote that, “Washington and Moscow both needed to change the world in order to prove the universal applicability of their ideologies, and the elites of the newly independent states proved fertile ground for their competition” (Westad, 2005, p. 4). Newly independent states, as well as other developing countries, became known as the “Third World” in the 1950s to represent their potential mobilization (based on the French prerevolutionary “third estate”) as well as their non-alignment status in the Cold War (Westad, 2005).

The convergence of technological optimism and foreign intervention through agriculture was a remarkably shared between the consecutive US presidents Eisenhower, Kennedy, and Johnson. The common political advisors of these presidents (mostly Kennedy and Johnson) explains some of this convergence. Historian David Ekbladh wrote that foreign policy advisors Rostow, William Bundy, McGeorge Bundy, Robert Komer, Dean Rusk, and Robert McNamara all “nurtured a belief in the transformative power of development led by the United States” (Ekbladh, 2002, p. 262). Rusk, Komer,

⁵ The US faced a massive surplus of grain throughout the 1950s and into the mid-1960s. See Ahlberg (2007).

and Rostow were on Kennedy's National Security Council and helped Kennedy establish the 1960s as the Decade of Development (Cullather, 2010; Latham, 2011). The US's focus on international development through technology continued through the Johnson administration.

As historian John Perkins explained in his 1997 book, *Geopolitics and the green revolution*, Cold War intellectuals made the link between population, hunger, and national security, theorizing that overpopulation and hunger would cause political instability in the developing world. Historian Alison Bashford wrote that “the problematization of population often raised questions about and plans for migration, colonial expansion of territory, and the properties of land and soil: in other words, geopolitics” (Bashford, 2008, p. 328). Perkins called this constellation of geopolitics the “population-national security theory” (PNST) (*Figure 4*), which was the dominant Cold War-discourse linking dense populations to areas ripe for communism, thus framing foreign population control, food security, and pacification as a national security strategy (1997, p. 118).

**Overpopulation → Resource exhaustion → Hunger → Political instability
→ Communist insurrection → Danger to American interests → War**

Figure 4. Representation of John Perkins' “population-national security theory” model.

My research on American foundations in the 1940s through '60s supports the PNST model. The key takeaway here is that modernization theory and the PNST model linked hunger and war, and policymakers saw fighting hunger as an attainable intervention. This brought plant breeders directly into Cold War politics.

Neo-Malthusianism

Simultaneous to the US's involvement in international technical assistance, fears of the population bomb, or population problem, escalated in the 1940s and into the 60s as a foreign policy concern. Neo-Malthusianism resurged in the U.S. starting in the 1920s due to the development of demography and population models (Bashford, 2008; Cullather, 2010; Ramsden, 2002). Thomas Malthus became famous for his theory of population growth in 1798, *Essay on the Principle of Population* (Cowen & Shenton, 1996). He proposed that population expands at an exponential rate, while food production grows at a linear rate, meaning that population will eventually outpace the food supply and result in hunger and starvation (Perkins, 1997). Around the 1930s, US and British academics starting becoming concerned about overpopulation in what is now called neo-Malthusian thought (Perkins, 1997). Neo-Malthusianism was further bifurcated into political neo-Malthusians, who were concerned about the effects of overpopulation on geopolitics, and ecological neo-Malthusians, who were concerned about the outstripping of natural resources and possible ecological collapse due to overpopulation (Perkins, 1997).

In 1948, two publications, Fairfield Osborn's *Our plundered planet* and William Vogt's *Road to survival*, raised fears of ecological crisis. Fears of the population bomb were further sparked by Hugh Everett Moore's eponymous popular pamphlet in 1954 (Schlosser, 2009). While campaigning in 1952, Eisenhower spoke that "Our nation is moving swiftly into an entirely new balance between our population and our food supply" (quoted in Cullather, 2010, p. 104). By 1964 US foreign policy focused on the

“gap, in certain areas, between the rate of increase in population and the rate of increase in local food production, yielding a slide into increased dependence on U.S. food surpluses” (Rostow, October 28, 1964). The population problem was high on the U.S. national agenda as it peaked in the 1960s and 1970s, when neo-Malthusian theories were appropriated by the modern environmental movement (Hughes, 2000).

Malthus’ ideas captured collective imaginations of fear and disaster. But the 1960s brought optimism that these problems could be solved through science and technology. Paul Mangelsdorf, a plant breeder from Harvard who consulted for the RF in Mexico in 1941 and India in 1951–1952 (Perkins 1997), wrote in an essay in 1961 on food and population that stated, “I think that as biologists we can all agree that the first of Malthus' principles is absolutely valid” (Mangelsdorf, 1961, p. 280). Rusk, past president of the RF and now Secretary of State, testified to Congress in 1966:

The root of the problem is clear enough. In many parts of the world, food production is beginning to lag behind population growth.... With limited land availability, our best hope is to obtain more food from lands already under cultivation... better seeds, fertilizers... and other miracles of technology can multiply food production many times. (Food for freedom program and commodity reserves, 1966, pp. 191–194)

Thus, neo-Malthusian fears were infused with a modern sense of technological optimism. One of the most notably optimistic promoters of science to fight population was Nobel Prize-winner John Boyd Orr (Bashford, 2008), who wrote with David Lubbock in 1953, “Modern science has the answer to Malthus” (Orr & Lubbock, 1953, p. 80).

US Foundations embraced technological optimism over neo-Malthusian pessimism. Cullather wrote that the Rockefeller and Ford Foundations were among the first to promote “food-first” strategies as a means of development and in response to the population problem (2010). Warren Weaver, director of the RF’s Natural Sciences division, was particularly convinced that modern technology could increase agricultural production. He stated in 1951 that, “Agriculture is nothing more than the application of the principles of biology and other natural sciences to the art of growing food.” (Advisory Committee for Agricultural Activities, 1953, p.7). Weaver was also responsible for the RF’s conception of growing more food per area, which soon became central to the international discourse on population growth that the RF relied on for 30 years (Cullather, 2010). The conception of increased yield per area as a solution to population growth fit among existing concerns of rapidly growing, urbanized populations leading to soil erosion of surrounding agricultural lands (Bashford, 2008). Coupled with the scarcity of new agricultural land, this almost naturally pointed to fertilizer and higher yielding crops as a solution to this mismatch of space and production. The perceived limit of available land, along with demographic changes like urbanization, led to the conceptualization of space where increasing yields per area were the most desirable solution (World Population and Food Crisis, 1965). Further, the RF envisioned higher yields as a radical force of modernization that would lead to other progressive social changes (Cullather, 2010).

In the later 1960s, however, the post-war technological optimism faded and declensionist narratives resurged. In a Congressional hearing on the green revolution in

1969, RF's William Myers warned that if developed countries fail to provide continued support to developing ones, "We will sink once more into a sea of despair to wait for famine and chaos to overwhelm us" (The Green Revolution, 1969, p. 13). By the early 1970s, the fears of overpopulation were far from over. Secretary of State William Rogers spoke to Congress in 1971:

The need remains as great as it was when Point Four was proclaimed to apply technology to the problems of development. Indeed, population pressure in some nations is so acute that even the greatest agricultural breakthrough in decades—the development of the grain which created the green revolution—has only... "bought time." (Panel on science and technology, 1971, p. 3)

Works such as Paul Ehrlich's *The population bomb* (1968), Garrett Hardin's "Tragedy of the commons" (1968), and the Club of Rome's *Limits to growth* (Meadows et al., 1972) further contributed to an aura of pessimism (Hughes, 2000). A 1974 CIA report stated, "there are some factors pointing to the danger—even the likelihood—of a Malthusian crisis in India in the years ahead" (U.S. Central Intelligence Agency, June 1974).

The Rockefeller Foundation and Agricultural Development

The Rockefeller Foundation began supporting agricultural research in the early 1900s (Fitzgerald, 1986). Many of the RF's programs at this time aimed to apply scientific research to social problems such as public health and population. The RF's first agricultural program operated in the southern United States starting in 1906 under the General Education Board (Jennings, 1988). Then in 1924 RF started a technical assistance program in China that included an agricultural program, in order to raise rural

standards of living (Perkins, 1997). In the 1940s, the RF became involved in agricultural research in Mexico.

Several factors prefaced the RF's Mexican Agricultural Program (MAP), often referenced as the start of the green revolution (Fitzgerald, 1986). First, during World War II President Roosevelt focused on strengthening economic ties in the Americas in order to resist communist and fascist powers (Lewontin, 1983). Also during WWII, the RF had to end their programs in Europe, and it saw moving to Latin America as a natural adaptation of these programs. As historian Nick Cullather described, the RF saw Mexico as a "surrogate" for other developing countries (2010, p. 43). Scholars have debated what motivated the RF's MAP, as Stephen Lewontin pointed out, "critics of the green revolution in Mexico have argued that political motivations and the associated ideological and cultural biases determined the program's outcome" (1983, p. 73). He argued instead that the politics were more complex, and that painting the MAP as a picture of imperialistic motives of the RF ignores the various interests of both Mexican and RF scientists, other actors in Mexico, and the existing social structure of Mexican agriculture (1983).

What we do know about the origins of the MAP is that it began as a rural development program. We also know that in the 1940s, the US government, particularly secretary of agriculture Henry A. Wallace, encouraged the RF to become involved in Mexican agriculture and framed it as a "yield per acre" problem (Cullather, 2010, p. 57). The US government also saw the RF as a way to support developing countries through a more autonomous and less politically charged organization than the US Department of

State (Cotter, 2003). There was also a direct connection with Dean Rusk, who came to the RF from the State Department in 1952 to become president of the RF until he became Secretary of State in 1960 (Anderson, Levy, & Morrison, 1991).

The US public and policymakers saw post-war Mexico as a success of foreign economic development (Cullather, 2010). But fears of an apocalyptic population explosion in Mexico started in 1948 and into the 1950s, and this shifted the meaning and focus of the MAP (Cotter, 2003; Cullather, 2010). While Mexico became a national security concern to the US, the RF did not explicitly play into these fears of peasant uprising in Mexico. But they did co-opt the population discourse, choosing to highlight the success of their agricultural modernization program in Mexico.

Starting the Mexican Agricultural Program

In 1941 the RF sent a group of scientific advisors to Mexico to survey the possibilities for an agricultural program in Mexico. Elvin Stakman, Richard Bradfield, and Paul Mangelsdorf, all professors of agricultural science (from University of Minnesota, Cornell, and Harvard, respectively), went to Mexico and developed a set of recommendations for a technical assistance program (Perkins, 1997). Their recommendations included: improving maize, wheat, and bean varieties; and better agronomic management practices, ostensibly under the leadership of American scientists (Jennings, 1988).

The RF entered into an agreement with the Government of Mexico in 1943 that would be known as the RF's Mexican Agricultural Program (MAP) under the Office of Special Studies. MAP was hosted by the Chapingo Autonomous University, outside of

Mexico City (Cullather, 2010). The MAP started under the scientific leadership of J. George Harrar and initially focused on improving maize, beans, and wheat for Mexican conditions. The RF aimed to bring agricultural expertise to problems facing Mexican agriculture, such as breeding varieties of wheat that were resistant to stem rust, and developing improved varieties of maize (Fitzgerald, 1986; Perkins, 1997). Norman Borlaug was hired in 1944 to work with the wheat program. Borlaug and Harrar had both done their doctoral work with Stakman at Minnesota (Perkins, 1997). Many of the important details of the MAP are well documented in the secondary literature, therefore the next sections will focus on some of the scientific aspects of the MAP and how the MAP became a model for international agricultural development.

Wheat and maize from the US and Canada were generally poorly adapted to Mexican conditions due to different lengths of daylight and seasons. Therefore MAP scientists realized that improved varieties for the semi-tropics needed to be derived from gene pools in the tropical, rather than temperate zones (Dahlberg, 1979). Under Borlaug's supervision, MAP released new rust-resistant spring wheat varieties in 1949, and by 1957 these new varieties constituted 90% of Mexican wheat acreage (Fitzgerald, 1986). Borlaug later discovered that wheat crosses between certain foreign strains produced varieties that could be grown over wide geographic areas. He stated at a 1960 meeting that "wheat is very different from corn in that it appears to be much more flexible in its adaptation to different soils and climatic conditions" (*Notes on the Consultants' Meeting*, 1960). This would become the basis of Borlaug's international wheat program, discussed in more detail in Chapter 2. This flexibility was due to their photoperiod insensitive

properties—a dominant genetic trait—that allowed them to grow under a variety of altitudes and latitudes, unlike Canadian and American wheat varieties (Borlaug, June 24, 1960; Trethowan et al., 2007). While Borlaug’s discovery affected wheat only, the maize and rice programs also aimed at producing photoperiod insensitive varieties that could be grown over wide geographic regions.

Borlaug’s discovery contradicted what many scientists, even Harrar, presumed at that time, which was that agricultural assistance programs necessarily had a limited geographic scope. In his 1961 oral history, Harrar, then-president of the RF, stated:

Unfortunately, most scientific advances most directly benefit the particular geographic area in which they originated. This is especially true in the agricultural sciences. Of course, many basic principles are discovered which can be useful on an international front, on a broad front, but the application of those principles depends upon local climatic conditions and on many other factors. ([Oral history of J. George Harrar], 1961–62, p. 38)

Harrar’s statement stands in contrast to statements from Borlaug at that time.

Harrar also pointed out two other principles of the MAP: 1) collecting and testing plant germplasm and 2) testing finished varieties under controlled conditions. He noted, “one of the things we did was bring together the varieties of crop plants on which we were working from all of those parts of the world where climatic conditions had reasonable similarities... you don't know what to throw away until you get them together and test them” ([Oral history of J. George Harrar], 1961–62, p. 39). Secondly, Harrar stated that, “the plant itself tells you how many bushels you will get per acre, and the

only way you can find that out is by growing it under controlled conditions, theory to the contrary” ([Oral history of J. George Harrar], 1961–62, p. 39). Stakman, an advisor to the RF, echoed these principles in his own statement that, “wheat and corn and beans are grown under many different conditions and the varieties suitable in one area may not be suitable in another; these facts must be determined by experimentation” [Oral history of Elvin Charles Stakman], 1966–67, p. 9). These principles of germplasm collection and testing became standard for the RF’s later international agricultural programs.

The other principle that Stakman advised the RF to follow came in 1953, when he wrote to RF’s president Rusk:

It would be possible, and possibly desirable, to concentrate on incorporating into lines of the principal food crops the best possible combination of genes for yielding ability, disease resistance, or any other universally useful character, *without considering adaptability to particular areas*. These lines could then be given to breeders in all interested countries for use in developing varieties adapted to their conditions... others [problems], such as improvement of crop varieties, could be put on a regional or international basis where ecological zones extend over several countries. [emphasis added] (Stakman, December 22, 1953, p. 11)

By the early 1960s, Stakman’s thoughts on developing broadly adapted varieties became a reality through Borlaug’s photoperiod insensitive wheat varieties. Throughout the 1960s, the RF-sponsored programs focused on developing a few widely adapted varieties of wheat, rice, and maize that could be utilized in many countries (Smith, 2008). While

each program has continued up to the present, wheat was the most successful at proving itself as widely adapted.

Wheat and Maize Research Programs in Mexico: Goals vs. Impacts

By the 1950s it was clear that the MAP would primarily focus on maize and wheat improvement, and less on beans and other crops. And, as many scholars have pointed out, the programs would focus on one “isolable technical problem”: raising yields per acre, primarily through germplasm improvement (Harrar, Mangelsdorf, & Weaver, 1951, quoted in Anderson, Levy, & Morrison, 1991, p. 35). The MAP’s wheat program was incredibly successful in irrigated central and northern Mexico, with farmers clamoring for Borlaug’s newly-released, disease-resistant varieties. The maize program was less successful, however. While the RF’s new wheat varieties were rapidly adopted by farmers in Mexico’s Pacific Northwest, farmers in central Mexico adopted new maize varieties at a much lower rate. By the mid-1960s, farmers grew over 95% new wheat varieties, but only 13% new maize varieties (Griffin, 1974).

A number of factors belied the different rates of adoption of RF varieties of wheat and maize in Mexico. First, the crop programs, led by their respective RF scientists, had different goals. Sterling Wortman, who worked for MAP and later directed RF’s Agricultural Sciences program recalled:

In the corn program we primarily had a plant breeding effort under way, not a comprehensive production program... We were concerned very much with the problems of developing many varieties of hybrids needed for the great number of ecological situations in Mexico... The wheat program on the other hand was

concerned not only with development of rust resistant, high yielding varieties but with seed production, the use of higher amounts of fertilizer, and adoption by farmers. (Wortman quoted in Myren, 1969, p. 441)

The biophysical properties of wheat and maize also varied: wheat was self-pollinated, which allowed farmers to multiply and save their seed, but hybrid maize required farmers to repurchase seed each year. Hybrid maize also appeared to be less widely adaptable than wheat. Maize varieties required specific adaptation to local conditions, prohibiting the wide spread of one or a few varieties. Myren wrote that, “In order to obtain hybrids that yield better than local native varieties, it has generally been necessary to develop them for specific climatic conditions” (1969, pp. 446–7).

A final factor in the relative success of the RF’s maize and wheat programs was the different characteristics of farmers and the agricultural landscape for maize and wheat in Mexico. Most maize farmers were subsistence farmers on small tracts of land. They relied on rainfall for irrigation, and were mostly located in the climatically variable central Mexico, where the main demand for maize was for tortillas. Myren estimated that there were “40 times more corn farmers than wheat farmers, and consequently 40 times as many decisionmakers to be reached with information about new production practices” (1969, p. 444). Twenty years after MAP started, only 9.9% of Mexico’s maize-growing area was irrigated in 1962 (Myren, 1969).

Edwin J. Wellhausen, a maize geneticist for the MAP from its beginning, and director starting in 1951 (later director of CIMMYT), apparently understood the many challenges faced by maize farmers (Jennings, 1988). In his oral history, he explained how

maize farmers in Mexico were different from maize farmers in the US. He stated,

In the United States we think about producing varieties that yield the most in average years, and if we get caught with an abnormal season or an early frost... we'll hope to make up for it in succeeding years. But not here. They [farmers] selected very hard for those things that produced under very adverse conditions, regardless of how much they yield. They weren't interested in maximum yield, but they were interested in getting something every year.... They had selected for adaptation to the extremes of climate, rainfall. ([Oral history of Edwin J. Wellhausen], 1966, p. 47)

In summary, maize farmers in Mexico faced a variety of site-specific challenges that required locally adapted varieties and technologies.

Wheat farmers, in contrast, had socioeconomic and environmental characteristics that were compatible with the RF's wheat program. In 1940, the central part of Mexico was the main wheat producing-area, harvesting 43% of the wheat while the northwestern region harvested only 17% (Myren, 1969). Then from 1940 to 1960, the RF's research shifted from mainly the central Bajío area (where the Chapingo station was located—see *Figure 5*) to the Pacific Northwest, home to commercial wheat farmers but very few maize farmers (Lewontin, 1983). In 1955 the MAP started a research station at Ciudad Obregón (Jennings, 1988, p. 85) in the northwest. By 1964 the northwestern region of Mexico, the Sonora, contributed to 71.5% of production, due not to a replacement of wheat farming in central Mexico, but a drastic expansion of wheat-growing land. In the moderate desert climate of Sonora, wheat farmers held large tracts of land, had the socio-

economic ability to invest in new seeds, inputs, and production technologies, and as a result of government-sponsored irrigation projects in the northwest, 89% of the wheat-growing areas in Mexico were irrigated in 1963 (Myren, 1969).



Figure 5. Map of Mexico highlighting Chapingo and Ciudad Obregón, two sites of wheat research in Mexico.

For both maize and wheat, starting around 1945 and certainly by 1955, the RF scientists in Mexico focused on testing varieties under irrigated, heavily fertilized conditions (Oasa, 1981; Cullather, 2010). They used these same conditions to demonstrate the superiority of new crops to the indigenous varieties, and seldom

performed experiments at different levels of fertilizer⁶ (Lewontin, 1983). Historian Stephen Lewontin attributed the focus on irrigated, fertilized conditions to the RF scientists catering to the rural elite in Mexico. Given Wellhausen's nuanced understanding of maize adaptation and his leadership in the MAP and later in CIMMYT, it is unclear why the "ideal conditions" adaptation strategy prevailed for maize, other than optimism that improved maize varieties would follow the same trajectory of adoption as Borlaug's wheat varieties.

The RF encountered early criticism of their Mexican program from Carl Sauer, a geographer at the University of California, Berkeley. Sauer was deeply concerned that the MAP would result in a loss in maize biodiversity in Mexico, and warned against modeling the MAP on US agriculture (Jennings, 1988). He found that Wellhausen was attuned to the local cultural specificities, and that "he is not a missionary for soybeans, and I suspect he sees the pitfalls in the wheat campaign" (Sauer quoted in Jennings, 1988, p. 52).

Despite farmers' lack of adoption of the MAP's maize varieties, maize held political importance in Mexico and according to researcher Bruce Jennings, MAP scientists dismissed "claims of inadequate production as a myth" (1988, p. 71). RF scientists continued breeding maize for irrigated, highly fertilized conditions until the late 1960s, when criticisms of MAP and the Green Revolution became more widespread. Even then, the RF launched the controversial Plan Puebla, an extension-heavy project to disseminate modern maize varieties to rainfed farmers, which was largely unsuccessful due to the lack of adaptation of maize to the rainfed conditions (Chapter 4 discusses Plan

⁶ An exception being the agronomic experiments performed by Reggie Laird.

Puebla in more detail). The wheat research program, inspired by its success in northwest Mexico, continued to focus on breeding wheat with photoperiod insensitivity, fertilizer responsiveness, and dependence on irrigation until the 1970s. I will further discuss this in the rest of my dissertation. Ultimately, the MAP and its subsequent transnational and international programs would focus on both wheat and maize.

Many scholars have retrospectively criticized MAP scientists for catering to wealthier farmers (Fitzgerald, 1986; Griffin, 1974; Lewontin, 1983). These scholars argued that by focusing on varieties suited to irrigated and fertilized conditions, RF scientists created a research program that was biased towards higher capital farmers. My own analysis is less critical and supports the work of Anderson, Levy, and Morrison (1991) who argue that scientists often operate under the assumptions that their own biases are neutral, and that technologies are also “neutral” in that they are not biased towards a certain socioeconomic population. MAP scientists believed that improved plant varieties and packages of practices were socioeconomically neutral technologies (that anyone could benefit from them). Of course in reality, farmers adopted technologies and packages of practices at different rates leading to socioeconomic changes that favored larger farmers with access to irrigation and fertilizers over small subsistence farmers. In retrospect, however, it is clear why green revolution-technologies favored larger farmers. First, one of the basic assumptions of innovation theory is that actors with more capital adopt innovations first (Rogers, 2010).

Secondly, I argue that scientists working in Mexico focused on commercial farmers with access to irrigation and assured rainfall out of both convenience and an

imperative to focus on higher production areas.⁷ For example, Wellhausen, said in 1947 that “areas in which a surplus can be produced should be attacked first’ the pure subsistence areas may be left to a long time in the future” (Cotter, 2002, p. 196). This philosophy, held by both Wellhausen and especially by Borlaug led to the disparity of outcomes between the MAP’s wheat and maize programs.

Finally, as several scholars have noted, the evolution of MAP’s research trajectory was connected to the Mexican government’s political agenda at that time (Cotter, 2003; Lewontin, 1983; Matchett, 2002). As Karen Matchett (2002) described in her dissertation, the idea of genetic improvement of maize varieties was very attractive to the leftist Mexican government under Lázaro Cárdenas, president from 1934–40, who believed that scientific agriculture could improve the livelihoods of peasant farmers, and redistributed land according to the *ejido* system. Manuel Ávila Camacho, president from 1940–46, shifted the discourse to overall production, rather than peasant livelihood. Under his successor, Miguel Alemán, most government funding went to irrigated rather than rainfed crops.

Maize scientists at CIMMYT realized by the late 1960s that aiming for widely adapted maize varieties did not suit the needs of rainfed farmers, as shown by the failure of Project Puebla. Myren wrote of Project Puebla, “what we have learned up to now is that identifying improved germ plasm for rainfed production is much more complex than

⁷ See Jennings (1988), Matchett (2002), and Harwood (2009) for further discussion of the motivations of the RF research staff in Mexico. These authors argue that the MAP’s maize research was initially peasant-focused. Matchett emphasized that the MAP’s maize breeding program first focused on inbred, open-pollinated (vs. hybrid) varieties that would benefit small farmers. While hybrids could attain higher potential yields, open-pollinated varieties were more likely to thrive under the field conditions in Mexico.

it is for irrigated conditions. It also appears that up to now most breeding programs have focused on selecting material for optimum moisture conditions” (Myren, 1972, p. 11). In turn, social scientist Kenneth Dahlberg wrote: “One wonders what the shape of agricultural research in those developing countries influenced primarily by the Rockefeller approach would be today if Project Puebla had been started in 1941 instead of the original project for increasing production on irrigated soils through specialized seed technologies” (1979, pp. 54–55). Dahlberg argued further that scientists should focus on adaptive agriculture, requiring “a shift from the emphasis on trying to control or change the environment to emphasis on better adapting production to the local environment” (1979, p. 183). Like other scholars, Dahlberg argued that the land grant model of US agricultural universities, applied to countries like Mexico and India, was a flawed and incomplete attempt at replication (Busch, 1988; Fitzgerald, 1986). He wrote that an important aspect of crop varietal development in the US was adaptation to local conditions, facilitated by local institutions and land-grant colleges.

New Country-Specific and Cooperative Agricultural Programs

Around 1950 RF administrators began considering the long-term future of the agricultural program in Mexico. Minutes from the 1950 meeting of the RF’s International Health Division stated that, "Five years ago we got into agricultural development in Mexico.... The success and interest in it would in itself raise the question whether The Rockefeller Foundation should, as a general undertaking, continue to do that kind of program" [emphasis original] (*Excerpt from minutes of meeting of IHD Commission on Review*, May 19, 1950). While some RF employees disagreed with the “universal

approach” of the MAP, by the end of the decade, it was clear that the RF would proceed with country-specific and transnational (cooperative) agricultural programs based on the MAP; that is, bringing in international scientists and working in-country to improve yields of staple crops (Cullather, 2010; Fahs, 1954).

Harrar—who in 1950 transferred from director of the MAP to Deputy Director of Agriculture for the RF in New York—was particularly important in expanding the MAP model to other countries in Latin America and Asia (Harrar, 1951). RF administrators, including Harrar, reflected that the RF could make a niche supporting long-term programs in agriculture. Starting in 1950, the RF began a series of country-specific agricultural programs based on the MAP and focused on maize. These programs started as countrywide programs in Columbia (1950), India (1954), and Chile (1955). The programs in Columbia and Chile both focused on improvement of wheat and maize, but also included livestock and other programs (Dahlberg, 1979).

In 1952 the RF sent Harrar, Manglesdorf, and Weaver⁸ to India to advise on the agricultural situation there. Then in 1954, the Indian government contracted two maize scientists, Wellhausen and U.J. Grant, from the RF to survey India’s research and development system and advise on collaboration between India and the RF. At that time, Wellhausen directed the MAP, and Grant led the RF’s Columbian Agricultural Program’s (CAP) maize improvement program. The RF and Government of India signed a memorandum of understanding in 1956 aimed at improving secondary education in agriculture and focusing on three cereal crops: hybrid maize, sorghum, and millet. On March 8, 1957 Ralph W. Cummings, a soil scientist from North Carolina State

⁸ Weaver was at the time the RF’s Director of the Division of Science and Agriculture.

University, arrived in India as field director for the Rockefeller Foundation, and Grant as assistant field director and director of maize breeding (Rockefeller Foundation, 1957).

Simultaneously, the RF expanded into what they called international cooperative programs that would typically be based in one country and would reach out to other countries. These programs aimed to 1) facilitate the international distribution of improved germplasm and 2) assist in building and training scientific researchers and staff in semi-tropical countries, without stationing RF staff directly in those countries (Stakman, Bradfield, Mangelsdorf, 1967). Throughout the 1950s and 1960s, the RF's international coordinated agricultural programs rapidly proliferated. The RF developed a network of cooperative crop testing programs through South America, the Middle East, and Asia. These cooperative programs were a means for the RF to institutionalize their research agenda beyond Mexico (Jennings, 1988). They functioned as an internationally diffused MAP, with the same basic cluster of administrators, crops, and goals.

The RF's first international cooperative program was the Central American Corn Improvement Program, which started in 1954 ([Oral history of Elvin Charles Stakman], 1966–67). Several countries in Central America came together to request RF assistance with their maize production in the early 1950s (Stakman, Bradfield, & Mangelsdorf, 1967). These countries included El Salvador, Honduras, Nicaragua, Costa Rica, and Panama. In 1954, the RF sent Harrar, Stakman, and Sterling Wortman, a corn breeder for MAP, through Central America to survey the possibility of an international cooperative maize testing program (Oficina de Estudios Especiales, 1954). Based out of the RF offices in Mexico and Columbia, the RF began the Central American Corn Improvement

Program. Wortman led the program in 1954 and then Donald Smith, of the RF's Mexico office, directed after that (Stakman, Bradfield, & Mangelsdorf, 1967). The goal of the Central American Corn Improvement Program was to test maize varieties that had already been exchanged between Columbia and Mexico to "see whether some of them may be used at once in the cooperating countries" (Oficina de Estudios Especiales, 1954). Then starting in 1956, MAP and CAP started a world-wide maize testing program that extended to several more countries in South America and also India, Indonesia, and the Philippines (Columbian Agricultural Program, 1956). The world-wide program aimed to evaluate the "adaptability and genetic value of specific material throughout the world, help breeders learn what is available, and help the germ plasm banks to fill seed requests intelligently" (Columbian Agricultural Program, 1957, p. 35).

In 1959 the RF created the Inter-American Food Crop Improvement Program, led by Wellhausen (Rockefeller Foundation, 1959). The program initially focused on maize but later included wheat and potatoes. This program aimed to apply the Columbian, Chilean, and Mexican agricultural programs more broadly throughout the western hemisphere. By 1960 the Inter-American Food Crop Improvement Program grew to include wheat, which would soon become its major focus. The RF established this program for two additional reasons, both internal and external. Internally, the MAP was moving towards a complete administrative transfer to Mexican scientists, which started in 1960 with the appointment of Ignacio Narvaez Morales as director of the MAP's wheat improvement program (Rockefeller Foundation, 1960). Because of this, the RF decided to establish Inter-American Food Crop Improvement Program so that they could continue

to operate internationally in Latin and South America. Secondly, the RF responded to demand from attendees at the Fourth Latin-American Conference of Agricultural Scientists in 1958. Wheat scientists at this meeting agreed to establish an inter-American cooperative yield test for wheat, similar to the Central American Corn Improvement Program. RF scientists, specifically Borlaug, would coordinate this program out of Mexico.

In the late 1950s, RF administrators were eager to get Borlaug into a position of international leadership for wheat science. Harrar, then-director for agriculture at the RF, wrote to Borlaug in 1958, “it is now timely to begin to intensify international research on small grain improvement in the Americas and its logical leadership to this effort should come out of the cooperative agricultural program in Mexico” (Harrar, 1958). Harrar also wrote in 1959 to José Vallega of the Food and Agriculture Organization (FAO) that “we now want Dr. Borlaug to operate on a very much more international scale. We would like to support him in an effort to strengthen cereals improvement research throughout the Americas and link these more closely together from the northern to the southern extremes of production areas” (Harrar, October 21, 1959).

RF’s International Agricultural Programs

Starting in the 1960s, the RF’s agricultural programs expanded from country-wide and cooperative to what they called international. The international programs for maize and wheat were extensions of the previous cooperative programs, but their rice program in the Philippines (described later) was more or less *de novo*. Nonetheless, the conceptual shift to international programs was a deliberate move by RF administrators. Not

surprisingly, the international programs continued to address the controllable technological variables of agriculture, working on assumptions of uniform field conditions. These programs also assumed that wide adaptation gave varieties transitive yield properties under controlled conditions, which was truer for wheat than for maize or rice.

In 1961, the administrative portion of the MAP was terminated and the National Institute for Agricultural Investigations (INIA) was formed to take over the MAP's national operations in Mexico (Jennings, 1988). According to Jennings (1988), the Inter-American programs floundered so the RF began looking for new institutional support for its international cooperative programs. Additionally, many of the RF scientists were not happy working at the INIA due to budget constraints and political tensions. So in 1963, the RF partnered with the Ford Foundation and Government of Mexico to form the International Center for Corn and Wheat Improvement, headquartered in Chapingo, Mexico, and directed by Wellhausen. The RF still provided funding to the INIA but built a new scientific complex to house the RF researchers (*Press release*, November 29, 1963).

The International Center for Corn and Wheat Improvement (the Center), as its name suggests, focused on international research programs for maize and wheat. The maize and wheat programs expanded on the existing transnational infrastructure in the Americas, expanding over the next few years to include collaborators in Africa and Asia. The Center's overall goal was "to aid, on an international scale, in the improvement of materials and methods for the production of maize and wheat by obtaining improved

varieties and by applying breeding techniques to achieve greater protection against insect pests and diseases as well as destructive climatic effects,” as described by the Secretary of Agriculture and Livestock of Mexico, Ing. Julian Rodriguez Adame (*Speech by Ing. Julian Rodriguez Adame*, October 25, 1963). The main goals of the wheat program included developing new varieties of wheat that were rust resistant and also “high-yield, widely-adapted” (The International Center for Corn and Wheat Improvement, 1963). For maize, the goals were to collect and distribute maize germplasm, to breed varieties resistant to disease, to develop varieties for high fertility conditions, and “to develop corn varieties insensitive to day length and temperature, thereby increasing adaptability” (The International Center for Corn and Wheat Improvement, 1963).

As will be discussed later in this dissertation, this was exactly the time that Borlaug and others began to popularize wide adaptation as a breeding goal for cereal crops. Borlaug had already discovered, through the cooperative wheat program, that several of the varieties of wheat from Mexico and Columbia could be grown in the Middle East and South Asia, and produced relatively high yields. RF scientists were finding that maize, however, was not as successful abroad. It had a more narrow range of adaptation, likely due to its sensitivity to day length. By 1965, it was more and more clear that wheat would be the main *international* focus of the Center, due to its ability to grow under a variety of conditions and locations. Yet maize research was still, and remains today, an important component of the Center and its later evolutions.

In the mid-1960s the RF had become involved in wheat improvement in India, and Ignacio Narvaez was contracted by the government of Pakistan and the Ford

Foundation to assist with Pakistan's wheat program. Lewis M. Roberts, an Associate Director of Agricultural Sciences at the RF, wrote in a 1965 that an asset of the Center was their four wheat breeders: Borlaug, R. Glenn Anderson (recently hired by the RF to work in India), and John W. Gibler and Charles F. Krull, who were both recently transferred from Columbia to the Center's headquarters in Mexico (Roberts, 1965). And interestingly, Roberts viewed the location of Mexico as an asset as well. He wrote, "The broad range of ecological conditions in that country provide a highly favorable natural setting for maize and wheat improvement work applicable to a broad belt of the globe, especially in the tropical latitudes" (Roberts, 1965). Roberts, among others, saw the potential to expand the RF's international wheat program based on widely adapted germplasm developed and tested in Mexico. Though he hoped the same for maize, its more specific adaptation prevented wide international success.

The International Center for Corn and Wheat Improvement became CIMMYT in 1966 (Centro Internacional de Mejoramiento de Maíz y Trigo / International Maize and Wheat Improvement Center). CIMMYT was governed by an international board of RF and Ford Foundation affiliates as well as international scientists that participated in CIMMYT's international programs (Jennings, 1988). Wellhausen became CIMMYT's director general and Borlaug led the wheat program. This 1966 change also meant that CIMMYT was more autonomous from the RF, though clearly still financially dependent. The RF continued to sponsor the country-specific programs, such as in India.

In summary: in the 1940s and 50s, the MAP aimed to collect foreign germplasm and test newly developed varieties under controlled conditions in Mexico. In the 1960s,

as the MAP ended and Inter-American Food Crop Improvement Program began, RF scientists added the element of international testing to the equation. Then the Center in 1963 and CIMMYT in 1966 solidified the international component of Wellhausen's maize program and Borlaug's wheat program. All of the RF country-specific, coordinated, and international programs shared a common theme: reducing agro-ecological complexity down to a uniform prescription of fertilizers, irrigation, and high yield potential varieties. This is reflected in the crop-specific nature of many of the programs, and the assumptions of their transferability to large agro-climatic zones.

Rice Research in Asia

Soon after starting the MAP, the RF became interested in agricultural development in Asia, and decided to use the MAP as a base to develop agricultural programs in other countries (Anderson, Levy, & Morrison, 1991). RF administrators as well as US State Department officials believed that increasing food production in Asia would quell potential communist uprisings in rural Asia, in line with Perkins' PNST. Thus, the success of the MAP was retrospectively imbued with political rhetoric about population growth, and the MAP was subsequently looked to by the State Department as the paragon of good international policy (Perkins, 1997). The MAP became model of preventing rural revolt and leading to happy, well-fed peasants (Cullather, 2010). The RF saw expanding their research into staple crops of Asia as a humanitarian mission to feed the hungry masses, especially in light of growing fears of a population explosion. To them and many others, the twin goals of increasing food production and stemming population growth were natural solutions.

The population problem was closely tied to post-war fears of communist expansion in Asia, as laid out by intellectuals such as John Boyd Orr and RF administrators such as Weaver (Orr, 1950; Perkins, 1997). Mangelsdorf wrote in 1961 about the uncommitted, but politically valuable, recently independent nation-state of India. He wrote that, “the near-famine areas of India are usually strongly Communistic and all of India may well move toward Communism” (Mangelsdorf, 1961, p. 279). President Eisenhower in 1959 “hoped that India would some day become a great counterweight to Communist China” (Gleason, May 28, 1959). India became a symbol of both overpopulation and a test whether a newly democratic nation could survive and not turn to communism (Hess, 2005).

The RF began planning and expanding their agricultural program into Asia in the 1950s. The RF’s Advisory Committee became especially interested in agriculture in the Philippines, which faced threats of both hunger and communism (Anderson, Levy, & Morrison, 1991). As previously mentioned, the RF sent three scientists to primarily India, but also other countries in Asia, to survey the agricultural situation there in 1952. In 1953 the RF appointed Elvin C. Stakman, an eminent and recently retired pathologist, as a special consultant to their international programs. Stakman wrote a detailed letter to RF president Dean Rusk outlining his thoughts on the expansion into Asia (as mentioned earlier, Harrar also discussed the expansion of the MAP to Asia). Stakman wrote to Rusk:

To what extent could experience from the Latin-American programs be utilized elsewhere? There already is a demonstration of considerable transfer value for Latin America. But how much would be applicable to a politically, radically, and

linguistically heterogeneous region like the overpopulated areas of Asia?... If the objective is to do the greatest good to the largest number as quickly and inexpensively as possible, all available and applicable experience should be used as a basis for plans and procedures if it were decided to try an experiment in Asia. (Stakman, December 22, 1953, p. 8)

This letter outlined some of the humanitarian motivations for the RF's involvement in Asia.

Simultaneously, in the early 1950s the RF considered becoming involved in rice research in India, Japan, and the Philippines, and in starting a regional rice research hub in one of these countries (Oasa, 1981). Over the next few years, the RF came to an agreement with the Ford Foundation to found an international rice research institute. While the Ford Foundation had previously been engaged in community development, or agricultural extension-based international programs, they had realized "that temperate zone agricultural technologies and western institutional arrangements did not transfer directly or easily into the tropical or semitropical environments of most developing countries. Fundamental and adaptive research was needed for the transfer to take place successfully" (Ford Foundation quoted in Oasa, 1981, p. 146). As previously mentioned, the RF's Harrar was instrumental in proposing a rice research center in the Philippines, which he had hinted at back in 1951, and after he visited the Philippines in 1953 with Weaver (Oasa, 1981). In the midst of expanding technical and scientific assistance

programs and fears of overpopulation and communism, the RF chose rice as a tool of modernization and,⁹ some would argue, sociopolitical control (Smith, 2008).

In 1959 the Rockefeller and Ford Foundations signed a memorandum of understanding with Juan de G. Rodriguez, Secretary of Agriculture and Natural Resources of the Philippines (Oasa, 1981).¹⁰ The International Rice Research Institute (IRRI) was created at Los Baños, near the University of Philippines campus, and Robert Chandler became IRRI's director. IRRI started official operations in 1962. IRRI was also funded by USAID starting in 1965 (Oasa, 1981).¹¹

In many ways, IRRI was modeled on the MAP: a centralized research facility that produced widely adapted germplasm to distribute. This is evident from IRRI's stated goals as well as from archival records that include correspondence between IRRI and CIMMYT scientists. IRRI drew inspiration from the success of the MAP model in Mexico, India, and Pakistan. At a 1966 Congressional hearing, Forrest Hill, and instrumental actor from the Ford Foundation in setting up IRRI, reported:

The introduction by Pakistan and India of improved wheat varieties developed in the Mexican-Rockefeller Foundation crop improvement programs may well mark the beginning of a revolution in wheat production.... There is every reason to believe that rice varieties developed at the International Rice Research Institute in the Philippines can be used to help revolutionize rice production in the humid

⁹ Many scholars of political ecology and science and technology studies have chosen rice technologies as a way to analyze sociotechnical systems in agriculture See: Anderson et al., 1991; Brooks, 2011a and 2011b; Moon, 2000; Oasa, 1981; Saha, 2012; Smith, 2008.

¹⁰ Anderson et al., 1991, Cullather, 2010, and Oasa, 1981 provide more detailed descriptions of the scientific and political origins of IRRI.

¹¹ USAID started funding agricultural research in Mexico starting in 1969, see Jennings, 1988.

tropics. (World War on Hunger, 1966, pp. 96–97).

IRRI initially operated with the goal of producing high yielding varieties of rice. IRRI's first director, Chandler, retrospectively told an interviewer that, "IRRI had to show the world that higher yields were possible" (Chandler quoted in Oasa, 1981, p. 174).

Cullather called this concerted scientific effort, led by US scientists, a "Manhattan Project for Food," one that aimed to create a revolutionary new rice technology in an isolated, project-oriented setting (Cullather, 2004, p. 233).

Social scientist Edmund K. Oasa's 1981 dissertation thoroughly documented the early research agenda of IRRI, which will not be discussed in detail here. Drawing on the work of Oasa and other scholars, I will analyze aspects of rice research in Asia that relate to the agro-climatic adaptation of rice varieties. As early as 1960 scientists in Asia promoted using "superior varieties with greater adaptability" (Dasananda cited in Anderson, Levy, & Morrison, 1991). Sala Dasanada, a rice scientist from Thailand, in 1963 presented a paper called "Breeding for adaptability to greater range of ecological conditions." Dasananda went on to become a board of trustees member at IRRI in 1968 and 1969 (Anderson, Levy, & Morrison, 1991).

Questions of adaptation were central to rice research, but were not explicitly studied. Instead, IRRI scientists focused on breeding rice varieties with an ideal plant type—photoperiod insensitive, fertilizer responsive, short-strawed, and disease resistant (Anderson, Levy, & Morrison, 1991; Oasa, 1981). Further, in 1964, just before IR8's release, IRRI scientists Peter Jennings and Henry Beachell repeatedly used the term "widely adapted" or "non-sensitive varieties" to justify the potential of IRRI varieties

across Asia (Jennings & Beachell cited in Anderson, Levy, & Morrison, 1991, pp. 65–66).

In the summer of 1965 a drought in India and Pakistan crystallized fears of food shortages and famine. This provided an immediate motivation for a miracle crop from IRRI; luckily for IRRI, they had been working towards the release of their ideal variety of rice—though ironically, drought resistance was not part of the equation, drought simply provided motivation for more food production (Cullather 2004; Oasa, 1981). A specific strain of rice, called IR8, was released in the Philippines and tested throughout Asian rice-growing countries (Cullather, 2010; Oasa, 1981). IR8 was derived from a parent, developed by the Dutch in Indonesia, which was photoperiod insensitive (Barker & Herdt, 1982). IR8's other main technological advancement was that it combined fertilizer responsiveness (typical of *japonica* varieties) with short duration suited to tropical conditions (of the *indica* varieties) (Farmer, 1979). IR8 was immediately dubbed the miracle rice.

According to Anderson, Levy, and Morrison, the ideal of widely adapted varieties was ironic because IR8 was only adapted to irrigated conditions (1991). Like the wheat varieties developed by Borlaug in Mexico, IR8 was developed under irrigated, highly controlled conditions, and high levels of fertilizer. It was then tested in multiple locations in order to test its local adaptation. From a 1966 USAID report:

The IRRI plant breeders have repeatedly stressed the importance of extensive local field trials, with proper attention to cultural practices and fertilizer use, before this, or any, new variety can be recommended for a specific region because

of the extreme variability in local soil and general environmental conditions and the uncertainty with respect to disease or insect hazards. (Moseman, 1966)

But unlike Borlaug's wheat varieties, IR8 did not prove itself as widely adapted across locations. Many authors have pointed to the extremely diverse rice-cultivation landscape as a reason for the limited success of IR8 in South Asia (Anderson, Levy, & Morrison, 1991; Farmer, 1979; Oasa, 1981). IRRI scientist Richard Bradfield justified the focus on irrigated conditions because he expected marginal rice cultivation to decrease and irrigated rice cultivation to increase, assuming a constant availability of water resources (Anderson, Levy, & Morrison, 1991).

Oasa wrote that, "although IR8 was tested on private farms throughout parts of Asia in 1966, nowhere in his report did Chandler state the conditions under which it and other promising IR varieties were tested. *In short, wide adaptability had a different meaning for IRRI. It actually meant wide adaptability under controlled conditions*" [emphasis added] (1981, p. 251). Oasa cited the controlled experimental conditions of IRRI as a main contributor to its lack of adaptation to Asian landscapes, in particular the irregularity of water resources. While some IRRI scientists recognized that "it is entirely unrealistic... to develop a variety under one set of conditions and to expect it to perform equally well under another," the IRRI mandate was to focus on conditions with demonstrable, big payoffs, which required testing and demonstrating results under controlled irrigation and fertilizer applications (Oasa, 1981, p. 200).

Commenting on rice varieties released by IRRI in Asia, scholars have been more explicit in their criticisms of IRRI's concept of wide adaptation. Farmer's 1979 article

brought the issue of wide adaptation to light. In response to the failure of rice varieties in both flood- and drought-prone areas of India and Sri Lanka, he wrote:

There are two kinds of adaptability. First, there is the adaptability... brought about by the selection of environment-specific varieties. Secondly, there is the adaptability of a single variety to a wide range of environments, including a variety of pests. Some of the claims made for IR.8, and some early pronouncements on strategy emanating from IRRI and elsewhere, clearly envisaged the second kind of adaptability... It is more the first kind of adaptability that is now essential in rice-breeding. It appears that the two kinds of adaptability have been confused, or at any rate not sufficiently distinguished. (pp. 307–308)

Subsequent studies of IRRI rice during the Green Revolution have had similar findings. Oasa also wrote about adaptation at length in his dissertation (1981). Anderson et al. made several claims that IRRI's "assumptions" about the applications of their rice "were too narrow for the complexity of cultivation conditions in Asia" (1991, p. 66). They also noted that IRRI's concept of wide adaptation appeared to mean in geography (i.e. latitude and longitude) only, not in reference to different environments (Anderson, Levy, & Morrison, 1991). Further, Robert Chambers (1977) suggested that IRRI's strategy of selecting under irrigated and heavily fertilized conditions may have actually eliminated varieties with wide adaptation.

IRRI Rice in India and South Asia

Despite IRRI's claim of developing widely adapted rice, natural and social scientists alike began realizing that IRRI's rice was only adapted to irrigated conditions (Kalirajan & Shand, 1982; Maurya, Bottrall, & Farrington, 1988). In other words, most farms in Asia did not have the necessary environmental conditions to grow IRRI's new rice varieties. Although IRRI scientists aimed at a widely adapted variety, their methods and goals were mismatched. Several scholars have examined this issue in South Asia: in particular, Farmer (1979, 1986), Oasa (1981), Saha (2012), and Anderson et al. (1991).

New varieties of rice developed by IRRI were indeed adopted extensively throughout Asia, though limited to irrigated areas. Farmers in India's western Indo-Gangetic Plains, who had access to canal irrigation, adopted modern rice varieties and planted them during the *kharif* (summer monsoon) season, in rotation with modern wheat (Farmer, 1979). Pakistan (then West Pakistan) also rapidly adopted modern rice varieties. But Farmer pointed out that even countries that adopted modern rice did not see massively increased gains in production the way they did for wheat, due to lack of local adaptation and susceptibility to pest and disease (1979). Farmer pointed out that IRRI varieties—particularly IR8—failed in low-lying lands of India and Bangladesh because of their reduced stature, which became submerged during the monsoon, and also their photoperiod insensitivity, which meant they matured before the monsoon season finished (1979). Shortage of water, i.e. reliance on rainfall, in about half of South and Southeast Asia in the early 1970s, constrained adoption of irrigation-dependent rice varieties, or farmers risked lowering the water table through pump irrigation.

As will be discussed in Chapter 3, agricultural scientists at this time, globally, wavered between the traditional view that crops must be developed in the location they would be grown, and the relatively new concept of wide adaptation that allowed quick adoption of foreign germplasm without extensive testing. Oasa recorded that IRRI scientists attempted to prove the wide adaptability of IR8 through well-managed experimental farms in 1965 and 1966 but that “some national scientists, India's in particular, questioned IR8's adaptability. Indian scientists felt that a variety should be tested widely before being released to areas outside of where it was bred and raised” (Robert F. Chandler cited in Oasa, 1981, p. 250). Indian scientists insisted on testing new materials under both irrigated and rainfed conditions. But Saha wrote that the “rainfed ecosystem, however, had its own set of problems that made cultivation of the newly bred varieties a challenging task for the scientists” (2012, p. 99).

When an El Niño event in 1965 and 1966 resulted in a failed monsoon season, it caused drought and food shortages in India and Pakistan. According to some scholars, this catalyzed the rapid diffusion of modern wheat and rice varieties into India from RF-funded research centers in Mexico and the Philippines (Ahlberg, 2007; Cullather, 2010). As green revolution-style research took hold in India, scientists shifted their focus to rapid gains in production that could be achieved in irrigated areas and under progressive farmers, rather than on equally distributing those gains (a similar “giving up” on the social aims of the project occurred in Mexico in the 1940s). This led to a bias in testing new varieties in well-fertilized, irrigated areas in the Punjab region. Saha argued that

Indian scientists, in focusing on developing rice varieties with high yields under ideal growing conditions, gave limited attention to the needs of marginal farmers (2012).¹²

Although some of the IRRI varieties released after IR8 were better adapted to tropical conditions (Farmer, 1986), from the late 1960s and on, IRRI scientists began grappling with the limited success of IR8 in rainfed and upland rice-growing areas. Several authors have documented this shift in goals, including Anderson et al. (1991) and Oasa. IRRI's Bradfield, who earlier believed that irrigation would increase in Asian rice farms, returned to his earlier arguments for focusing on multiple cropping systems, and offered a training course on this topic in 1969 (Oasa, 1981). Bradfield also began experimenting with rainfed rice starting in 1967, and by 1969 there was a growing consensus among national rice scientists that a universal variety was an unrealistic goal. By 1975, IRRI stopped releasing named varieties at all, and instead shifted their focus to supporting national research programs (Oasa, 1981).

Conclusion

Existing scholarship has shown that the RF adopted a model of agricultural development aimed at producing varieties that gave high yields under controlled conditions of high fertilizer and irrigation. Some scholars argue that this in fact produced varieties narrowly adapted to ideal conditions, although RF scientist claimed that the new varieties of wheat and rice¹³ were well adapted to both ideal and marginal conditions.

Most scholars who have critically assessed the green revolution have found that at least in

¹² The reader should note that the focus on production and food supply per capita preceded Amartya Sen's insights on hunger and famine as a failure of food distribution rather than production.

¹³ Limitations of maize's adaptation were more well-known.

the cases of maize and rice, wide adaptation was either not attainable (maize) or improperly attributed (rice). Especially in the case of rice, scholars argued that IRRI scientists used wide adaptation in an aspirational and geographic sense, when in fact rice developed by IRRI was specifically adapted to high fertility and irrigated conditions.

Few if any studies have specifically challenged the wide adaptation of green revolution wheat. For example, Perkins' history of wheat research in the 20th century, one of the most thorough and cited accounts of such, hardly addressed adaptation (1997). I posit that this is because Borlaug's wheat varieties did indeed have an impressive global spread and, coupled with higher amounts of fertilizer, drastically increased the national wheat yields of Mexico and India. These countries both had particular significance to US foreign policy. Also, the aggregate success of new wheat varieties shielded them from scrutiny on the claims of wide adaptation, specifically adaptation across conditions. Nonetheless, if anything, this chapter should show that the wide adaptation of wheat is an object worth studying in greater detail.

CHAPTER 2
NARRATIVES AROUND WIDE ADAPTATION IN INTERNATIONAL
WHEAT PROGRAMS, 1960–1970: NORMAN E. BORLAUG, CHARLES F.
KRULL, AND KEITH W. FINLAY

Chapter 2 examines three figures in international wheat research in the 1960s: Norman E. Borlaug, Charles F. Krull, and Keith W. Finlay. Borlaug and Krull were affiliated with the Rockefeller Foundation (RF) and the International Maize and Wheat Improvement Center (CIMMYT) in Mexico in the 1960s, while Finlay was an Australian academic who was hired by CIMMYT in the late 1960s. Borlaug undisputedly played the major role in elevating wide adaptation as a goal in the international agricultural sciences and establishing the narrative and meaning of wide adaptation. Krull and Finlay, however, have been overlooked in the history of agricultural science for their influential role in wheat research in the 1960s. Krull worked directly with Borlaug to promote wide adaptation and breeding for favorable environments in the Middle East, where the RF was strengthening its ties with plant breeders there through international wheat trials and by training plant breeders in collaboration with the Food and Agriculture Organization (FAO). Krull proposed that varieties developed in favorable environments could still have high yields in marginal environments, but not vice versa (Krull, [Diary Notes], April-May, 1966). Finlay, on the other hand, corresponded with Borlaug over the more theoretical aspects of wide adaptation. Finlay also promoted wide adaptation in several international forums, such as the Third International Wheat Genetics Symposium in 1968 in Australia, and the FAO/International Biological Program's plant gene pools project with his more famous collaborator, Otto H. Frankel.

In parts 1 and 2 of this chapter, I argue that Borlaug and Krull promoted wide adaptation in order to expand the RF's wheat programs and to increase global wheat production, but they did so under questionable scientific premises. The team promoted breeding and testing under only high fertility and irrigated conditions, but extended the meaning of "wide adaptation" from location-based to adaptation across agronomic conditions. I conclude that their narrative of wide adaptation relied heavily on the assumption of uniform and ideal field conditions. In part 3, I argue that Finlay's theoretical and administrative work on adaptation helped solidify adaptation as a measurable object of study in the plant sciences. Finlay, best known for his mathematical model of adaptation, helped start a revolution in quantitative plant breeding.

Background

Over the years, wide adaptation has been 'black boxed:' it has been packed with multiple, unfounded meanings and has been relatively unexamined by agricultural scientists, even today. This chapter unpacks the black box of wide adaptation and what is called yield performance. Borlaug and Krull developed a narrative that widely adapted crops have some inherent yield that expresses under any environment. But these scientists did not consider, nor care to, the mechanisms of wide adaptation beyond what they already knew: that it was a result of fertilizer responsiveness and photoperiod insensitivity. So why didn't Borlaug rely on these more accurate descriptions of his wheat varieties? Why did Borlaug and Krull insist that adaptation conferred some special

properties that could be measured via international yield tests? These are questions worth investigating.

In short, Borlaug and Krull made an argument about wide adaptation that I have summarized as: *Fertilizer responsive + Photoperiod insensitive = Wide adaptation = Inherent yield*.¹⁴ Borlaug and Krull assumed that wide adaptation imbued new wheat varieties with a transitive property of high yield. These scientists also assumed a relatively stable and favorable set of agro-climatic conditions, characterized by irrigation and high fertility. Thus, they promoted wide adaptation on the assumption of stable and favorable environments, but also extended the argument to marginal environments, using the argument of inherent yield. While Borlaug's correspondent, Finlay, repeatedly tried to get Borlaug to study the mechanisms of wide adaptation, Borlaug was not interested at all.

This chapter and the next two show the incredible expansion of RF and CIMMYT wheat programs led by Borlaug. Wide adaptation fit extraordinarily well into Borlaug's vision of a centralized system of wheat research in Mexico and the extension of that model to other countries. Borlaug throughout this time was mostly involved in working in Mexico and India, training students from both places, but Krull worked as Borlaug's proxy to establish connections in the Middle East, spreading the gospel of wide adaptation.

¹⁴ Again, I define inherent yield as high yield independent of environmental context.

Part 1: Norman E. Borlaug's International Wheat Program

In 1960 the RF officially started its international wheat improvement program, led by Norman E. Borlaug.¹⁵ Borlaug's international wheat program initially had two prongs: 1) to train foreign agricultural scientists (mostly from the Middle East) in Mexico, and 2) to distribute new wheat varieties to semi-tropical and tropical countries, through what were called international wheat nurseries. Through the exchange of some plant germplasm with his RF colleagues in South America, Borlaug incidentally realized that wheat developed through RF programs in Mexico and Columbia could be grown under a wide range of locations and conditions, which he called wide, or broad, adaptation.

International coordinated wheat yield trials. Up until 1960, wide adaptation was not viewed as a particularly desirable trait in wheat varieties. While plant breeders and explorers would collect plant varieties and store them for future use, there was no systematic testing of international wheat varieties throughout different agro-climatic areas of the world. Possibly the first systematic testing was the USDA's International Wheat Rust Nursery Project, started in 1950. Due to an epidemic of wheat stem rust, a viral pathogen, in North America, the USDA decided to test their large collection of wheat germplasm in different environments around North and Central America in order to identify rust resistant varieties. By 1952 the nursery had expanded to Australia and various countries in Africa and Europe (Oficina de Estudios Especiales, 1954).

Borlaug and the Mexican Agricultural Project were involved in the USDA's International Wheat Rust Nursery Project from its start (Oficina de Estudios Especiales,

¹⁵ See Chapter 1 for more context on the RF's international agricultural programs up to 1970.

1954). Simultaneously, Borlaug engaged in collecting, breeding, and testing foreign wheat germplasm for his own program (separate from the USDA). In 1947 he had written to wheat scientists around the world, asking them to send him seeds of local wheat varieties, and around this time he also started sending out RF-varieties to any interested scientists.

Around 1959 Borlaug proposed a new transnational wheat nursery that would focus on the adaptation of wheat to different locations. He wrote:

In the past there has been a great deal of circumstantial evidence that certain types of wheat have great flexibility and adaptation; however, this has never been checked experimentally, and it seems that the time has now arrived for doing so...

We in the R. F. program have profited greatly by moving seed, especially early generation, segregating material, from one country to another. (Borlaug, 1959a)

He proposed a “uniform yield nursery” to collect “valuable information on varietal adaptation” in wheat (Borlaug, 1959b). In 1960, Borlaug started his first international nursery, called the Cooperative Inter-American Spring Wheat Test. Borlaug sent wheat seeds to twenty different locations in the Americas (Borlaug, June 24, 1960).

Between 1960 and 1962 Borlaug became more involved in wheat science in the Middle (Near) East, working with wheat scientists from the FAO such as J. B. Harrington and José Vallega, who were stationed in or assigned to cover countries in the Near East. In 1959 he travelled to Libya, Egypt, India, and Pakistan to distribute packages of his dwarf wheats there (Cullather 2010). Borlaug worked with the FAO to train foreign agricultural scientists from the Near East in Mexico, starting in 1960. The FAO also

invited Borlaug to tour the Near East in 1960, where Borlaug examined some of the problems of wheat cultivation there (Borlaug, November 3, 1970). Borlaug apparently was surprised by the excellent yields of some of the wheat varieties developed through the RF programs in Columbia and Mexico. In 1960 he wrote to an RF scientist in Chile:

The Mexican, Columbian and Australian wheats fit very broadly in the spring wheat areas of Pakistan, India, Iraq, Lebanon, Jordan, Israel, Egypt and Libya, and as we have known through Thorpe, in Kenya as well... On the basis of what I saw in the Near and Middle East, I am exploring the possibility of establishing another spring wheat yield nursery orienting east and west, which would include Australia, India, Pakistan, Iraq, Lebanon, Jordan, Egypt, Syria, Libya Kenya, Colombia, Chile and Mexico. (Borlaug, June 24, 1960)

Borlaug was “amazed to see the wide adaptability of many of the wheat materials” and felt that many of the scientists did not recognize this, due to their lack of experience outside their own country (*Notes on the Consultants' Meeting*, 1960). In a draft of the RF’s 1960 to 1961 annual report, Borlaug noted that some of the wheat varieties developed in Mexico and Columbia “have wide patterns of adaptation” (Rockefeller Foundation, 1960, p. 254).

In 1962 Borlaug collaborated with the FAO to start the Co-operative Near East-American Spring Wheat Yield Nursery. In his 1967 oral history, Borlaug stated:

The FAO was putting out what they called rust nurseries that were supposed to be uniform. This was badly handled, and the identity of the lines was only known by God Himself frequently... So we set up the second one, which was called the Near

East-American nursery, and this included essentially all of the varieties from the Near East—their main commercial ones—Mexican varieties and Columbian varieties. Because these fit. We knew from our programs in Colombia and Mexico that these had this flexibility and adaptability. ([Oral history of Norman Borlaug], 1967, p. 282)

From the 1961-62 and 1962-63 Near East-American trials, five Mexican varieties yielded, on average, the highest of all twenty-five varieties entered in the trials (Borlaug, Ortega, & Rodriguez, 1964). All varieties were grown under widely varied conditions, as Borlaug recommended planting seeds on uniform plots that represented average local conditions. While the Inter-American and Near East-American nurseries were first considered separate, in 1964 Borlaug combined the programs into the International Spring Wheat Yield Nursery. Based in Mexico, he sent twenty-five varieties to thirty-four locations in twenty-three wheat-growing countries (Table 3). Seeds were grown under both irrigated and rainfed and fertilized and non-fertilized conditions (Krull et al., 1968). Again, five Mexican varieties yielded the highest, on average. The initial reports of the international nurseries casually noted the wide adaptation of the Mexican varieties. But the results for the third Near East-American nursery and the first International nursery, both analyzed by Charles F. Krull in 1966 and 1968, respectively, included detailed arguments about the superiority of widely adapted wheats to local varieties under varied conditions (Krull, et al., 1966, 1968). This latter argument was incidental to the original purpose of the wheat varieties, which was to give high yields under fertilized conditions. But due mostly to Krull, it would become CIMMYT's main justification for their wheat

research program: that a few varieties, if widely adapted, would raise yields and production in a great diversity of wheat-growing regions.

Table 3

Counties That Participated in the First International Spring Wheat Yield Nursery, 1964–65 (Krull et al., 1968).

Country	Number of sites
Argentina	4
Chile	1
Ecuador	1
Columbia	1
Guatemala	1
Mexico	2
United States	2
South Africa	1
Libya	1
Cyprus	1
Saudi Arabia	1
Ethiopia	1
Sudan	2
Jordan	1
Syria	1
Lebanon	1
Turkey	1
Rumania	1
Iraq	1
Iran	1
Pakistan	3
India	3
Australia	2

Wide adaptation: Borlaug goes against the grain. In 1968, RF scientists noted, “not only is there surprisingly little information concerning the performance of varieties over a broad range of environments in even the major crop plants, but there is considerable confusion among plant breeders as to whether broad adaptation is desirable or not” (Krull et al., 1968, p. 1). Borlaug planned to use his international trials to evaluate

“the relative adaptability of a uniform set of varieties of different origins by growing and observing them systematically under widely different conditions of climate, soil, and latitude” as well as “possibility of developing wheat varieties with extremely wide patterns of adaptation” (Rockefeller Foundation, 1960, p. 254).

By 1960 Borlaug had, in fact, already developed and identified several varieties that he considered widely adapted. According to him, he made this discovery during the USDA’s International Spring Wheat Rust Nursery (Loegering & Borlaug, 1963). Apparently, after seeing the wide adaptation of some of the varieties developed by RF programs in Mexico and Columbia, he developed an interest in collecting basic data on the adaptation, or geographic range, of wheat varieties (Loegering & Borlaug, 1963).

Borlaug first attributed the wide adaptation of certain varieties to his particular method of wheat breeding. Around 1945 Borlaug began alternately growing wheat generations between north and central Mexico, which was later called “shuttle breeding” (Cullather, 2010). Shuttle breeding is one of Borlaug’s best-known legacies, but it was not actually named that until the 1970s, when CIMMYT director Haldore Hanson “suggested that it be called ‘shuttle breeding,’ after US Secretary of State Henry Kissinger's shuttle diplomacy in the Middle East (CIMMYT, 1990, p. 14). In the winter, Borlaug planted wheat in the Sonora region of Mexico-- a coastal, irrigated region near sea level and at 28° N latitude. Then he would select the best offspring from that season and plant them in Toluca (near Mexico City), which was at 18° N latitude, had a high altitude, and had heavy rainfall and a higher prevalence of pathogens. In 1948, Borlaug almost resigned from his post in Mexico due to a conflict with his former professor,

Herbert Hayes. Hayes insisted that Borlaug's shuttle breeding would never work (Hesser, 2006). Borlaug stated in his 1967 oral history:

We were constantly, and very early, we were doing it consciously—discarding those things that fit in only one environment. We were interested because of the ease of multiplication of varieties of having things that were broadly adapted and consequently probably less vulnerable to the vagaries of climate, but also that if we found a variety that was well adapted and yielded well—it could be grown widely in Mexico. ([Oral history of Norman Borlaug], 1967, p. 188)

While Borlaug's 1967 oral history suggested that wide adaptation was intentional, other sources suggest that discovering wide adaptation was accidental, and Borlaug initially used two contrasting locations in order to speed up the plant breeding process.

Borlaug first proposed that wide adaptation was the result of certain “germ plasm complexes” that were genetically controlled (Rockefeller Foundation, 1960, p. 255). He surmised this because varieties he derived from the lines Mentana (Italy), Florence-Aurore (Tunisia), and Gabo (Australia), tended to be more adaptable across locations. Within a few years, however, Borlaug realized that the main genetic contributor to wide adaptation was photoperiod insensitivity.¹⁶ As discussed in the previous chapter, the photoperiod insensitivity of wheat varieties he developed in Mexico contributed to their ability to grow in a diversity of geographic areas. Borlaug found that wheat varieties from the US and Canada were specifically adapted to certain daylight requirements and did not perform well in other locations, but photoperiod insensitive wheats could be grown in a

¹⁶ Photoperiodism was discovered in 1918 (Kingsland, 2009)

variety of latitudes, elevations, and seasons (Borlaug, 1972a).¹⁷ He hypothesized that his shuttle breeding method had resulted in selection that favored photoperiod insensitive varieties that thrived in both the Sonora and Toluca regions, which have different seasons and photoperiods (Reynolds, Pask, & Mullan, 2012). Although Borlaug clearly recognized photoperiod insensitivity as a primary component of wide adaptation, his research program moved towards developing “even more widely adapted genetic types” of wheat and the question of “what is the maximum range of adaptation that can be incorporated into a variety” (Rockefeller Foundation, 1964, p. 228 & 229).

As early as 1965, Borlaug began promoting the idea that widely adapted varieties were not only adapted to different geographies, but also across agro-climatic conditions such as irrigation and soil fertility. He wrote: “Some Mexican and Columbian varieties have been among the highest in yield in locations from 0° to 50° latitude and over a wide range of longitudes, *under both irrigated and nonirrigated conditions*” [emphasis added] (Borlaug, October 1965, pp. 1092–3). When asked about Lerma Rojo, a tall, photoperiod insensitive variety that Borlaug created, he replied that its best feature was “its wide adaptation—fertilized or unfertilized” ([Oral history of Norman Borlaug], 1967, p. 196). Borlaug’s shift in narrative around 1965 seems highly correlated with his working with Krull, as section 2 will explore.

Other aspects of Borlaug’s program: dwarf wheats and breeding for high fertility. Photoperiod insensitivity is likely the most important trait that allowed Borlaug’s wheats, and derivations of them, to have the global spread that they did. But

¹⁷ Present-day studies have shown that a semi-dominant genetic mutation conferred photoperiod insensitivity in Green Revolution-era wheats. See Beals et al., 2007.

Borlaug's wheat program attempted several other innovations in wheat breeding, the most famous of those being his incorporation of dwarfing traits and rust resistance into varieties. Dwarf and semi-dwarf wheats have shorter and thicker straw than traditional wheat varieties. Semi-dwarf wheats can withstand higher levels of fertilizers without falling over and lodging, as traditional varieties are prone to do. Because they can utilize more fertilizer, semi-dwarfs have a higher yield potential than the traditional tall wheats. Borlaug learned of the dwarfing trait through Orville Vogel at Washington State University, who had obtained the wheat variety Norin 10 from Japan. Borlaug began crossing Norin 10 with Mexican wheat varieties in the 1950s, which resulted in a semi-dwarf wheat variety adapted to Mexican conditions. By 1955 Borlaug had successfully crossed Norin 10 with Mexican varieties, though he did not release a semi-dwarf variety adapted to Mexican conditions until 1961 (Perkins, 1997).

Borlaug began adapting wheat varieties to higher fertility conditions starting around 1945, under the assumption that fertilizers would soon become more easily available and affordable (Cullather, 2010). Borlaug also saw fertilizer inputs as key to reducing lost fertility from improper farming for centuries, leading to depletion of soil fertility. By 1955, Borlaug tested new wheat varieties under exclusively high fertility conditions (Oasa, 1981). He had several reasons for doing so, obviously the first being that the semi-dwarfs responded extremely well to increased levels of fertilizer. Second, Borlaug believed that with readily available fertilizer, varieties must be adapted to higher fertility conditions in order to increase overall food production. On his suggestion, Argentina's varietal improvement program was "reoriented in 1962 in order to develop

varieties which would be better adapted to higher levels of soil fertility should the use of chemical fertilizers become widespread” (Borlaug & Gibler, January 12, 1965, p. 1). He reasoned that, “any breeding program which did not take into consideration a change in levels of soil fertility within the next five years, would be doomed to failure” (Borlaug, [Diary], 1962).

Borlaug also believed that planting wheat under favorable environments (high fertility and optimum irrigation) allowed the scientist to observe a variety’s “true genetic potential,” because variation between varieties would be more obvious (Borlaug, January 6, 1987). In a letter to a scientific advisor in West Pakistan in 1964, Borlaug argued:

If these tests are conducted at high fertility levels with adequate irrigation one will begin to see many other problems in wheat production which are not evident when wheat is grown on ‘tired soil’, i.e. lodging, the magnitude of disease problems, new insect problems, lack of water penetration or percolation in some soil types, etc. This was my experience in India the past season when I finally succeeded in getting one of the yield trials established on 60 and 120 pounds of nitrogen levels at several locations. (Borlaug, June 18, 1964)

Borlaug also emphasized that “under irrigation two years of yield testing is more meaningful and reliable than five years under rainfed conditions,” because the environment-caused phenotypic variation under rainfed conditions would eclipse the genotypic differences (Borlaug, June 18, 1964).

Finally, Borlaug believed that varieties adapted to higher levels of fertilizer would lead to social change among farmers and scientists and overall higher levels of wheat

production. He wrote in 1966 that the government of West Pakistan “should realize that solving the fertilizer problem for wheat will be the start, not the end, of increased fertilizer demand. For once a farmer learns how to use fertilizer in large dosage on wheat, the practice will quickly spread to other crops. That was our Mexican experience” (Narvaez & Borlaug, March 30, 1966, p. 17). In India, Borlaug promoted “the program should try to produce tremendous yield increases on the area where the dwarf varieties can be heavily fertilized and properly watered. By so doing a complete change in the psychology of wheat production—from one of survival to one of high yields—will shock both the farmer and the scientist” (Borlaug, April 12, 1966, p. 11).

Borlaug also anticipated criticisms of the semi-dwarf wheats. As early as 1962, he responded to the “belief that these dwarfs in drought years will be short, that they will produce little or no grain, and that under such conditions what is produced will not be harvestable” (Borlaug, [Untitled report], n.d.), arguing that “the dwarfs now growing in Toluca show none of the weaknesses that had always been predicted for this kind of wheat under dry land conditions,” such as lack of straw (Borlaug, August 30, 1962). In response to those who might criticize the focus on breeding for favorable environments in India, Borlaug later wrote that, “even at low fertility and on dryland, they [Mexican semi-dwarf wheats] do surprisingly well, displaying their efficiency even though they were developed under irrigation” (Borlaug, 1972a, p. 586).

Criticisms of Borlaug’s wheat program became more widespread in the late 1960s, especially focusing on the RF’s impact on Indian wheat production. One review of

the International Maize and Wheat Improvement Center's (CIMMYT) program, by S. H. Wittwer from Michigan State University, found:

The lack of input [sic] of plant physiology and variations in cultural practices in the wheat breeding program was apparent. One rate of fertilizer (160 pounds of nitrogen per acre) is used throughout the 140 acres of experimental plots devoted to wheat. The same irrigation practices and rates for all wheat selections is used and the same plant spacing. Tests in which fertilizer levels, particularly nitrogen and water as variables, are being conducted on a limited number of wheat varieties by Dr. Reggie J. Laird. (Wittwer, 1969, p. 8)

Indeed, plant physiology as a field declined in popularity in the latter half of the 20th century partly due to the prominence of plant breeding as the central discipline. Today, agronomists typically carry out these types of fertilizer tests, and this happens almost exclusively in 'downstream' research, after varieties are already finished.

Borlaug's narrative of wide adaptation. Borlaug's semi-dwarf, fertilizer responsive, and photoperiod insensitive wheat varieties were quickly adopted in certain regions, especially in the irrigated parts of India, Pakistan, and coastal Turkey. USAID administrator William Gaud declared the green revolution in 1968, and Borlaug was awarded the Nobel Peace Prize in 1970. Although Borlaug was modest about his award, he had by that time adopted a 'missionary zeal'¹⁸ for increasing world food production and decreasing global population. The rapid increase in Borlaug's international connections, through the international nurseries and the RF/FAO training program, resulted in a generation of international wheat scientists who were known as Borlaug's

¹⁸ This phrase is attributed to E. C. Stakman, but is widely used in retrospectives about Borlaug.

“wheat apostles” (Shiva, 1991, p. 86). As highlighted by Shiva (1991), Borlaug said, “‘What Mexico did, you country can also do, except that yours should do it in half the time.’ This is the doctrine that Borlaug preaches to his apostles” (Stakman, Bradfield, & Manglesdorf, 1967, p. 283).

Borlaug’s program of breeding wheat for favorable conditions seemed at first to be a response to his assumption of rising levels of fertilizer. Simultaneous with his discovery of widely adapted varieties, however, he seemed to shift his focus to defending his choice of favorable environments. Without a doubt, favorable environments were easier for plant breeders to make selections in. But Borlaug went a step further by justifying his breeding environments with wide adaptation, arguing that wide adaptation allowed wheat varieties to yield higher than virtually any other variety (although this was only true on average). Unfortunately, Borlaug’s experiments relied on highly uniform and idealized field conditions. His major oversight was assuming that other locations around the world could also achieve these resource-intensive growing environments.

Borlaug attributed much of the success of his global wheat program to the discovery of wide adaptation in wheat. In an undated outline of a report titled, “The Development of High Yielding, Broadly-Adapted Spring Wheat Varieties,” Borlaug handwrote the rest of the title to be “*and its Significance for Increasing World Food Production*” (Borlaug, n.d.). In the margins of the outline, he wrote “KF” and “CK” next to various sections. These would be Keith Finlay and Charles Krull, Borlaug’s two colleagues who were most prominently involved in the promotion of wide adaptation as a plant breeding ideal. The next two parts of this chapter will focus on these scientists.

Part 2: Charles F. Krull and the RF's Cooperative Program in the Near East, 1965–

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From 1965 to 1968, Charles F. Krull was a key scientist involved in RF/CIMMYT's international wheat program, working in Mexico with Borlaug during this time. Luckily for historians of agriculture, there remains a wealth of primary sources from Krull's work at the RF from 1965 to 1968, including his diary notes, correspondence, and a 1966 oral history. Yet while many historians have focused on Borlaug, Krull was equally, if not more important in promoting Borlaug's concept of wide adaptation in the Near East—including in Cyprus, Egypt, Iran, Iraq, Jordan, Lebanon, Turkey, and Syria. He also led the analysis of the International Spring Wheat Yield Nurseries, which were published starting in 1964 and in full starting in 1966. Throughout his time with the RF in Mexico, Krull portrayed a consistent argument that: 1) scientists should consider the importance of widely adapted wheat varieties; 2) that countries should focus efforts on only one breeding and testing program for fertilized and irrigated environments; and 3) widely adapted varieties chosen under favorable environments could unequivocally outperform local varieties, regardless of environment.

As Krull managed and reported data from the RF's international wheat trials, Borlaug and others used his arguments to promote wide adaptation. Krull's story is also extremely interesting because he took Borlaug's arguments a step further. Krull argued that widely adapted varieties developed in irrigated and fertilized environments would yield equal or higher than local varieties in any environment. I call this premise "questionable" because it is so contrary to ecological theory, and also because Krull

apparently based this proposal on only a few years of the RF's international wheat trial data.

It is clear from Krull's records that his views on wide adaptation and breeding for high fertility conditions were not mainstream among wheat breeders, especially those from the FAO who were working in the Near East. The three conventional wisdoms that Krull fought against were that: 1) crop varieties could not be widely adapted; 2) varieties exhibited large genotype-by-location effects, so they should be specifically adapted to the farmer's conditions; and 3) varieties should be tested under local (often non-favorable) conditions. Using primary sources from 1965 to 1967, I will examine Krull's arguments for wide adaptation and breeding for favorable environments, and some of the arguments against this from scientists with whom he corresponded.

The RF hired Krull directly out of graduate school at Iowa State University, where he worked with Kenneth J. Frey, a well-known oat breeder. The RF needed an oat breeder to work in their Columbian Agricultural Sciences program, and Krull fit their requirements. Arriving in Bogota, Columbia, in June 1960, Krull worked with the RF wheat breeder John Gibler ([Oral history of Charles F. Krull], 1966). Although oats and barley were a component of the research program, based on the RF's goals at this time, Krull and Gibler both became involved mainly in the wheat improvement program in Columbia ([Oral history of Charles F. Krull], 1966). After a period of a few years, however, both Krull and Gibler became dissatisfied working with each other and in the Columbia program. Gibler was transferred to Ecuador and Krull to Mexico in 1965.

Around the same time, Borlaug had been considering utilizing Krull's assistance

in analyzing results of the international wheat yield trials. For several years, only preliminary results had been sent to the international collaborators (Krull, 1967). Borlaug needed someone with experience in both plant breeding and statistics to help him, and Krull was experienced in both from his dissertation work with Frey. Borlaug wrote to Moseman in 1964 that, “perhaps Dr. Charles Krull of our Columbian program is the man best prepared from all points of view to try to unscramble these difficulties with which we are confronted” (Borlaug, October 1, 1964). Borlaug wrote directly to Krull a few weeks later about “handling the data out at the Statistical Center in Chapingo, for both corn and wheat” (Borlaug, October 20, 1964). In August 1965 Krull transferred to Mexico to lead the coordination of the international wheat yield nurseries and their analysis, as well as to cover many of Borlaug’s duties in Mexico while Borlaug traveled internationally. Krull was named Resident Coordinator of International Wheat Program in May 1967.

Having Krull in Mexico from 1965 to 1968 was a great boon to Borlaug’s program on wide adaptation. With the results of the international spring wheat yield trials analyzed, Borlaug now had empirical evidence to support wide adaptation: several of the Mexican varieties yielded, on average, the best of all varieties tested. He stated in his 1967 oral history:

We begin to understand some of the basic things that underlie this adaptation.

This, to me, is a fundamental discovery that has long been overlooked. And it has been borne out now, and we have ample evidence, some of which has been reported in these recent bulletins that Dr. Krull has been getting out, that are

backed up by large quantities of experimental data that comes from what we call the International Yield Nursery. ([Oral history of Norman Borlaug], 1967, p. 191)

Further, this understanding of wide adaptation bolstered Borlaug's confidence in expanding the RF's wheat program into the eastern hemisphere. He stated, "And it's because of this mass of information that we feel pretty confident also in moving aggressively in Pakistan and India or in Turkey" ([Oral history of Norman Borlaug], 1967, p. 192).

Scientists should consider the importance of widely adapted wheat varieties.

Krull argued that wide adaptation was an important and undervalued concept in wheat breeding. As examined earlier in this chapter, the philosophy of developing varieties with wide adaptation was uncommon outside of the RF's programs in Central and South America, and a few other international scientists (including in India, as the next chapter will examine). Krull's views on wide adaptation were extremely emphatic and clear in the "Results of the Third Near East American Spring Wheat Yield Nursery." Krull is listed as first author, and from the subsequent reports, it is clear that his is the main voice in the report. Krull *et al.* wrote, "plant breeders frequently feel that varieties must be well adapted to only very small areas. They feel that since variety x location interactions are frequently encountered the ideal variety must be narrowly adapted. Indeed, such varieties can be produced. It is also possible, however, as is illustrated by these data, to produce varieties that are widely adapted" [emphasis original], even proclaiming the "possibility of producing spring wheat varieties with nearly universal adaptations" (Krull et al., 1966, p. 10). Discussing the prevailing idea that a country should have separate breeding

programs for different micro-climates, Krull *et al.* emphasized that, “Such a profusion of breeding programs unnecessarily depletes and weakens the effort being devoted to a crop as well as fostering an isolationist philosophy and narrow point of view on the part of the scientists” (Krull et al., 1966, p. 10). Based on the other writings of Krull, this is very much in line with his arguments at that time.

Speaking about the “elusive concept of breeding for adaptation,” Krull addressed the Crop Quality Council¹⁹ in 1967 about the “deeply ingrained philosophy that is held and taught by most of the North American graduate schools that such adaptation is probably neither possible nor desirable” (Krull, 1967, p. 3). He wrote of the RF’s experimental results:

The most striking thing concerning these results is, of course, the consistent, wide adaptation of certain varieties.... We know of no group of varieties in any crop plant that has shown a wider range of adaptation than is demonstrated by the highest yielding varieties in these nurseries. This is true in a crop that has generally been considered rather specific in its adaptation. (Krull, 1967, p. 3)

It is not entirely clear why Krull became such an advocate for wide adaptation. One possibility is the strong influence of Borlaug. Krull did not have much direct contact with Borlaug until 1965, and it is only after that date that Krull presented his many arguments for wide adaptation and selection under favorable environments. Other evidence of Borlaug’s influence is that three major supporters of wide adaptation—Krull, R. Glenn Anderson, and Keith W. Finlay—were all hired and promoted within CIMMYT, and worked directly with Borlaug. Finlay was the only one who regularly wrote about wide

¹⁹ An organization based in Minnesota that tested wheat varieties in the 20th century.

adaptation before being hired by CIMMYT; Krull and Anderson both started talking about wide adaptation after having close contact with Borlaug, though Finlay's influence is unclear.

Countries should focus efforts on only one breeding and testing program for high fertility conditions. In this next section, I will address Krull's efforts in the Near East to convince scientists to focus on breeding wheat for fertilized and irrigated conditions. This section will include correspondence between Krull and two FAO scientists working in the Near East: Abdul Hafiz, a Regional Consultant for the FAO's Near East Wheat and Barley Improvement Project, and located in Egypt in the 1960s and 1970s, and C. L. Pan, a cereal breeder for the FAO in Iraq and like Borlaug, a former student of Hayes and Stackman. Other FAO scientists that the RF regularly corresponded with in the Near East included Juan Tessi, José Vallega, and J. B. Harrington. In late 1966, Gibler wrote to Borlaug that, "working through Tessi, Hafiz, and Vallega, the FAO group could be used to concentrate on production. Even now they still aren't breeding wheats for fertilized conditions" (Gibler, December 28, 1966). This indicates that the RF scientists were interested in working in the Near East and continuing their collaboration with the FAO, but that they recognized a fundamental difference in opinion over breeding programs. Krull, through visits to the Near East and correspondence, attempted to influence wheat breeders in the Near East to adopt breeding and testing practices more similar to Borlaug's methods.

Unlike the Sonora of Mexico or the Punjab region of India, the Near East contained a diversity of wheat farming practices that included both irrigated and large

amounts of rainfed cultivation. In the 1960s, existing wheat breeding efforts in the Near East focused on low fertility conditions that farmers were more likely to experience. Krull, however, made a very specific argument that wheat breeding efforts should focus on highly fertilized conditions.

Krull made a trip to the Near East in April and May 1966, where he recorded his detailed observations and opinions of the wheat programs there. Krull wrote about a dryland area of Jordan:

The yield nurseries showed a decided lack of fertilizer, and this tended to make all varieties look the same. The reasoning was that most of the farmers do not use fertilizers so varieties must be selected under these conditions. This is a common fallacy among wheat breeders in under-developed countries, and there is actually little basis for it. Varieties selected under optimum conditions tend to yield relatively as well under poor conditions but under low fertility conditions yield differences are so small that they cannot be selected. In addition, under low fertility conditions the soil variability is more pronounced. (Krull, [Diary notes], April–May, 1966, p. 5)

This excerpt shows Krull's discontent with the prevailing breeding and testing system, and his argument that wheat varieties should be selected under favorable environments. His reason here was that favorable environments allow the breeder to see the variability between varieties to help them make their selection. In a letter to Hafiz in 1966, Krull wrote, "as suggested, I would like to see the nurseries more heavily fertilized. It is simply much easier to see yield differences at these high fertility levels. Putting on a good

amount of fertilizer tends to iron out any soil differences that there might be, so that the differences in yields observed are mainly genetic” (Krull, [Letter to Hafiz], June 23, 1966). In his 1966 diary, just a few months earlier, Krull wrote, “under low fertility the soil difference has to be quite large and it becomes impossible to pick out the genetically superior ones from those that just happen to fall on the spot with slightly higher fertility” (Krull, [Diary notes], April–May, 1966, p. 12). These arguments are clearly reminiscent of Borlaug’s.

A letter from J. C. Parisinos, a wheat breeder in Cyprus (cited in Hafiz’s letter) indicates another purpose of selecting under favorable environments. Parisinos wrote, “I have also noticed that varieties with a high potential yielding capacity are usually superior under both dryland and irrigated conditions and I also believe that liberal quantities of fertilizers should be applied on all variety trials to enable on to select the types with the highest yield potential” (Hafiz, September 3, 1966). This statement is very similar to the arguments by Borlaug and Krull that yield *potential* was more important than yield under prevailing agronomic conditions.

For selecting under high fertility conditions, Krull also reasoned, similar again to Borlaug, that “If the breeder is only working at the fertilizer level now used by the farmers, by the time the variety is actually selected and multiplied, it will already be obsolete with the better farmers” and that wheat breeders should anticipate higher fertilizer levels in the future and breed for responsive varieties (Krull, [Letter to Hafiz], June 23, 1966). Hafiz echoed this, writing to Krull that, “no doubt, the Cereal Breeders have now realized the great importance of breeding and testing varieties under high

fertilization... the Breeders will have to cater for varieties suitable to be grown under high fertilization, which is the only answer to meet the food shortage” (Hafiz, June 30, 1966).

In addition to breeding under fertilized conditions, Krull also promoted *testing* under those conditions. Similar to selection under favorable environments, Krull argued that testing under high fertility allows the scientist to see the yield potential of the variety. While visiting the international yield trials in the Near East in 1966, Krull took note of the lack of fertilizers. When visiting Iraq, Krull wrote in his diary that, “The experiments needed fertilizers badly and there were water logged spots that damaged parts of most experiments... Pan had not fertilized the nursery on the basis that farmers do not fertilize” (Krull, [Diary notes], April–May, 1966, p. 12). After some discussion, Krull thought that, “Pan finally seemed pretty well convinced” to use higher levels of fertilizer (Krull, [Diary notes], April–May, 1966, p. 12). Pan wrote soon after that visit to his former advisor, Hayes:

I have put most of the variety trials in a field entirely with the local method of farm management with a brief that any promising varieties thus screened out will be adoptable to the local conditions. Dr. Krull’s way of thinking in this respect, however, is quite different from mine. He thought that such a variety trial should be carried out in a field provided with the best conditions for the growth of the plant, such that the experimental plots must be sufficiently fertilized and amply irrigated... This seems to me a more realistic way of approach, and I am prepared to follow such new approach when I design trials in the future... I would become

much more convinced if you also can endorse this new approach. (Pan, April 30, 1966)

Unfortunately, Hayes' response was not included in the archives.

Krull was palpably frustrated that “there seems to be little basis for the widely spread belief that varieties selected under high fertility do not usually do as well under low fertility,” especially in the Near East (Krull, [Diary notes], April–May, 1966, p. 12). To argue against these critics, Krull drew from the results of the international nurseries, where the Mexican semi-dwarfs had high average yield across conditions and locations. In the “Results of the Fourth Inter-American Spring Wheat Yield Nursery,” published in 1967, Krull *et al.* addressed the prevailing belief that, “each environmental niche must ideally have its own set of varieties” (Krull et al., 1967, p. 10). They stated:

The seeming paradox can be understood by a simple illustration: if five tall, weak-strawed varieties and five strong-strawed varieties are planted in an experiment without fertilizer and also at another site with heavy fertilization, both groups of varieties will usually yield similarly without fertilizer but the strong-strawed group will yield infinitely better with fertilizer than the weak-strawed group (assuming fairly adequate moisture). (Krull et al., 1967, p. 11)

Krull gave a nearly identical argument in a 1967 presentation to the Crop Quality Council in the US:

If we seed 10 Mexican and 10 Indian varieties without fertilizer in India, we find that they all yield about the same. If we then seed the same experiment at another site with 120 pounds per acre of nitrogen, we find that the group of Mexican

varieties yields considerably more than the tall, weak-strawed Indian lines... The varieties that yield well with fertilizer also tend to be the same ones that yield best with poor management. This is very nicely illustrated by... literally hundreds of smaller tests that were run last year throughout India and Pakistan, and to a lesser extent in other countries in the Near East and the Americas. (Krull, 1967, p. 3)

He stated further that, “my point is that the presence of variety X location interactions does not necessarily imply that the same varieties are not the highest yielding in all environments” (Krull, 1967, p. 5). He extended his argument to state that, “evidence is accumulating that this same thing is true in irrigated versus dryland conditions... Such a statement is considered to be rank heresy by most wheat breeders” (Krull, 1967, p. 5). Finally, he argued “that varieties that show good adaptation in area are also better adapted over time” (Krull, 1967, p. 8). Krull’s statements, all from 1966 and 1967, reflect his confidence that widely adapted wheat varieties from Mexico and Columbia were genetically superior to varieties from other countries, independent of their environmental context.

Widely adapted varieties could outperform local varieties under both irrigated and dryland conditions. Krull also promoted wheat breeding focused only on irrigated areas. Krull argued that selecting under high fertility conditions produced varieties adapted to marginal conditions, and that they were superior to local varieties. Regarding the former, in a 1967 letter to Byrd C. Curtis, a plant breeder at Colorado State University, Krull wrote that the Mexican dwarf wheats were “extremely productive under irrigation and high fertilization, but the results of our international nurseries indicate that

they do as well as supposedly drought-resistant varieties under poor conditions,” indicating that they have stable production across different agronomic conditions (Krull, January 5, 1967). He wrote further that, “in other words, the dwarfs respond to but do not necessarily require irrigation and extremely heavy fertilization” [emphasis original] (Krull, January 5, 1967). This argument was a clear departure from Borlaug’s theory on wide adaptation centered around adaptation across locations, and is one that implies there is some inherent yield of widely adapted varieties.

Krull explained that regardless of the conditions of a region, the ideal variety would be adapted to both irrigated and drought conditions. He wrote to Hafiz:

When you were here in Mexico you suggested as did Dr. Vallega, that I take a look at non-irrigated areas of the Near East to see what can be done in breeding wheats for these areas. I spent a good bit of time studying the non-irrigated lands wherever I had a chance to see them... It appears that varieties that are adapted to intensive irrigation may also be adapted to very droughty conditions. Thus, it is not necessary to initiate a separate program for the irrigated and arid areas. (Krull, [Letter to Hafiz], June 23, 1963)

Krull insisted that a country should only have one breeding program to serve both conditions.

Both Hafiz and Pan disagreed with Krull on his suggestions for dryland agriculture, although they generally took his recommendations on breeding for higher fertility levels. Hafiz wrote to Krull that agronomic improvements (“agrotechniques” were necessary for dryland conditions, not just widely adapted germplasm:

For dry farming areas we will try to follow your suggestions but still I feel these areas require at least one comprehensive programme for the Region not only from the point of view of developing drought resistant and higher yielding varieties but also for developing better agrotechniques for the efficient use of soil moisture and fertilizers... It is really a very big and very difficult problem, but at the same time the most important and immediate one. (Hafiz, June 30, 1966)

Krull responded only to the genetic aspect, writing "I certainly do not disagree that it would be worthwhile to concentrate heavily in at least one place on drought resistance. My point was simply that I don't believe it would be wise to separate it from an irrigated program as it appears to be possible to produce drought resistance varieties that are also adapted to irrigated conditions" (Krull, July 12, 1966). Pan also wrote to Krull about the problems of dryland farming, ostensibly arguing that the RF's wheat breeding program did not fit the local conditions. For the wheat growing areas of Iraq, Pan wrote to Krull that, "it seems that wheat breeding should concentrate on drought resistance in the north and salinity tolerance in the south" (Pan, March 20, 1966). A year later, Pan still insisted to Krull that a drought resistant variety of wheat was necessary for Iraq, writing that, "as you know more than two thirds of the wheat crop in Iraq are grown in the north in the rainfed area. But rainfall varies very greatly from year to year. It seems that the most effective way to increase the yield level of wheat in the rainfed area is to use drought resistant variety" (Pan, April 18, 1967).

But during the mid-1960s the RF's wheat program ignored breeding drought resistant wheat, instead opting to focus on the higher gains that could be made in irrigated

wheat production. The evidence presented here shows a very clear contrast between views of the RF's Krull and the FAO's regional wheat breeders in the Near East. The FAO breeders were not only more embedded in the local conditions of the Near East, but also evidently held a different philosophy towards agricultural development there from the RF wheat scientists. Krull wrote in his diary that, "While there is interest in many countries in producing varieties that do not require fertilizer or water, there is no such group of varieties. The important thing in changing the production pattern in a country is to introduce varieties that will respond to good management and then change the management" [emphasis original] (Krull, [Diary, May 28-June 8 1967], pp. 3–4). This statement reflects a belief, held by the RF administration and Borlaug, that technical change would inevitably create desired social change. Borlaug created a normative characterization of what good management meant: maximization of yield under high-resource conditions. It also gives insight to why Krull so strongly resisted the status quo of wheat breeding in the Near East. Similar to Borlaug, he may have believed that the conservative views of scientists were a major barrier to progress in wheat production. He sought to change these views.

To conclude, Krull argued in 1965 that "the published results of our first five international yield trials have shown that it is possible to produce a series of varieties that are capable of outyielding local varieties from Chile to Canada and from Minnesota to the Near East. This same high yield and wide adaptation can also be built into a hybrid spring wheat" (Krull, October 4, 1965). He believed, like Borlaug, that high yield and wide adaptation made the Mexican semi-dwarf wheats superior to nearly all other wheats

produced by modern science.

Krull ultimately left Mexico in 1968 due to a divorce, but remained affiliated with the RF (Romig, February 20, 1968). While Krull seems to have been very influenced by Borlaug, Krull left an impression on Borlaug as well, in particular through his ability to use empirical data to support Borlaug's theory on widely adapted, fertilizer responsive wheats. In Krull's 1966 oral history interview, he stated:

We have come up with systems of breeding and philosophies that in some cases are different [from other programs]... One set of experiments that we are beginning... is to test some of our philosophies and breeding methods, to come up with small groups of experiments that illustrate our reasons for believing this or that about plant breeding methods. ([Oral history of Charles F. Krull], 1966, p. 58)

Krull argued that the most productive way to improve a national plant breeding program was to aim for widely adapted varieties selected under favorable environments. He backed up this philosophy with the results of the international spring wheat yield nurseries. As we will see in the next section, Keith Finlay used empirical analysis to take Borlaug and Krull's results a step further: to quantify adaptation across environments.

Part 3: Keith Finlay's Correspondence on Adaptation, 1963–1968

Borlaug and Finlay correspond on adaptation. Agriculturalists long regarded adaptation as a factor that could not be predicted or quantified, but only tested through trial and error when introducing plant varieties to new locations. But starting in the late 1930s, they began using quantitative methods, based off of analysis of variance models,

to analyze crop experiment data with an independent variable of agro-climatic zones or agronomic practices (Yates & Cochran, 1938; Horner & Frey, 1957). Then in 1963, an Australian wheat breeder, Keith W. Finlay, and his colleague, statistician Graham N. Wilkinson, released an experimental design and mathematical model that measured the yield stability of plant varieties over variable locations. The model measured what Finlay called “phenotypic stability,” but was more often referred to as adaptation at the time (Finlay & Wilkinson, 1963). Finlay’s model allowed scientists to more easily classify crop varieties as stable (high yielding across varied conditions) versus unstable (meaning unusually responsive to more favorable conditions). His model became immediately popular among plant breeders and led to a variety of other mathematic models of stability that still continues today. The model also became a tool of various ideologies on adaptation, as Cleveland described how the prominent physiologist Lloyd T. Evans called stability models “the plant breeder’s icons, ubiquitous but with a variety of styles to support a variety of dogmas” (Evans, 1996, p. 163; Cleveland, 2001).

Finlay’s model plotted the yield of an individual variety at a location against the mean yield of varieties tested at that site (as a measure of environmental quality) for i varieties and j sites. The slope of the resulting linear curve for a variety corresponded with the stability of that variety (see *Figure 6*). Finlay and Wilkinson’s corresponding article, the “The Analysis of Adaptation in a Plant-Breeding Programme,” was highly cited and influential on the field of plant breeding (1963).²⁰ According to plant breeders, Finlay and Wilkinson’s 1963 article was significant in that it was one of the first

²⁰ Finlay’s other 1963 article, “Adaptation- its measurement and significance in barley breeding” presented at the First International Barley Genetics Symposium in Wagenigen, was also popular among plant breeders.

computational analyses of plant breeding, and that it turned plant breeders towards studying adaptation (Romagosa & Fox, 1993).

Finlay and Wilkinson's model, and variations of it, are used by scientists to approximate genotype by environment (GxE) interactions for crop varieties (other fields, from psychology to ecology, use similar models). Scientists measure GxE by observing how much a change in environment leads to a change in a variety's phenotype. For example in *Figure 6*. Examples of varieties plotted in a Finlay-Wilkinson type model., variety 2 is more responsive to improved environments, whereas variety 1 is more stable (less GxE interactions) over all environments.

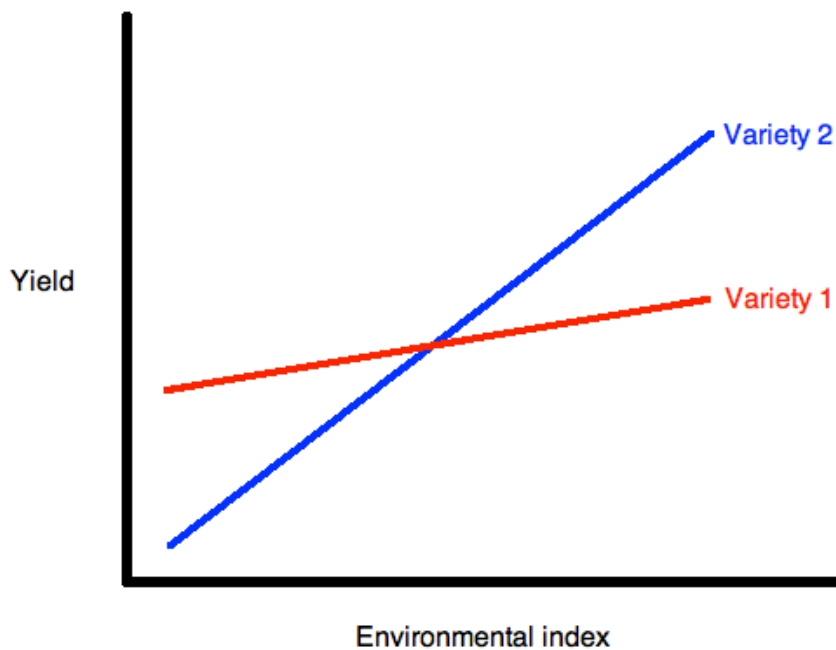


Figure 6. Examples of varieties plotted in a Finlay-Wilkinson type model.

Finlay's work on adaptation, both theoretically and programmatically (through his involvement in various international programs) helped solidify adaptation as a

measurable object of study in the plant breeding community. Interacting with Borlaug starting in 1963, and employed by CIMMYT from 1968 until his death in 1980, Finlay's analysis of adaptation "proved the obvious" of Borlaug's adaptation program—that certain varieties could be widely adapted across environments (Reitz, June 23, 1964). But it is only in investigating correspondence between Borlaug and Finlay, and Finlay's other publications, that the nuances of both scientists' arguments for wide adaptation emerge—specifically, Finlay's calls for more understanding of the mechanisms of adaptation, while Borlaug focused mostly on yield and production. Finlay appeared to be more interested in how adaptation emerged and how it could be developed in a plant breeding program, especially through the use of dynamic gene pools. Borlaug, on the other hand, seemed more concerned with the practical and immediate uses of widely adapted varieties to increase global food production, disregarding empirical evidence at times. Despite their differences, Borlaug and Finlay depended on each other for theoretical models and experimental data, respectively, which they used to promote wide adaptation internationally.

Before joining CIMMYT in 1968, Finlay was a professor of plant breeding at the Waite Agricultural Research Institute at the University of Adelaide, Australia. Borlaug became aware of Finlay through the recommendation of Vogel, who considered Finlay a "first choice" for the RF's Indian wheat program (Vogel, Aug 23, 1963). Finlay visited Borlaug in Mexico in October through November 1963, hoping for the opportunity to work in Mexico; in the meantime Borlaug was evaluating him for the India position (Borlaug, September 17, 1963). During Finlay's visit to Mexico (and subsequently

Columbia) he presented his work on adaptation, and later sent his 1963 paper to Borlaug (Borlaug, [Letter to Finlay], July 6, 1964). Borlaug, despite finding Finlay “a very capable theoretical research scientist,” found him too academically oriented for either the India or Mexico position, where Borlaug wanted someone with an inclination towards fieldwork (Borlaug, January 6, 1964).

Then six months later, Borlaug wrote to Finlay to update him, “since I last saw you we have learned considerably more about adaptation of the Mexican breeding material in far-away places... The Mexican material was equally as well adapted in India as in Sonora” (Borlaug, July 6, [Letter to Finlay], 1964). Finlay responded, “there is certainly no doubt that the more recent Mexican varieties have a very wide adaptation” and that he hoped they could continue working on adaptation together (Finlay, July 20, 1964). Finlay also included some preliminary analyses of the 1961–62 and 1962–63 Near East-American Spring Wheat Yield Nurseries, where he plotted the varieties’ average stability by their average yield, clustering the varieties by these qualities. He found that the newer Mexican varieties were definitely superior in terms of being more stable across locations and having a higher average yield, although there was not much different between the varieties released in 1960, ‘62, and ‘64 (Finlay, July 20, 1964).²¹ In other words, there was not much different between the tall and semi-dwarf varieties, both were widely adapted and high yielding.

Contemporaneous with his previous letter to Finlay, Borlaug wrote to Robert Osler, then-assistant director of agricultural sciences for the RF:

²¹ For example, the dwarf variety Lerma Rojo 64 had almost no advantage over the tall version of Lerma Rojo.

I have now had time to review rather carefully Dr. Keith Finlay's researches on development of flexible gene pools as plant breeding aids for under-developed countries, as well as the more fundamental studies relating to the understanding of varietal adaptation. I feel that Dr. Finlay has developed some useful information to partially explain adaptation phenomena we have already uncovered in the FAO-Near East-American Spring Wheat Yield Tests, and the Inter-American Spring Wheat Yield Nurseries. (Borlaug, [Letter to Osler], July 6, 1964)

By flexible or "dynamic" gene pools, Finlay meant the collection of germplasm available to a plant breeder (Finlay, 1968, p. 407). Louis P. Reitz, the USDA's leader of wheat investigations, was also corresponding with Osler about Finlay. He wrote, "support of his suggestion surely would lead to wider use of the fine Mexican materials and the work might lead to improved pools and greater understanding of gene pools. Some benefits would come even if the work merely 'proved the obvious'" (Reitz, June 23, 1964).

In late 1966, Finlay wrote a long, detailed letter to Borlaug about adaptation, the analysis of the international yield trial results, and the future directions of CIMMYT. While excited about "Phase 2" of CIMMYT, Finlay also had some concerns about Borlaug's program. One of his main emphases was that Borlaug should focus on determining what causes wide adaptation through collection of basic data. He wrote:

Your present wide adaptation is resulting from selection successively in a number of different environments, but the type and degree of adaptation is not known for any particular variety until it goes into the International Yield Trial. There is no doubting the results obtained, but equally there is no doubt that there is

tremendous advantages to be gained by actually measuring and selecting for this characteristic during the breeding process. (Finlay, September 6, 1966)

Further, he disagreed with Borlaug's shuttle breeding, stating that, "the selection technique used at present certainly allows the selection of widely adapted genotypes but it also automatically eliminates genotypes with exceptional potential for yield given the correct specific environment" (Finlay, September 6, 1966). Finlay's famous 1963 article noted, similarly, that:

Plant breeders are inclined to ignore the results obtained in low-yielding environments (e.g. drought years), on the basis that the yields are too low and are therefore not very useful for sorting out the differences between selections. This may be a serious error, because high-yielding selections under favourable conditions may show relatively greater failure under adverse conditions. (Finlay & Wilkinson, 1963, p. 752)

Despite Finlay and Borlaug's different views, through their correspondence they both appear to be driven to apply plant adaptation on a humanitarian basis.

Due to his interest in gene pools, Finlay cautioned plant breeders against keeping too narrow a genetic base, and also against selecting for only a specific type/characteristic of plant (Finlay, 1968). In this respect, he disagreed with Borlaug's shuttle breeding for wide adaptation (Finlay, 1968), but favored instead "my general thesis at the moment that a broad and continuously variable gene-pool is necessary for the production of high yielding and widely adapted varieties" (Finlay, June 24, 1965). Finlay's outlook fit in extremely well with the growing movement for collection and protection of global plant

genetic resources.

Finlay promotes adaptation as an object of international study. Finlay worked with Australia's well-known plant breeder and promoter of biodiversity, Otto H. Frankel,²² on the International Biological Program's project: Biology of Adaptation, which Finlay convened starting around 1966. The International Biological Program (IBP: 1964–1976) was an attempt at “big biology” to collect large-scale data sets, modeled after the International Geophysical Year (Aronova, Baker, & Oreskes, 2010). The Biology of Adaptation project fell within the IBP's subcommittee on “Use and Management of Biological Resources,” of which “plant-germ-plasm pools” was another project on the suggestion of G. Ledyard Stebbins (Pistorius, 1997; U.S. National Committee for the IBP 1965, p. 29). Finlay and Frankel were not unusual in their interest in preservation of plant germplasm; it became a major focus of plant breeders around the world, including India's famous M. S. Swaminathan, who was also involved in the IBP program on adaptation.

The original goal of the Biology of Adaptation project was an “analysis of the performance of a large number of varieties in certain standard, selected environments... and consequent analysis of productivity in genetic, physiological, and ecological terms” for four to six crops (U.S. National Committee for the IBP 1965, p. 29). The Plant Gene Pools project later subsumed the Biology of Adaptation working group. But by 1965, Frankel, chairperson of the Plant Gene Pools working group, met with the FAO's Vallega where they jointly decided that the FAO should handle agricultural aspects of plant germplasm, and the IBP would focus on more basic biology of plants (Pistorius, 1997). This led to the FAO/IBP 1967 Technical Conference on the Exploration, Utilization and

²² Though Frankel was born in Vienna, not Australia, he worked in Australia in his later life.

Conservation of Plant Genetic Resources, which ignited the movement for preserving plant genetic resources, led primarily by the FAO and later, the CGIAR (Consultative Group for International Agricultural Research) (Pistorius, 1997).²³ According to Robin Pistorius, however, the 1967 conference dropped the focus on adaptation and gene pools, instead concentrating on *ex situ* preservation of germplasm (1997). Based on my archival work, it appears that the biological adaptation program simply moved with Finlay to CIMMYT, when he started working there in late 1968. From a preliminary FAO/IBP meeting in 1966, there were plans to incorporate Biology of Adaptation into FAO/IBP program (Frankel, October 10, 1966). The FAO/IBP would focus on the adaptation of spring wheat, rice, and chickpeas. This would be led by Finlay for wheat, B. R. Murty for chickpeas (who we will see again next chapter), and Takane Matsuo of Japan and Te Tzu Chang of the International Rice Research Institute (IRRI) for rice (Frankel, October 10, 1966). The rice and chickpea experiments did take place through other RF-sponsored international research centers (ICRISAT, 1975; Matsuo, 1974; Sneep & Hendriksen, 1979).

Although Borlaug and Finlay appeared to have a cordial relationship, Borlaug was initially not at all impressed by the IBP's Biology of Adaptation project. On his copy of the "IBP Second Circular" from August 1966, Borlaug wrote into the margins of the planned experiments, "Being done by RF," "Charlie—this looks like our own ISWYN [International Spring Wheat Yield Nursery]," and on Finlay as coordinator for temperate

²³ Other histories of the plant genetic resources movement can be found in van Hintum, Frese, & Perret (1991), Busch et al. (1995), Pistorius (1997), and Kloppenberg (2004).

zone cereals, Borlaug wrote, “Competition?” (Frankel, August 3, 1966). Borlaug wrote to the RF’s Director of Agricultural Sciences, Sterling Wortman,

Frankly I don’t believe we should spend our money in support of this program, at this time. In the first place the work they are proposing on adaptation studies on spring wheats and durum wheat tests we are already doing in our International Spring Wheat Yield Nursery... Why should we set another organization up in competition with our own? (Borlaug, August 24, 1966)

In fact, the resemblance was likely due to Frankel, who was impressed by Finlay on the analytic aspects of adaptation, and Borlaug on the practical aspects (Frankel, 1966). But Borlaug, ever-focused on expanding his wheat program, was offended rather than flattered. Frankel wrote to Borlaug that, “we are mainly concerned with a broad adaptability study on the Finlay pattern; you are, I imagine, mainly concerned with the agricultural success,” and became personally interested in getting Borlaug involved in the FAO/IBP adaptation program, and to get him to attend the conference meetings in Rome (Frankel, August 30, 1966). By January 1967 Borlaug appeared to be onboard with supporting the IBP’s adaptation program (Frankel, January 12, 1967). According to the results of the sixth International Spring Wheat Yield Nursery of 1969–70, CIMMYT collaborated with IBP to conduct their trials in parallel with CIMMYT’s own nursery (CIMMYT, 1972). The wheat adaptation program does not seem to have progressed much beyond that, however, and likely was simply subsumed by CIMMYT’s existing international testing nurseries.

This section highlights the RF/CIMMYT's involvement in the movement for conservation of plant genetic resources that preceded the formal and tumultuous collaboration between the FAO and CGIAR that started in 1971. One of the linking factors was Borlaug's research program on adaptation, which brought together researchers in Australia, Japan, India, the Philippines, and Mexico.

Conclusion on Finlay. In 1968, Finlay helped organize the Third International Wheat Genetics Symposium, held in Canberra, Australia, in early August 1968. This conference seemed to signify Borlaug's wider acceptance by the wheat breeding community, and Borlaug gave a public lecture on "wheat breeding and its impact on world food supply" (Borlaug, 1968). In addition to organizing, Finlay also presented a paper titled, "The Significance of Adaptation in Wheat Breeding." He stated that after the Finlay and Wilkinson paper in 1963 "several other workers including Borlaug (1965), Eberhart and Russell (1966), and St. Pierre *et al.* (1967) indicated the advantages of selecting for wide adaptability... Many cereal breeders still consider the wide adaptability is synonymous with mediocrity in performance" (Finlay, 1968, pp. 403-404). He used the results of Borlaug's international trials to show that varieties could be bred with both high average yield and wide adaptation, or stability.

Though Borlaug had passed over Finlay for positions at CIMMYT several times already, with the departure of Krull in 1968 he again needed someone with a strong mathematical background to help with the international trials and general administration of the wheat program (Borlaug & Gibler, August 1, 1968). John Gibler was promoted to Associate Director of the wheat program, and Finlay was recruited to assist Borlaug and

Gibler. Finlay was quickly hired as “Director, Basic Research and Training (International nurseries and data retrieval)” for the maize and wheat programs at CIMMYT (CIMMYT, 1969), and remained there until his death in 1980.

Conclusion

Both Borlaug and Krull believed that by promoting widely adapted varieties and a singular research program for favorable environments, that they could promote the greatest good for the greatest number of people in the world. Finlay’s involvement with Borlaug and CIMMYT over the years points to some problems with Borlaug and Krull’s mission-driven approach to expanding their wheat program. Namely, Borlaug and Krull focused on irrigated and fertilized conditions through carefully controlled experiments, while overlooking the genetic and physiological factors that contributed to wide adaptation (besides photoperiod insensitivity). In many cases, they used ‘location’ as a proxy for ‘environment,’ which biologists and modern agricultural scientists recognize as an important distinction. This helps explain why Krull and Finlay, using the same data and different methods, came to different conclusions about the adaptation of Borlaug’s wheat varieties. Finlay’s analysis uses the relative performance of varieties as an index for the environmental quality, whereas Krull aggregated data based on location, weighting each location equally.

The following few chapters will examine the consequences of Borlaug’s focus on wide adaptation. By the late 1960s, RF, CIMMYT, and Indian scientists realized that the wide adaptation of the Mexican semi-dwarf varieties did not ensure their adaptation to rainfed environments, and that these environments required new approaches to wheat

research. Chapters 2 through 5 of this dissertation also show the impact of Keith W. Finlay's mathematical model to measure adaptation.

Finally, a pervasive theme throughout my chapters is the assumption that crop yield became more genetically controlled and immune to agro-climatic variability. This is reflected in the use of stability models to prove yield stability over time and place, and Borlaug and his colleague Charles Krull's rather tenuous leap to define wide adaptation as "highest in yield... under both irrigated and nonirrigated conditions" (Borlaug, October 1965, pp. 1092–1093) and that "high yield and wide adaptation can also be built into a hybrid spring wheat" (Krull, October 4, 1965). These scientists strongly believed in the transformative potential of seeds with built-in yield potential. But as others have pointed out, yield potential is exactly that: the potential yield dependent on environmental influence. Throughout the heyday of agricultural modernization, the 1960s, environment was seen as a constant and more importance was placed on plant genetics. At the same time, however, the reader will witness Borlaug's persistent argument for irrigation and fertilizers. Scientists later recognized that improved crop production post-green revolution was more representative of historically improved environments and fertilizer-responsive varieties than as a direct result of the supposed inherent yield of widely adapted varieties (Simmonds, 1981).

CHAPTER 3
INTERNATIONAL ROOTS AND THE INDIAN CONTEXT OF A NEW
PLANT BREEDING IDEAL, 1960–1970

Indian wheat cultivation changed radically in the 1960s due to new technologies and policy reforms introduced during the green revolution, and farmers' adoption of 'technology packages' of modern seeds, fertilizer, and irrigation. Just prior to the green revolution, Indian scientists adopted a new plant breeding philosophy—that varieties should have as wide an adaptation as possible, meaning high and stable yields across different environments. But scientists also argued that wide adaptation could be achieved by selecting only plants that did well in high fertility and irrigated environments. Scientists claimed that widely adapted varieties still produce high yields in marginal areas. Many people have criticized the green revolution for its unequal spread of benefits, but none of these critiques address wide adaptation—the core tenant held by Indian agricultural scientists to justify their focus on highly productive land while ignoring marginal or rainfed agriculture.

This paper describes Norman Borlaug and the Rockefeller Foundation's (RF) research program in wide adaptation, Borlaug's involvement in the Indian wheat program, and internal debates about wide adaptation and selection under favorable environments among Indian scientists. It argues that scientists leveraged the concept of wide adaptation to justify a particular regime of research focused on high production agriculture. Scientists utilized wide adaptation—and the practices associated with it, such as the release of new fertilizer-responsive varieties and importation and concentration of fertilizer—as a rhetorical strategy to justify radical changes in Indian agricultural policy

during a time of crisis. Indian and RF scientists ushered in a new doctrine of plant breeding under the crisis of food and population.

While these changes were meant as a stopgap for the food crisis, they became embedded as policy and have remained the norm in India. Wide adaptation became a vehicle to shuttle in new varieties and technologies with the promise that they would promote social equity, while simultaneously cementing a research regime focused on the favorable agro-climatic conditions of northwest India. These changes have led to a problematic, systemic bias against marginal agriculture in India, despite political efforts to the contrary.

This chapter focuses on why wheat scientists in India and at the RF argued for, and in some cases against, centralization of research, wide adaptation, and selection in favorable (high fertility, controlled irrigation) environments. Both qualitative and quantitative data suggest that widely adapted varieties from Mexico were in fact adapted to high fertility, irrigated conditions but not low fertility, rainfed conditions.

These scientists believed in their noble reasons for holding wide adaptation as a breeding goal. With memories of the 1943 Bengal Famine still looming, India aimed to rapidly increase food production during the 1960s. Scientists and foundation personnel also wanted to reduce the number of wheat varieties in production (Government of India, 1961). Wide adaptation made pragmatic sense to maximize resource allocation and to bring together decentralized wheat research. It also allowed wheat breeders to work towards a new blockbuster wheat variety that would bring them personal and professional

prestige. But wide adaptation was based on some questionable assumptions that played out once Borlaug's new varieties were planted on a wide scale in India.

Over the past 45 years, many scholars and activists have criticized the impacts of green revolution agriculture in India. Some of these critiques focused on the unequal socio-economic spread of technologies that favored the larger, irrigated, commercial farms of the Punjab region over smaller, rainfed farms (Anderson et al., 1982; Cleaver, 1972; Frankel, 1971; Griffin, 1974; Ladejinsky, 1969). Fewer of these critiques identified that the varieties of wheat and rice released in the mid-1960s²⁴ were not adapted to low rainfall, low fertility agro-climatic conditions that marginal farmers²⁵ typically face (Farmer, 1979; Lewontin, 1983; Oasa, 1981; Saha, 2012; Sen, 1974). But few of these critiques directly addressed *why*, in the mid-1960s, Indian agricultural scientists decided to focus the national system of wheat research on high fertility and irrigated conditions. Understanding why wide adaptation became the dominant framework in Indian plant breeding is critical to further cracking open the “black box” around wide adaptation that is still extremely influential to modern wheat breeding programs for developing countries.

²⁴ Varieties developed through Rockefeller Foundation-sponsored international agricultural research programs.

²⁵ Farmers in marginal environments. I use Cleveland's definition of marginal farms or environments as “crop growing environments that have relatively high levels of stress for yield production (e.g., drought), ...that often have relatively high levels of variability in these stress factors through space and time (e.g., rainfall with high spatial, intraannual, and interannual variation), and where farmers do not apply many external inputs (e.g., irrigation water)” (2001, p. 264).

Just prior to the green revolution, Indian wheat scientists adopted a new plant breeding philosophy that emphasized the wide adaptation of crop varieties.²⁶ Scientists defined this as varieties that produce high and stable yields in varying environments, also called broad adaptation or phenotypic stability (Finlay & Wilkinson, 1963). Up to the mid-1960s, cereal breeders viewed wide adaptation with skepticism, assuming that varieties should be bred in the area they are to be grown. But in the late 1950s, Borlaug discovered wheat varieties with what he called “surprisingly broad adaptation” due to their photoperiod insensitivity (Borlaug, 1968, p. 8). Borlaug’s successful wheat program introduced the radical idea of purposeful wide adaptation into mainstream science. Simultaneously, Borlaug consulted on Indian wheat research, and is credited with introducing his new varieties of wheat and the concept of wide adaptation to India.

In the mid-1960s the Indian wheat program, under RF influence, underwent three significant changes. It became centralized (Raina, 2009; Saha, 2012), varieties were tested under soil fertility rates roughly twenty-five times higher than average soil fertility rates in India, and varieties were judged based on their average performance over several locations in multi-state agro-climatic zones (*Proceedings of the Seventh All India Wheat Research Workers’ Workshop*, 1968). Indian and RF wheat scientists argued that by selecting varieties under high fertility and irrigated conditions, they could create high yielding, widely adapted varieties. They claimed that widely adapted varieties would still produce high yields in marginal environments, ostensibly to placate India’s economic

²⁶ Adaptation in this case refers to the performance of a plant in a given environment or condition rather than a process (Cooper & Byth, 1996).

planners who favored a socialist agricultural system (Saha, 2012). In reality, the Indian wheat program focused on the favorable agro-climatic conditions of northwest India.

The concept of wide adaptation underlies past and present research agendas, technologies, and policies in India, yet has seldom been scrutinized through a historical lens. Historians of biology can contribute to recent literature on controversial agricultural science by exploring the historical roots of agricultural technologies and ideologies.²⁷ Using historical sources such as conference proceedings, correspondence, and crop data from India, this paper highlights the history of the controversy over wide adaptation to understand how wide adaptation became a doctrine of Indian wheat science.

In the next two chapters I examine the consequences of the wide adaptation regime in India and other countries. I argue that the conditions under which the Indian wheat program developed in the mid-1960s have created a research system that is ‘locked-in’ to wide adaptation. In my dissertation conclusion I describe this as technological momentum, based on Thomas Hughes’ concept (1994). That Indian agricultural scientists uncritically rely on wide adaptation is a reflection of the historical codification of wide adaptation into the coordinated system rather than the interests of the scientists themselves (see interviews in Chapter 5). Nonetheless, a wheat research and testing system attuned only to wide adaptation severely limits the capacity of the system to respond to the needs of marginal farmers, to incorporate participatory research methods, to release more locally-adapted varieties, and to address climate change

²⁷ See Sumberg, J. & Thompson, J. (Eds.) (2012). *Contested Agronomy: Agricultural Research in a Changing World*. London: Routledge.

adaptation. Understanding the history of wide adaptation in India lends some insight to why it became so entrenched in the current system.

A Brief History of Indian Agricultural Research

The history of agricultural research and production in India is closely tied to British colonialism. Famines frequently occurred in British India due to a combination of climatic fluctuations and ineffective famine prevention or relief (Davis, 2002). The British Raj focused on improving cash and export crops over food crops, which may have continued to the decline in crop yields starting around 1920, reaching a nadir in the Great Bengal Famine of 1943 (Parayil, 1992; Perkins, 1997). In 1942, the British Government announced a “Grow More Food” campaign in India due to the start of WWII, encouraging farmers to switch from cash to food crops (Busch, 1988; Perkins, 1997). Grow More Food focused on increasing cereal grain production and availability of irrigation (Knight, 1954). Ultimately, the campaign languished due to lack of investment and concrete plans.

The British established the first research institute in India, the Imperial Agricultural Research Institute (IARI), in 1905 in Pusa, Bihar (Randhawa, 1987). In 1908, five agricultural colleges were established in India. By 1910, special government laboratories for wheat and rice had been established (Anderson, 1983). Starting in 1919, the Government of India delegated authority over agricultural development to the provinces, with limited success. The administrative body of the Imperial Agricultural Research Council (ICAR) then formed in 1929. IARI moved to Delhi in 1934 after an earthquake at Pusa, leading towards Delhi becoming the seat of Indian agricultural

research (Busch, 1988). Another main research focus was the Punjab, but mostly in the western region that would become Pakistan after the partition. The Punjab region was also the major site of canal irrigation by the British imperialists. After India's independence in 1947, IARI and ICAR changed their names to the Indian Agricultural Research Institute and Indian Council for Agricultural Research, respectively, but retained their abbreviations. According to Perkins, by the end of WWII ICAR had limited capacity to plan or achieve agricultural policies (1997). By 1948, ICAR staff had increased about 70%, especially in Delhi, and the scientific staff primarily focused on food crops, and especially wheat (Perkins, 1997).

Indian wheat science and adaptation. Before 1965, wheat cultivation in India was mostly limited to the northwest, while rice prevailed in the east and south, and millets in the peninsular region (Guha, 2007). Indian wheat breeding programs existed from the early 1900s, but efforts were decentralized and resulted in marginal gains in wheat yield. Contributing to the marginal gains was the fact that foreign wheat varieties were not well-adapted to Indian conditions (Parayil, 1992). Sir Albert and Gabrielle L. C. Howard are known as the pioneers of Indian wheat research, working in Pusa, Bihar. They wrote in their 1909 book that, “the introduction of exotic wheats into India has been a long record of failure” (Howard and Howard 1909, p. 117). This was partly due to the different climatic needs of European winter wheat, whereas India's climate required spring wheat. The Howards also noted the location-specificity of agricultural research, writing that, “the smallest differences in procedure are closely bound up with differences in local conditions” (cited in Saha, 2012, p. 95). Despite early failures, India

systematically experimented with foreign plant introduction from 1942 onwards, in line with the global movement of plant germplasm at that time (Government of India, 1961, p. 82).

Despite early decentralization and generally low support of wheat research by the Raj, Indian wheat scientists, often trained abroad, lobbied for ICAR to support their programs. Indian wheats faced, and still face, the problem of three different types of rust, a viral pathogen that can result in reduced wheat yields (leaf rust, stem rust, and stripe rust are the three main types). Benjamin Peary Pal led the charge for the coordination wheat disease research in India throughout the 1930s, 40s, and 50s (ICAR, 1952). Pal received his PhD in botany from the University of Cambridge in England under Rowland Biffen and Frank Engledow. He also mentored India's most celebrated wheat scientist, Mankombu Sambasivan Swaminathan (profiled later this chapter). In 1934 Indian scientists decided "that a collaborative beginning for breeding rust-resistant varieties of hill wheats should be undertaken at Simla... placed under the charge of Dr. B.P. Pal" (Kohli, 1968, p. 20). Two years after Pal was promoted to director of IARI in 1950, in 1952 ICAR approved Pal's coordinated scheme to control rust (ICAR, 1954). By 1959, Pal and pathologist K. C. Mehta released an "all purpose variety" of wheat, NP800, with resistance to all three rusts (Saha, 2012). This idea of a coordinated agricultural program and an "all purpose variety" would soon become a central feature of Indian agricultural research, as Pal became director of ICAR in 1965.

By 1957, the introduction and adaptation of foreign wheat varieties to Indian conditions was still mostly unsuccessful. At a 1957 plant breeding conference in New Delhi, India, G. Ledyard Stebbins said that,

The highly productive varieties created by breeders in one country cannot be transported to other countries simply by shipments of seeds... This is partly because climates vary greatly from one region to another, and a highly productive variety is always selected for top performance in its country of origin. (Stebbins, 1957, p. 127)

At that same meeting, Otto H. Frankel countered Stebbins' views, citing Clausen's experiments with hybrids from geographically disparate areas that resulted in plants with a wide range of adaptation (Frankel, 1957).

Stebbins' statement is similar to the views of Indian scientists who questioned the ability of crops to adapt to wide environments even within India. Scientists S. M. Sikka and K. B. L. Jain wrote, "The yield is, therefore, at its best when the plants of a particular variety are in harmony with the environment. It is now well recognised that a single variety of any crop, howsoever improved it may be, cannot be a universal success" (Sikka & Jain, 1960, p. 154). Sikka was head of IARI's botany department at that time, so we might assume that his views were not outside the mainstream. Minutes from a 1953 ICAR advisory board meeting show sugarcane breeder T. S. Venkataraman's skepticism towards wide adaptation, while B. P. Pal reassured him that for crops like sugarcane, "it was true that the crosses were best made in certain localities because these crops did not produce flowers in all localities... But in the case of crops like wheat and rice their

experience was that that did not happen” (ICAR, 1953, p. 32). Pal argued that the physiological aspects of wheat made it particularly adaptable as compared to other crops. Thus, India began to set the stage for wide scale introduction of foreign wheat and rice long before the green revolution. In 1963, Stebbins and Venkataraman’s statements would ring obsolete as India shipped tons of Mexican wheat seed to be grown, mostly successfully, under Indian conditions.

Post-Independence Indian Agricultural Policies and Modernism

India became independent from Britain in 1947, and Prime Minister Jawaharlal Nehru became the primary influence on Indian politics until his death in 1964. Under Nehru’s leadership, postcolonial Indian policies, including agricultural policies, veered towards a centralized and socialist agenda (Guha, 2007). India’s constitution prescribed for “the ownership and control of the material resources of the community are so distributed as best to subserve the common good” and against “concentration of wealth” (quoted in Guha, 2007, p. 206).

Nehru was also a strong proponent of modernism and “the scientific temper” (Guha, 2007). Both pre- and postcolonial India exalted “planning” and models for development (for example, the National Planning Committee established in 1938) (Bose, 1997; Cullather, 2010; Klingensmith, 2003; Guha, 2007). According to historian Daniel Klingensmith, “Indian politicians and engineers of the 1950s were careful to emphasize that science and technology were universal, not simply ‘American’... The United States as a land of science, affluence, and progress... looms large in the rhetoric of modernization in late colonial and post-Independence India” (2003, pp. 132–133).

Historian of science Madhumita Saha wrote that, “Moreover science and technology held to Nehru promise of social and economic changes without the dislocation and violence witnessed in other revolutions” (2012, p. 34). This modernism view of science and technology would have profound impacts on agricultural policy.

India’s economic planners hoped to separate India’s agricultural from its colonial past. Agricultural reforms included socialist land reform, helping landless peasants settle new land, and abolishing absentee landlords (Guha, 2007). India’s first five-year plan in 1951 focused more on industrial production than agriculture, because Nehru and his advisors believed that growth in industry to provide jobs for laborers and to prove the country as modern (Guha, 2007). Nehru believed that focusing on industry would ultimately create more demand for agriculture and would boost production (Parayil, 1992). The first five-year plan also focused on village self-sufficiency and community development—the development model that would prevail until the RF’s involvement in the mid-1960s (Perkins, 1997; Saha, 2012).

The second five-year plan (1956) expanded support for agriculture and social welfare programs, but it reduced spending on fertilizers. India imported its chemical fertilizers, which required spending valuable foreign exchange (Cullather, 2010). Thus, the second plan aimed to use the scarce foreign exchange supply for industrial rather than agricultural purposes. Then due to a crop failure, in 1957 the Indian government set up a Food Grains Enquiry Committee (Saha, 2012). This committee recommended a shift to chemical (rather than manurial) fertilizers and application to concentrated areas of food production (Saha, 2012). India’s Planning Committee rejected those recommendations

because they went against Nehru's and the Planning Committee's main goal of social equity (Saha, 2012). In the third-year plan (1961), the final plan before the green revolution, the Planning Committee put a high priority on self-sufficiency in food grains, but still reduced spending on irrigation and fertilizer (Cullather, 2010).

US Foundation Involvement in Indian Agricultural Development

US foundations became involved in Indian agricultural development in the 1950s, after India's Grow More Food campaign ended. The Ford Foundation, RF, and United States Technical Cooperative Administration (which became USAID in 1961) all involved themselves in agricultural development in India in the 1950s and 60s. Initial programs focused on community development through village-centric programs, and technical assistance through demonstrations of new practices and technologies (Guha, 2007; Perkins, 1997).

The Ford Foundation started a food production campaign in India in 1951. As a result of poor crop yields in 1957-8, the Government of India invited the Ford Foundation again to intervene, resulting in a 1959 report titled, "India's Food Crisis and Steps to Meet It" (Sen, 1969). Then in 1961 the Ford Foundation started the Intensive Agricultural District Programme (IADP). Also known as the Package Programme, the IADP concentrated on improving rural development in selected districts through fertilizer, irrigation, and modern crop varieties. The basis of the IADP was as follows:

Instead of spreading the developmental efforts more or less on a uniform basis throughout the country without getting any striking results, intensive efforts for production should be undertaken with the combination of all the technological

improvements and concentration of man power and resources in selected areas which have optimum conditions for stepping up production. (Government of India, 1966, p. 1)

Although the Ford and Rockefeller Foundations had different philosophies towards agricultural research and extension, the RF also concentrated their Indian agricultural program on areas with high production potential. A 1965 RF report stated that, “Large, sustained increases in yield can be expected only on the irrigated areas, and they will come only through greatly expanded use of commercial fertilizers” (Rockefeller Foundation, 1965, p. 168). Dorris D. Brown, an American extension scientist who spent eight years with the Ford Foundation in India, stated at a 1961 IADP training conference that “the major criteria for selection of a variety for multiplication should be its ability to give higher yields under cultivator’s conditions including responsiveness to heavy doses of fertilizer and ample water and plant protection” (Government of India, 1961, p. 10). The targets of these programs were unambiguously the irrigated regions of India.

The RF’s involvement in Indian agriculture started in 1952, when they sent three scientists to survey Indian agriculture (refer back to Chapter 1). While the Ford Foundation focused on community development through the IADP, the RF thought agricultural research was the more effective intervention (Saha, 2012). The Indian government worked with the RF on two Joint Indo-American councils, in 1954 and 1959, that developed agricultural education and research policy for India (Randhawa, 1987). The first Joint Indo-American council recommended that India implement a rural agricultural education system based on the US land grant colleges (Abrol, 1983). The RF

supported this project, in partnership with five US land grant colleges, in 1955 (Abrol, 1983). In 1960, with much direction from the University of Illinois, an agricultural university was established in Pantnagar, Uttar Pradesh, and six more universities were set up in the following five years (Randhawa, 1987).

The RF and other foundations viewed the Punjab region, located in northwest India) as especially fertile grounds for both social and agricultural development experiments (Latham, 2011). Known as the Khanna Study, the Rockefeller Foundation supported and the Harvard School of Public Health conducted a large-scale study of population control in Punjab rural villages from 1953–1969 (Williams, 2012). The researchers remarked, “India... is the cauldron in which mankind will be tested” (Connelly, 2008, p. 171). US scientists viewed India as a laboratory for democracy and the “self-help” ideology that prevailed during the Johnson administration (1963–1969) (Latham, 2011).

As stated in Chapter 1, the US government passed Public Law 480 (PL480) in 1954, a law that allowed the distribution of surplus grains to other countries. Shipments of cheap grain to India started that year. Previous to this, the US Ambassador to India, Chester Bowles, orchestrated cooperation between India and the US under the Technical Cooperation Program (Point Four), in 1952 (Saha, 2012). One of the major outputs of this program was widescale distribution and demonstration of fertilizers to Indian farmers, along with soil fertility testing (Saha, 2012). Frank Parker, an American agronomist stationed in India in the early 1950s with the TCPs, stated, “India is not overpopulated, it is underfertilized” (quoted in Cullather, 2010, p. 199). A major reason for the US’s

involvement in India's food supply was to promote stability in the region. US foreign policy experts believed that supplying food to hungry and politically unstable countries was critical to national security (Perkins, 1997; Saha, 2012).

Reorganizing the Indian wheat program: centralization and a northwest bias

A major reorganization of Indian agricultural science occurred in 1965, building off of both prior institutional innovations and RF involvement. In the 1950s, Indian agricultural policy-makers and prominent scientists began a large-scale reorganization of crop research programs and agricultural education. The Indian government invited two RF scientists, Edwin J. Wellhausen and Ulysses J. Grant,²⁸ to review their research system in 1954, concentrating on maize research. The RF scientists noted that a major impediment to progress in maize breeding was the lack of coordination between decentralized research centers (Grant & Wellhausen, 1955); this was later confirmed by an agricultural review team consisting of both RF and Indian scientists (Parker et al., December 13, 1963).

In 1956, the Indian government invited the RF to coordinate maize, millet, and sorghum research. The Government of India and RF signed a memorandum of understanding and in 1957 started the Coordinated Maize Breeding Scheme under RF scientists Ralph W. Cummings and Grant. A subcommittee of ICAR, led by Pal, "recommended the division of the country into... four zones for purposes of maize breeding work" (ICAR, 1957). This novel idea for coordinated breeding according to broad agro-climatic zones ironically led to a centralized research program.

²⁸ At that time, Wellhausen directed the RF's Mexican Agricultural Program, and Grant led the RF Columbian Agricultural Program's corn improvement program.

ICAR started an informal coordinated wheat program in 1961 “modeled on the coordinated maize program,” and put scientists at the IARI in Delhi in charge (Anderson, 1970). According to Cullather, RF scientists guided IARI away from secondary and ornamental crops, and towards wheat (2010). The RF supported scholarships for wheat scientists to study in the US, and worked closely with Pal to train a generation of wheat researchers (Cullather, 2010).

ICAR then invited Borlaug to consult on wheat research in India in 1963. Borlaug recruited R. Glenn Anderson, a Canadian wheat scientist, to join the Indian team in late 1964 and spearhead the new “unified and aggressive” coordinated wheat program (Borlaug, August 18, 1964). In 1965 the All-India Coordinated Wheat Improvement Program was officially launched, separating India into five agro-climatic zones for wheat production. That same year, the government, through the food minister C. Subramaniam, restructured ICAR, appointed Pal as ICAR’s Director General (ICAR’s first scientist, rather than bureaucrat, leader), put IARI and other agricultural organizations under the jurisdiction of ICAR, and wrote the new agricultural programs into national policy (Saha, 2012).

The coordinated wheat program centralized power of the IARI, India’s main agricultural research body, and ICAR, the bureaucratic body, in New Delhi (Raina, 2011a). But IARI scientists had to prove that their research program could still benefit farmers in the majority of the country. A shift in emphasis towards a centralized wheat program raised “serious questions on the part of the states as to whether the Coordinator can fairly represent them,” in addition to in-house claims that “IARI has deliberately

usurped credit due to these all-India coordinated projects, to the detriment of the non-IARI staff concerned.” (Anderson, 1970; Rockefeller Foundation Indian Agricultural Program, n.d., p. 14). Under the auspices of IARI coordination, the centralized research system benefitted scientists at the top of IARI’s hierarchy. These scientists employed wide adaptation as a rhetorical strategy to justify the centralization of agricultural research in New Delhi. The RF also utilized wide adaptation to quickly disseminate knowledge and technologies throughout India in a politically savvy way.

With RF support from the 1960s onward, most wheat research efforts in India focused on the northwest region of Punjab, Haryana, and western Uttar Pradesh, which form an agriculturally productive and mostly irrigated region. Many prominent research organizations were located in the northwest, including the IARI in New Delhi, G. B. Pant University of Agriculture & Technology (Pantnagar) near Nainital, and Punjab Agricultural University in Ludhiana. Other regions of India also grew wheat extensively, but crop productivity was low and farmers there lacked assured irrigation. In the 1960s, rainfed agriculture accounted for 80 percent of cultivated land and about two thirds of the wheat-growing region (Sen, 1969). For farmers dependent on rainfall, the timing of rain affected not only their overall yield, but also whether it was economic to apply fertilizer (Dawson, Murphy, & Jones, 2008; Gangopadhyaya & Sarker, 1965).

Norman Borlaug’s Focus on Fertilizers in India

In order to understand the increased focus on wide adaptation and breeding for favorable environments in Indian wheat research, some background on Borlaug’s wheat program is necessary. In the 1960s, the RF agricultural science program aimed to

dramatically increase food production in tropical countries. RF administrators placed Borlaug in charge of the international wheat program²⁹ simultaneous with Borlaug's discovery of widely adapted wheats around 1960 (see Chapter 2). By this time, Borlaug's wheat program operated under exclusively high fertility conditions (Oasa, 1981). Borlaug fervently believed that more fertilizers would soon become available and affordable in developing countries, and that varieties traditionally adapted to lower fertility conditions—including tall-stalked Indian varieties—would fail under higher fertilizer levels. Under high fertility field conditions, tall wheats would fall over and heads became lodged from the weight of their grain. Borlaug thus developed wheats that were adapted to higher levels of fertilizer, including semi-dwarf wheats that would not lodge under high fertility.

Although never stationed in India, Borlaug corresponded with Indian officials and scientists directly and through the RF field staff in Delhi. Concerned about the growing population in India, he wrote in 1966, “India must really think big and positively on fertilizer, or starve,” convinced that more fertilizer levels would create “a complete change in the psychology of wheat production—from one of survival to one of high yields” (Borlaug, December 21, 1966; Borlaug, April 12, 1966). His wheat program aimed to revolutionize Indian agriculture long before the green revolution was coined.

In 1964 Borlaug presented his recommendations to ICAR. He wrote that despite past research efforts focused on local soil fertility levels, “new types of wheat varieties are urgently needed” to survive the “anticipated changes that will come about through the

²⁹ Started in 1960 as the Inter-American Wheat Improvement Program and then becoming part of the International Maize and Wheat Improvement Center in 1966.

use of heavy rates of chemical fertilizers” (Borlaug, April 11, 1964, p. 2). Next, he suggested that, “the major emphasis for the next 5 to 7 years in breeding should be on improvement of the varieties for irrigated wheat production,” in order to rapidly increase food production in India (Borlaug, April 11, 1964, p. 16). All of his recommendations were implemented.

In response to those who might criticize the focus on breeding for favorable environments in India, Borlaug later wrote that, “even at low fertility and on dryland, they [Mexican semi-dwarf wheats] do surprisingly well, displaying their efficiency even though they were developed under irrigation” (Borlaug, 1972a, p. 586). In a similar vein, his RF colleague, Charles F. Krull, argued that, “the dwarfs respond to but do not necessarily require irrigation and extremely heavy fertilization” [emphasis original] (Krull, January 5, 1967). Krull also proposed that varieties developed in favorable environments could perform well in marginal environments, but not vice versa, an axiom taken up by N. N. Roy and B. R. Murty of IARI (described later this chapter) (Krull, [Diary notes], April–May 1966).

R. Glenn Anderson joined the RF field staff in Delhi in August 1964 and soon became joint coordinator of the All-India Coordinated Wheat Improvement Program working with Borlaug to popularize wide adaptation among Indian scientists.³⁰ Anderson also heavily promoted investment in chemical fertilizers stating in retrospect at a 1973 conference in Mexico,

³⁰ Stating in 1969 that, “the question is often heard of whether greater yield can be obtained from the narrowly adapted or widely adapted varieties. I consider the widely adapted superior” (*Proceedings of the 8th All-India Wheat Research Worker’s Conference, Vol. I.*, 1969), p. 174).

The economist said if you put 30 pounds of fertilizer on 3x acres, you would increase production to a higher level than if you applied 120 pounds on x acres. This was true. On the biological side, we argued that the latter condition should be followed because of the psychological shock effect on the farmer. At the 120-pound level, he would harvest 2 to 3 times the grain he did formerly. At 40 pounds he would show a modest increase, but it could be attributed to weather, acts of God, etc. With the high yields, there was no questions but what he was a convert to the use of fertilizer. (Anderson, 1973, p. 84)

Despite Anderson's ground-level involvement, Indian scientists credit Borlaug with bringing wide adaptation in India. Borlaug argued that in India, "every effort should be made to develop widely adapted varieties" and further, aimed to "convince the research scientists that adaptation of wheat varieties seldom or never coincides with state boundaries" (Borlaug, April 11, 1964, p. 18).

Although the coordinated system of research in India did not initially aim to produce widely adapted crop varieties, wide adaptation became mainstream just as the program solidified. Wide adaptation happened to dovetail extraordinarily well with the scientific and administrative goals of the coordinated wheat program, and in particular the shift from a decentralized to centralized program headquartered in northwest India. As the remainder of the paper demonstrates, almost all arguments to implement wide adaptation came from RF scientists and Indian scientists who worked in northwest India. This implies two things: that scientists in the northwest had the most ready access to new

ideas and ability to publish, and that they also had some political and personal motivations for promoting wide adaptation as the dominant plant breeding ideal.

Changes in the Indian Wheat Research Agenda: Bias Towards Favorable Environments

Prior to RF involvement in Indian agriculture, Indian plant breeders were part of a global network of international scientists, often trained abroad. In the late 1950s Indian plant breeders were interested in theoretical questions of genetics, genotype and environment interactions, plant adaptation, and other contemporary topics in plant breeding (Lele & Goldsmith, 1989; Pal, 1957). But it is clear that prior to RF involvement, there was no unified approach to breeding under favorable environments and for wide adaptation.³¹ The structural shift in the wheat program paralleled an ideological shift towards new breeding goals, both under the RF's advisement.

Although ICAR was ultimately responsible for initiating the coordinated wheat program in 1965, Borlaug's influence is obvious. For instance, ICAR's proposal for the wheat program stated, "the main emphasis will be placed on breeding varieties for high fertility anticipating that fertilizer will become increasingly available" and "application of heavy rates of nitrogen on varieties bred for irrigation" (ICAR, 1965, p. 18). The wheat project coordinator, S. P. Kohli, also advocated "breeding for high fertility conditions," a

³¹ Dorris D. Brown, an American extension scientist who spent eight years with the Ford Foundation in India, stated at a 1961 IADP training conference that "the major criteria for selection of a variety for multiplication should be its ability to give higher yields under cultivator's conditions including responsiveness to heavy doses of fertilizer and ample water and plant protection" (Government of India, 1961, p. 10).

departure from earlier breeding goals (*Minutes of the Fourth All India Wheat Research Workers' Seminar*, 1965, p. 7).

One of Borlaug's main allies in India was M. S. Swaminathan. Swaminathan did his graduate studies with Pal, and also at Wageningen in the Netherlands, Cambridge, and the University of Wisconsin, Madison (Perkins, 1997). He joined IARI as a cytologist in 1954 (Perkins, 1997). Swaminathan is known in India for bringing Borlaug to India and catalyzing the introduction of Borlaug's semi-dwarf varieties. Starting in 1963, the Mexican varieties Sonora 63, Sonora 64, Mayo64, and Lerma Rojo 64 were tested in India under irrigated, "heavily fertilized land" (Rockefeller Foundation, 1964, p. 237).

Swaminathan frequently corresponded with Borlaug and often echoed his arguments for breeding under favorable environments. Swaminathan drew from Borlaug to claim that "when a variety is evolved under high fertility conditions, it can also yield well when grown under moderate fertility conditions. It is, however, not possible to achieve the reverse" (Swaminathan, 1965, p. 61). Borlaug and Swaminathan also both agreed on a 'betting on the strong' strategy to test varieties on large plots and under irrigated conditions (Cullather, 2010, p. 201). In 1966 Swaminathan became director of the IARI.

High-ranking scientists such as Kohli, Pal, and Swaminathan, worked in close contact with both Borlaug and Anderson to develop a research program that would produce wheats adapted to high fertility conditions. Other Indian wheat scientists also quickly adopted Borlaug's progressive view. P. N. Bahl, of the IARI's Department of Genetics, argued in 1966:

I am highly convinced that all selection work should be done under highly fertile conditions. We should not dissipate our energies by having separate programmes for different kinds of fertility levels. A variety that gives high yield under high fertility and adequate moisture conditions usually proves to be also good under low or average fertility. (*Proceedings of the 5th All-India Wheat Research Worker's Conference*, 1966, p. 243)

Bahl and other scientists in India argued that breeding under a set of completely different environmental conditions, namely high fertilizer and irrigation, would result in varieties also suited for marginal conditions. By 1967, an IARI report stated that, “selection under favourable conditions appears to be of promise both in breeding for drought resistance and in breeding for wide adaptability” (IARI, 1967, p. 16). High-profile Indian scientists justified research based in favorable agro-climatic conditions under the questionable premise that Borlaug’s widely adapted wheats would perform equally well if not better than local varieties under sub-optimal conditions.

Crisis in 1965 and Changes in India’s Fourth Plan: From Social Equity to Concentrated Capitalism

Scholars recognize that 1965–66 was a pivot point in Indian agriculture in response to both climatic and political factors (Perkins, 1997; Cullather, 2010; Patel, 2013). From 1965 to 1966, President Lyndon B. Johnson and his national security advisers used PL480 program as a bargaining tool with India to promote a transition from food aid to self-sufficient Indian grain production (Ahlberg, 2007). Simultaneously, India

faced both a failed monsoon (leading to drought) and a war with Pakistan in 1965 that put the country and its food supply into turmoil.

Instead of the multi-year bargains employed by his predecessors, Johnson strategically used PL480 as a tool to push his political objectives in India, by creating shorter term contracts and threatening to withhold aid (Ahlberg, 2007). Robert Komer wrote to Johnson in 1966:

We finally have the Indians where you've wanted them ever since last April—with the slate wiped clean of previous commitments and India coming to us asking for a new relationship on the terms we want. Circumstances helped (famine and the Pak/Indian war), but seldom has a visit been more carefully prepared, nor the Indians forced more skillfully to come to us (note how little press backlash about US pressure tactics—when it's been just that for almost a full year)... Similarly, you have already proved how our holding back on PL-480 can force India into revolutionizing its agriculture. Once the famine is licked, I'm for continuing to ride PL-480 with a short rein—it will be painful but productive. If these points don't add up to requiring self-help, I'll eat them. (R. W. Komer, March 27, 1966)

Around 1964, Johnson and his advisors pressured the Indian government to move towards fertilizer-intensive agriculture, through importation and production of chemical fertilizers (Cullather, 2010; Weisskopf, 1973). Up to the mid-1960s, the state had not supported internal production of fertilizer due to concerns about it causing unequal conditions (i.e. one state gets a fertilizer factory and others do not) (Cullather, 2010). The

Indian government also highly discouraged foreign investment in fertilizer production in India through policies that required companies to have majority domestic ownership (Weisskopf, 1973). India stuck with these policies until December 1965.

Furthering the context of crisis was the death of Prime Minister Nehru on 27 May 1964. Congress named Lal Bahadur Shastri the next prime minister, and Shastri was more supportive of agricultural reform than his predecessor (Cullather, 2010). Shastri made the food crisis a high priority and appointed C. Subramaniam as the minister of agriculture. Subramaniam supported capital-intensive agriculture as a way to solve the food crisis, and oversaw the centralization of Indian agricultural research. He stated, “If we concentrate our efforts in a given area where we have assured water supply and we have the necessary extension services also concentrated in that area... then it should be possible for us to achieve much better results than by merely dispersing our effort in a thin way throughout the country” (Subramaniam quoted in Frankel, 1969, p. 694).

During Shastri’s term, the Indian government implemented a minimum support price for wheat (to incentivize wheat production) (Abel, 1970), imported large quantities of wheat seed from Mexico, and created the Food Corporation of India. And, despite longstanding opposition from the Planning Commission, in December 1965 Indian congress agreed to import large quantities of fertilizer and allow foreign investment in fertilizer plants (Cullather, 2010; Weisskopf, 1973). In 1965 and 1966 India decided to import wheat seeds from Mexico. The Indian government shipped 250 tons of Borlaug’s Mexican wheat varieties to India for direct planting in irrigated areas, and in 1966,

18,000 tons (Rockefeller Foundation, 1966).³² These shipments were “the largest single seed transaction in history” in response to what Patel called “a political-ecological ‘shock doctrine’” (Patel, 2013, p. 14).

Wide adaptation as a rhetorical strategy. To satisfy economic planners, Indian scientists had to convince them that the wheat program contributed to social equality.³³ The doctrine of wide adaptation was a rhetorical lynchpin in these efforts. Saha’s dissertation showed how during the green revolution, Indian scientists planned “not to restrict cultivation of these high yielding varieties among well-endowed farmers, owning or having access to well-irrigated and fertile tracts, but to reach larger geographic and economic cross-sections.” (Saha, 2012, pp. 98–99). Indian scientists highlighted the wide adaptation of new wheat varieties to argue that overall wheat production could be increased and that farmers over a wide geographic area could benefit from new agricultural technologies, for instance Kohli’s statement that “to be successful under the Indian crop cultivation conditions, the wheat varieties must be adapted to a wide range of environmental conditions” (Kohli, 1968, p. 9). The concept of wide adaptation helped scientists appeal to economic planners while centralizing research.

To ensure the wide adaptation of a variety, Indian scientists tested new wheat varieties under different locations and agronomic conditions, and only approved varieties with high average yields. While most breeding was done under favorable environments, the program coordinators promised that “the extensive breeding material which will

³² The Indian government was also under pressure from the United States, which withheld food aid as a political bargaining device during this time. See Ahlberg (2007).

³³ Economic planners in India favored equally distributing fertilizer across farms, whereas the RF field staff favored concentrating fertilizer on high production potential farms growing Mexican wheats.

become available from the strengthened programs at the main centres will be fully tested under *barani* [rainfed] conditions and the most promising strains will be selected for release to cultivators for *barani* production” and that “a variety performing well under both conditions would be desirable” (ICAR, 1965, p. 18; *Proceedings of the 5th All-India Wheat Research Worker’s Conference*, 1966, p. 25). Thus, the central research facilities argued that they could provide varieties widely adapted across agro-climatic zones and conditions.

Simultaneous with the RF’s involvement in Indian wheat research, the study of wide adaptation soon became a major focus in India’s wheat program—just as it did internationally. Studies focused on how to produce and identify widely adapted varieties of wheat (e.g., through the use of summer nurseries for breeding and testing), as well as the biochemical and morphological characteristics of widely adapted varieties. At the annual research meetings of 1966, 1967, and 1969, sections of the reports were titled “Genetics of wide adaptation in wheat” and “Breeding for wide adaptability” (*Proceedings of the 5th All-India Wheat Research Worker’s Conference*, 1966, p. 226; IARI, 1967, p. 16; *Proceedings of the 8th All-India Wheat Research Worker’s Conference, Vol. I.*, 1969, p. 174). Varieties like Lerma Rojo 64, developed by Borlaug’s program in Mexico, were celebrated for their “adaptability over wide regions” (ICAR, 1966, p. 9).

Scientific justifications for wide adaptation. Many high ranking Indian agricultural scientists argued that selecting under optimum environments could lead to varieties with wide adaptation, even in marginal and drought-prone environments. Indian

scientists produced their own studies to support this standpoint, some of which were cited internationally in adaptation studies. They also relied on international literature, especially from American and Japanese researchers,³⁴ to argue that, “a crop plant which shows adaptive response under non-stress environments could also be able to produce adaptive phenotype under stress environments” (Das & Jain, 1971, p. 83). In other words, a variety bred under irrigated conditions, if widely adapted, could still adapt to drought conditions. Indian scientists used explicitly scientific arguments about wide adaptation to support breeding for favorable environments.

Published articles from 1960 through 1970 show how Indian scientists justified wide adaptation and selection under favorable environments in biological terms. For example, IARI scientists N. N. Roy and B. R. Murty, both of whom studied at Cornell University, produced a study that found varieties selected under high fertility usually had a higher yield than varieties selected under “suboptimal conditions” (1967, p. 481). They reasoned that under suboptimal conditions, environmental variation caused too much phenotypic variability in the test varieties, making selection inefficient.

In a 1970 paper again by Roy and Murty, the authors drew directly from RF scientists: “Krull et al. (1966) reported that wide adaptation is genetically controlled and that varieties do not require to be bred for the particular conditions. This indicates that minimizing G x E [genotype by environment] interaction is important for the choice of environment” (1970, p. 516). Roy and Murty assumed that if wide adaptation was genetic, varieties selected for their yield response under high fertility and irrigated conditions would still produce high yields in marginal conditions. At a 1967 conference,

³⁴ Especially the work of K.J. Frey, of Iowa State University, K. Gotoh and S.I. Osani of Japan.

Roy and R. D. Asana, also from IARI, gave a different biological reason for wide adaptation. They argued:

Although it might be possible eventually to define the characteristics for the ideal plants adapted to a specific environment like drought, it would be much more difficult to define all possible combinations of a range of characteristics necessary to provide good adaptability in an otherwise fluctuating environment.

(Proceedings of the 6th All-India Wheat Research Worker's Workshop, Vol. II., 1967, p. 124)

Roy and Asana argued that it was better to have a variety that could adapt to a range of conditions than one that only performed well under the specific condition of drought.

Many of the scientists who argued for wide adaptation and selection under favorable environments were affiliated with the IARI. As previously noted, the IARI served as a hub for the Indian coordinated wheat program, and benefitted from the centralization of power at its New Delhi headquarters. The next few sections will show that outside of the IARI research stations, theory did not match practice in most of the country.

Questioning Wide Adaptation and Favorable Selection Environments

The Mexican wheats were touted as widely adapted, and fast-tracked for release in India largely because of this (Government of India, August 6, 1965). Not all Indian scientists agreed with this decision. In light of the research focus on irrigated wheat in northwest India, some scientists wondered whether the wheat program would also serve the needs of the extensive area of marginal agriculture. Scientists who questioned the

release of Mexican wheats also tended to argue against centralization of research, breeding under favorable environments, and wide adaptation. These dissenting scientists argued that more localized research programs were needed to address the variety of agro-climatic conditions in India, specifically farming under low fertility and rainfed conditions.

Most dissents against selecting wheat under favorable environments do not appear in the official records. At least one strong claim remains from J. S. Kanwar, a respected soil scientist and Deputy Director General of ICAR at that time. At a 1969 wheat research meeting, Kanwar stated:

It will be drought tolerant or drought escaping varieties that are badly needed for different trials. It is suggested that the breeding programme for wheat be oriented to develop varieties for rainfed conditions. An active testing and demonstration programme is also required... There is a need for an active programme of selection of varieties for rainfed conditions. (*Proceedings of the 8th All-India Wheat Research Worker's Conference, Vol. I., 1969, pp. 34–35*)

Kanwar reacted strongly against the push for the Mexican varieties on a wide scale, and for further breeding and testing under only highly fertilized, irrigated, and managed conditions. Kanwar, while not explicitly disagreeing with wide adaptation, highlighted that “there is very inadequate data about the adaptability of new high yielding varieties for rainfed and unirrigated conditions” (*Proceedings of the 8th All-India Wheat Research Worker's Conference, Vol. I., 1969, p. 36*). Kanwar and a few other scientists resisted the new regime of research, and held to the more traditional viewpoint that crops should be

bred in the conditions that they are grown. Even Cummings, director of the RF's Indian Agriculture Program, was skeptical of how easily the Mexican wheats would perform in marginal areas, stating that, "as a complementary variety, the best Indian variety should be recommended" in 1965 (Government of India, August 6, 1965). Cummings' hesitation likely stemmed from his experience with hybrid maize in India, which was poorly adapted to Indian conditions. He was also concerned about the availability of irrigation and fertilizers (Cummings, March 17, 1964).

Other Indian scientists began to note that widely adapted wheats from Mexico did not live up to their reputation as universally high yielding. At a 1967 conference, S. M. Gandhi, a wheat scientist working in the mostly dryland state of Rajasthan, reported that "yield results obtained under rainfed conditions are particularly interesting in the present context of intensive efforts in breeding short strawed varieties. C306, CA82, K65 and D144, all tall varieties, have come in the first group of highest yielding entries in the series under rainfed conditions," although the tall varieties did not perform well under high fertility conditions and intensive irrigation (*Proceedings of the 6th All-India Wheat Research Worker's Workshop, Vol. II., 1967, p. 17*). In other words, the "Mexican wheats were superior under intensive farming conditions while, Indian varieties were superior under average or below average conditions" (*Proceedings of the 6th All-India Wheat Research Worker's Workshop, Vol. II, 1967, p. 12*). In some cases, local varieties were not even included in trials to compare them to Mexican wheats, as Kanwar reported, "it is also not possible to conclude anything regarding the performance of high yielding varieties as different varieties including local variety was not compared. More trials are

needed” (*Proceedings of the 8th All-India Wheat Research Worker’s Conference, Vol. I.*, 1969, p. 34). Quantitative data from the Indian wheat trials appears to support Gandhi’s claim.

Meanwhile, RF agronomist Bill C. Wright noted that in state of Gujarat, “tall varieties have consistently yielded as good or even better than the dwarf wheats,” but he considered this “unusual” (Wright, August 29, 1967). Then in 1967 Indian scientists decided to not include the Mexican varieties in the southern Peninsular zone experiments because the Indian varieties had consistently higher yields. This prompted one Indian scientist to argue, “I do not agree with Dr. Upadhyaya that dwarf varieties have no place in the Peninsular Zone if proper agronomy is practiced” (*Proceedings of the 6th All-India Wheat Research Worker’s Workshop, Vol. II.*, 1967, p. 17). In response Y. M. Upadhyaya, working for IARI in Indore, Madhya Pradesh, simply stated, “*proper agronomy is not possible under farmers’ conditions*” [emphasis added] (*Proceedings of the 6th All-India Wheat Research Worker’s Workshop, Vol. II.*, 1967, p. 17). “Proper agronomy,” which we can assume meant timely application of fertilizers and irrigation, was a necessary part of the package for Borlaug’s widely adapted wheats. His entire wheat program rested on the assumption that wheat would be grown in controlled, carefully managed, and favorable field conditions.

Mexican varieties and fertilizer: unfulfilled expectations. In 1965, after two years of testing, the Mexican varieties did not perform as well as expected, due to their different agronomic needs from the tall Indian varieties, such as need for more irrigation and a shallower planting depth. The Mexican varieties initially germinated poorly in

India, which may have been due to chemical damage when they were transported from Mexico. The lack of assured irrigation and conditions of drought also limited their yield (Sen, 1974).

Yet most scientific discussions after 1965 centered not on whether the Mexican varieties should be promoted, but rather on the amount of fertilizer to be used in field experiments.³⁵ Anderson argued that field trials did not have high enough levels of fertilizer, arguing that, “the Mexican varieties show their potential only under very high fertility levels,” directly contradicting Krull’s finding that “the dwarfs respond to but do not necessarily require irrigation and extremely heavy fertilization” (*Minutes of the Fourth All India Wheat Research Workers’ Seminar*, 1965, p. 14; Krull, January 5, 1967).

In response to the failure of Mexican wheats in the early coordinated trials, a “committee on high fertility and agronomic trials” from the Indian coordinated wheat program decided that for the next season, the Mexican varieties should be tested in separate trials with high levels of fertilizer (*Minutes of the Fourth All India Wheat Research Workers’ Seminar*, 1965, p. 42). In fact the committee decided in 1967 that these Uniform High Fertility Trials would *only* test at 135 kilograms of nitrogenous fertilizer per hectare (120 pounds per acre), the level that Borlaug recommended for semi-dwarf wheat (Borlaug, April 12, 1966; *Proceedings of the Seventh All India Wheat Research Workers’ Workshop*, 1968). Yet the average annual rate of nitrogenous fertilizers applied to cropland in India between 1965 and 1970 ranged from only 3.7 to 8.2 kilograms per hectare, roughly 25 times less than the rates used in high fertility

³⁵ Some discussion of whether the varieties should be released occurred at the Fourth All India Wheat Research Workers’ Seminar in 1965.

testing (The Fertilizer Association of India, 2012). Only in recent years has the average fertilizer use in India even approached 135 kilograms of nitrogenous fertilizer per hectare.

The initial poor performance of the Mexican varieties in India concerned both scientists and economists, from India's Planning Commission. A letter from W. David Hopper, of the Ford Foundation in India, to Borlaug stated,

The Mexican developed varieties gave only marginally better responses to nitrogen application, and on the basis of these results the Planning Commission is raising questions about the policy of the Agricultural Ministry that seeks to concentrate nitrogen, which is in short supply, primarily in the areas where the exotic varieties will be grown. (Hopper, February 10, 1966)

Further, one Planning Commission member asked "whether the Indian varieties which showed high response to fertilization at the lower levels may be fully exploited instead of using heavy doses of the limited quantity of the fertilizer with the Mexican varieties... especially when there is a shortage of fertilizers in the country" (*Minutes of the Fourth All India Wheat Research Workers' Seminar*, 1965, p. 14). The Indian economic planners believed that fertilizer should be distributed equally throughout the country, which Borlaug and Anderson strongly resisted ([Oral history of Norman Borlaug], 1967, p. 196; Government of India, August 6, 1965).

The fertilizer situation threatened the entire premise of the RF's Indian wheat program, which was based on the concentration and availability of fertilizers in irrigated regions. From a 1966 RF field directors meeting, Borlaug "said there is a complete lack

of understanding among people in government of the kinds and quantities of fertilizer needed. Expanding fertilizer capacity requires a great deal of capital which raises the problems of foreign investment versus nationalism” (*Report and summary of the agricultural science field directors' meeting*, 1966, p. 8). Borlaug wrote in 1966, “I am against this dispersion and dilution of fertilizer application, and instead feel that the program should try to produce tremendous yield increases on the area where the dwarf varieties can be heavily fertilized and properly watered” (Borlaug, April 12, 1966, p. 11). He was still convinced that the Mexican wheat agenda should move ahead as planned. In 1967 Borlaug encouraged his collaborators in India and Pakistan to “Get them [the seeds] multiplied—abandon the three year yield testing program that is being followed” [emphasis original] (Borlaug, October 15, 1967).

Mexican varieties and irrigation: an uneven landscape. Whether or not the Mexican varieties could perform well at lower levels of soil fertility, the level of irrigation or rainfall was likely more important. The Mexican wheats required irrigation at precise times, which was not usually possible under prevailing irrigation systems. Borlaug and his colleagues had introduced the semi-dwarf wheat into the commercial irrigated areas of Mexico, but India was a different landscape in that the majority of wheat cultivation was rainfed. Scientists and planners alike found classifying landscapes as rainfed or irrigated to be problematic (Sen, 1969).

Scientists affiliated with the Ford and Rockefeller Foundations noted the lack of a steady water supply even in areas classified as irrigated. Anderson wrote to Borlaug in 1966,

We have as yet not had any winter rains in the Delhi area and the crops which are not under irrigation are very thin and I am afraid yields will be quite low. Even on the irrigated acreage that is under canal control, many of the crops are suffering appreciably from lack of water because the canals have not been delivering water at regular intervals. (Anderson, January 24, 1966)

Cummings, upon visiting a tubewell project in Uttar Pradesh, wrote, “the technology in establishing and maintaining the wells was extremely poor. They have much too a great number of power failures and actual burning out” (Cummings, February 5, 1966). Indeed, many of India’s canals “were designed to provide protection against drought rather than productive irrigation” (Sen, 1974, p. 27).

Quantifying Wide Adaptation: The Case of Lerma Rojo 64

Despite the aforementioned inconsistencies, Borlaug and Anderson fiercely promoted the release of Lerma Rojo 64 in India on the basis of its high yields and wide adaptation. India imported Lerma Rojo 64 in large quantities in the 1960s, and it was one of the first Mexican varieties approved in India. Borlaug himself stated in his 1967 oral history that Lerma Rojo 64’s most important trait was its “wide adaptation—fertilized or unfertilized” ([Oral history of Norman Borlaug], 1967, p. 196). Anderson recalled at a 1969 conference in India that “I am firmly convinced that the adoption of cultivation of dwarf wheats in this country was materially hastened through the wide adaptation of Lerma Rojo” (*Proceedings of the 8th All-India Wheat Research Worker’s Conference, Vol. I.*, 1969, p. 174). For a few years in the 1960s, Lerma Rojo 64 gave outstanding yields in the RF’s international wheat yield trials, which Borlaug took as proof of its wide

adaptation. Historical evidence, however, suggests that Lerma Rojo 64 is adapted to specific conditions of high fertility, but was able to be grown over a wide area due to its photoperiod insensitivity and its high response to fertilizers.

Borlaug frequently used *Figure 7* **Error! Reference source not found.** to support the superiority of Lerma Rojo 64 to tall Indian varieties such as C306.³⁶ This figure shows the yield of three varieties—Sonora 64, Lerma Rojo 64, and C306—grown under fertilizer levels ranging from 0 to 200 kgs/ha. It clearly shows that C306 marginally responds to higher fertilizer, while the two Mexican varieties increase in yield proportionally to the fertilizer level. A version of this figure appeared in the 1965–66 RF Agricultural Sciences Program Annual Report, and Borlaug included this figure in various letters, publications, and talks about the RF’s Indian Agricultural Program.

³⁶ The data for the figure was based on an agronomic test performed in 1966 by K. C. Sharma and D. Misra of Pantnagar and Wright of the RF.

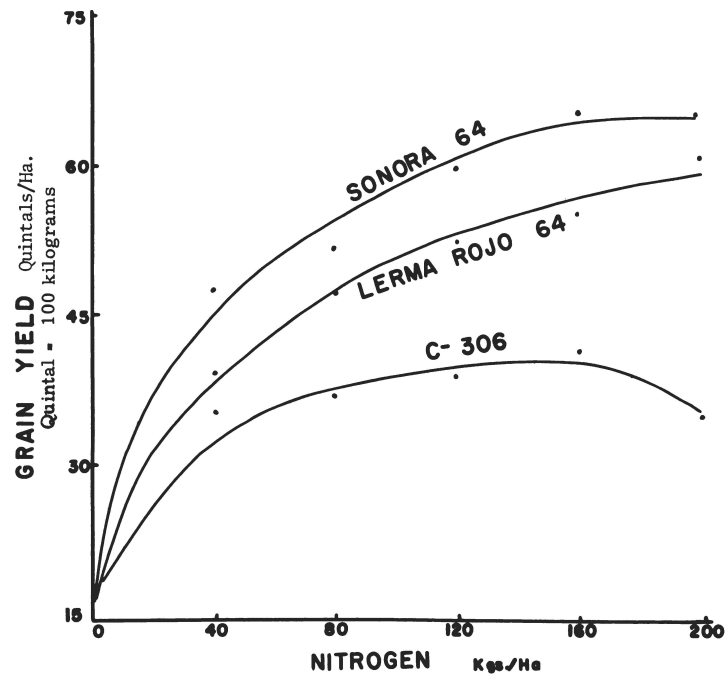


Figure 1. Differential nitrogen response curves for two Mexican dwarf wheat varieties compared to one of the best tall-strawed Indian varieties C 306, at the Uttar Pradesh Agricultural University, Pantanagar, U.P., India in 1966. Data by Drs. K. C. Sharma, D. Misra and B. C. Wright.

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Figure 7. Agronomic response to fertilizers of the wheat varieties Sonora 64, Lerma Rojo 64, and C306. Image reproduced from Norman Borlaug (1972b, p. 634) and digitized by Google, Inc.

Borlaug's colleague in Australia, Keith W. Finlay, wrote to Borlaug in 1966 that "Lerma Rojo 64 is of particular interest because it is even less stable—indicating that it is better adapted to 'higher yielding' environments" (Finlay, September 6, 1966). Finlay based his analysis on a model that he developed with statistician Graham N. Wilkinson in 1963. The pair released an experimental design and mathematical model that measured the yield stability, or adaptation, of a plant variety over multiple locations. Finlay applied this model to Borlaug's data from the international wheat tests, finding that while the group of Mexican varieties were generally higher yielding and more stable across locations, the results varied by year. Facing his data own analyzed by a new method, Borlaug responded to Finlay's concern about Sonora 64's low yields:

In trying to develop as rapidly as possible a dwarf variety with good gluten quality, we settled for a variety, such as Sonora 64, with less yield stability. This variety is high yielding when grown under heavy fertilizer conditions and is properly irrigated... *but it will produce lower yields generally when in the hands of the average farmer.* [emphasis added] (Borlaug, July 7, 1965)

Despite Finlay's critique and this admission, Borlaug canonized Lerma Rojo 64 for its wide adaptation across conditions.

In light of the confusion over Lerma Rojo 64's wide adaptation, I used Finlay and Wilkinson's analysis to test the adaptation of Lerma Rojo 64 and C306 using yield trial data from India (*Figure 8*).³⁷ Unlike Wright's figure that plotted only yield of a variety by fertilizer level, Finlay's model plots the yield of a variety over the average yield at that

³⁷ Data was collected from the *Results of the Coordinated Wheat Trial Results, 1964–65 and 1965–66* for the Northwest Plain Zone Uniform Regional Trials. Directorate of Wheat Research library, Karnal, and Indian Agricultural Research Institute archives, New Delhi.

location (a proxy for the environmental index), and then uses a regression analysis. A variety with a high regression coefficient (slope) is better adapted to favorable environments, and a low slope indicates stability across conditions, or wide adaptation. A slope of one is average stability.

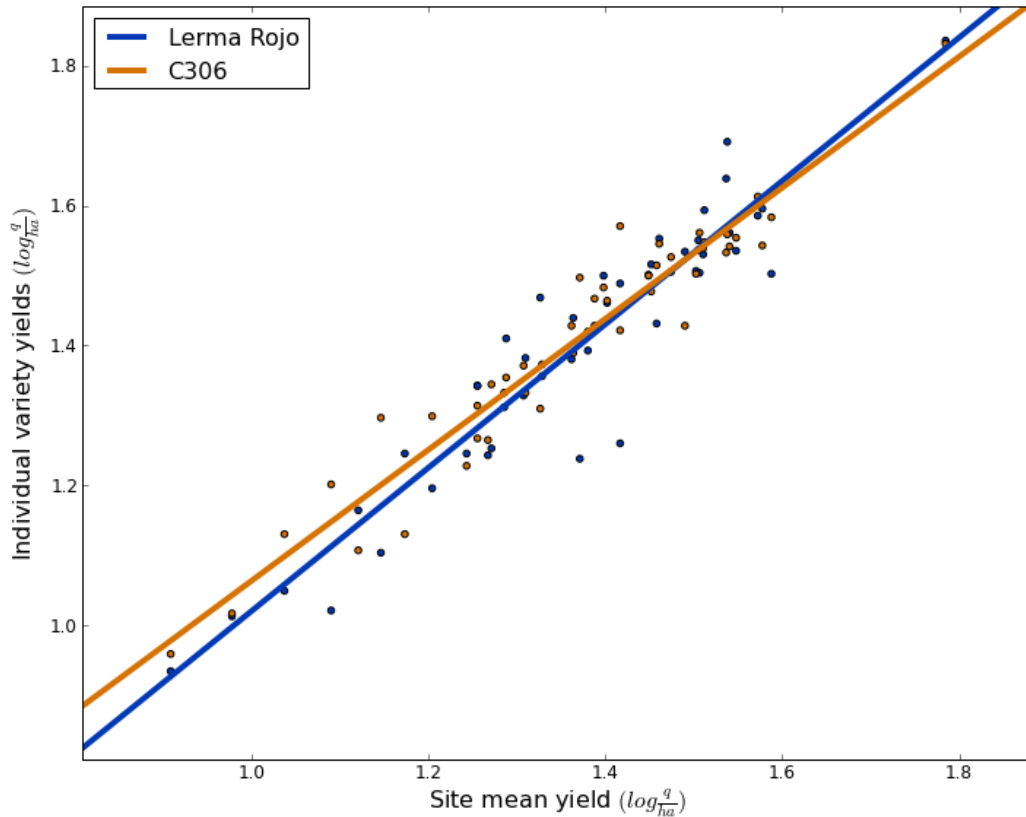


Figure 8. Regression lines of varieties C306 and Lerma Rojo for 1964–65 and 1965–66 wheat seasons in northwest India, over both rainfed and irrigated conditions. Lerma Rojo’s average yield was 27.3 ± 11.2 quintals per hectare (q/ha) and C306 was 27.5 ± 10.0 q/ha. Figure rendered by Erick Peirson.

C306 and Lerma Rojo 64, tested under this wide range of locations and conditions,³⁸ did not have significantly different yield responses according to Finlay’s model. Both varieties show average stability (a slope near one), though C306 is more

³⁸ This included both irrigated and rainfed conditions, and nitrogen fertilizer levels that ranged from 45 and 90 kgs/ha in 1964–65 and in the range of 0 to 120 kgs/ha in 1965–66.

stable (slope of 0.936) and Lerma Rojo 64 is more slightly adapted to better environments (slope of 1.024) (Table 4). But if I follow Finlay's 1966 analysis of Borlaug's data and separate the data into irrigated and rainfed conditions, the results change (Figure 9, Figure 10). While both varieties performed about equally under irrigated conditions under rainfed conditions C306 was more stable and higher yielding than Lerma Rojo 64.³⁹

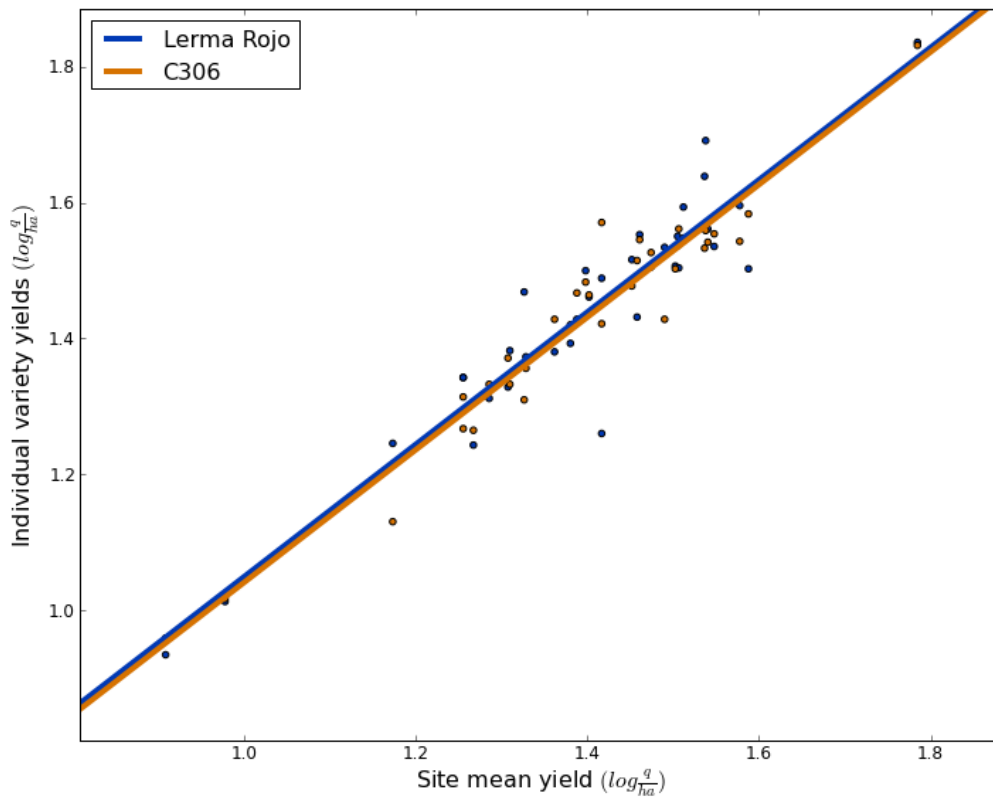


Figure 9. Regression lines of varieties C306 and Lerma Rojo for 1964–65 and 1965–66 wheat seasons in northwest India, over only irrigated conditions. Figure rendered by Erick Peirson.

³⁹ Using the raw data under rainfed conditions, Lerma Rojo had an average yield of 21.14 q/ha, whereas C306 was 23.58 q/ha, $p=0.088$.

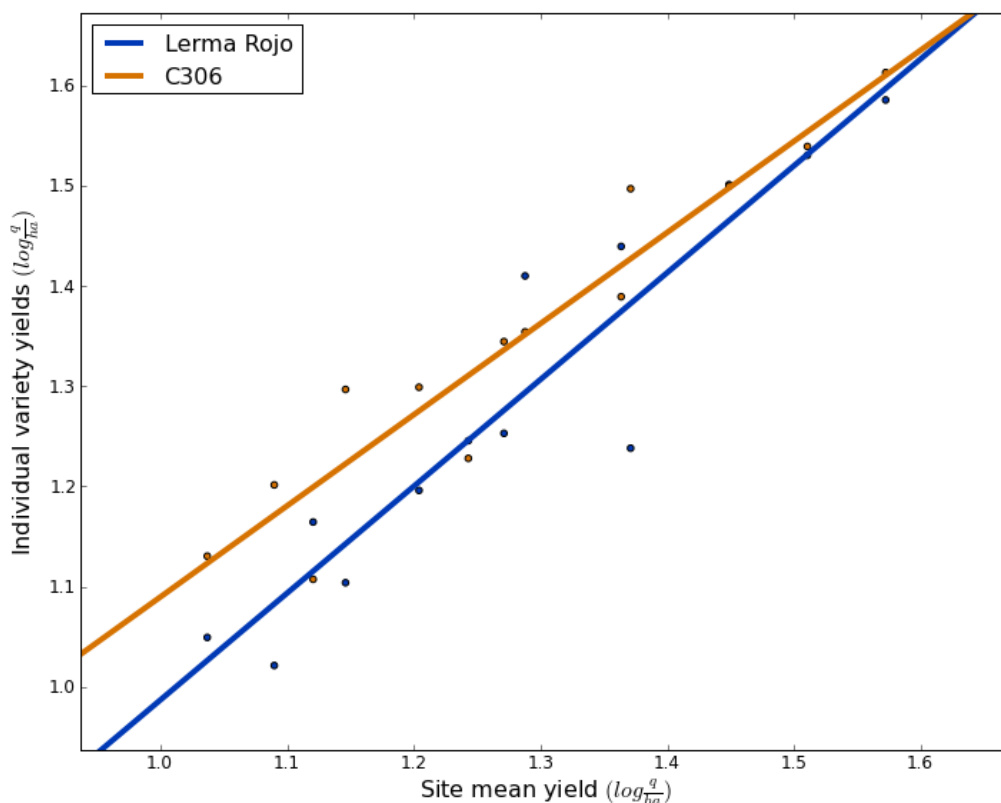


Figure 10. Regression lines of varieties C306 and Lerma Rojo for 1964–65 and 1965–66 wheat seasons in northwest India, over only rainfed conditions. Figure rendered by Erick Peirson.

Table 4

Results of Analysis of the Varieties C306 and Lerma Rojo Based on Slope and Average Yield under Irrigated, Rainfed, and Both Conditions.

	Both conditions	Irrigated only	Rainfed only
C306 slope	0.94	0.98	0.91
Lerma Rojo slope	1.02	0.97	1.07
C306 average yield (q/ha)	27.5	28.9	23.6
Lerma Rojo average yield (q/ha)	27.2	29.6	21.1

My analysis of this data suggests that Lerma Rojo 64 is adapted to irrigated conditions, and it more strongly suggests that C306, the tall Indian variety, is not only widely adapted, but is better adapted to rainfed conditions. Even under irrigated conditions and varying levels of fertilizer, Lerma Rojo 64 did not have higher average

yields than C306. Trials conducted at higher fertilizer levels could have different outcomes, given Lerma Rojo 64's fertilizer responsiveness.

In light of the evidence presented, I agree with B. H. Farmer's analysis of the adaptation of green revolution rice varieties in South Asia (Farmer, 1979). Farmer wrote that green revolution era scientists introduced varieties were photoperiod insensitive, thus making them adapted to a broad range of locations, given adequate inputs. RF affiliated scientists, however, claimed a second type of adaptation: adaptation across varied environments that included pests, monsoon, and drought. Farmer wrote that, "the two kinds of adaptability have been confused, or at any rate not sufficiently distinguished" (1979, p. 308). This appears to be the case with Borlaug and the Mexican semi-dwarf wheats as well, which were photoperiod insensitive and thus better adapted to Indian conditions than other foreign wheats. Mexican wheats showed wide adaptability across locations with similar agro-climatic conditions and management, not across field conditions of the average farmer.

Conclusion

Scientists in the green revolution era defined wide adaptation based on convenience to their research agenda, and unsurprisingly, they gravitated towards data that proved their agenda. Although the Mexican semi-dwarfs did have higher average yields under certain conditions, time has revealed that only a small area of India fit those constraints. Further, even varieties that perform well in low fertility experimental conditions do not always have that same performance in farmers' fields (Dawson, Murphy, & Jones, 2008). The actual performance of Mexican varieties in India in the

1960s, through both qualitative and quantitative historical evidence, supports this analysis.

By the time critiques of the green revolution emerged in the late 1960s and early 1970s, Indian wheat science had embraced the dogmas of wide adaptation and selection under favorable environments and codified it into the wheat program (*All-India Wheat Research Worker's Workshop [9th annual], Vol. IV, 1970*). In this author's interviews with Indian wheat scientists in 2013, most scientists agreed that wheat should be bred for wide adaptation, and made statements such as, "every breeder likes to go for developing the material which can fit everywhere." Although controversy still exists around the conflicting goals of wide versus specific adaptation (Swaminathan, 1993), the institutional structure of the Indian wheat program effectively weeds out alternative views.

Contemporary scholars argued that wide adaptation and the strict institutional structure of Indian agricultural research have discouraged alternative views on decentralized research for marginal environments, where the majority of farmers deal with variable agro-climatic conditions such as drought and heat stress (Cecceralli, 1989; Cleveland, 2006; Mishra, Ravindra, & Hesse, 2013; Raina, 2011a; Witcombe, Dirk, & Farrington, 1998). While marginal farmers in India may benefit from higher wheat yields if they have access to fertilizer and irrigation (or during years with adequate rainfall), the mainstream research system has mostly ignored alternatives such as different cropping systems or, until recently, varieties that can better withstand climatic variations and stress. Further, global climate change and other agro-ecological concerns will determine

the future of wheat production in India. A more diverse scientific and institutional approach to wheat research in India is necessary to successfully adapt agriculture to 21st century challenges.

CHAPTER 4 INTERNATIONAL WHEAT PROGRAMS IN THE WAKE OF THE GREEN REVOLUTION

The International Maize and Wheat Improvement Center (CIMMYT) continued to expand their wheat program internationally in the 1970s, bolstered by the declaration of the green revolution in 1968 and Norman Borlaug's Nobel Peace Prize in 1970. During this expansion of CIMMYT's wheat program and the establishment of new national and international research networks in Turkey and the Middle East, agricultural researchers faced new pressure from their administrators and from external critics to address a new agenda: rural equity. Researchers also attempted to improve wheat production and rainfed and dryland areas of the world, as they realized that the gains in production through green revolution wheat technology had only spread to irrigated areas.

In the first part of this chapter, I examine Indian wheat research in the wake of the green revolution. While Chapter 3 demonstrated the significant shift of Indian wheat breeding goals during the tumultuous mid-1960s, the late 1960s and 1970s brought serious critiques of changes in the system. With the departure of RF staff from Delhi, Indian scientists also asserted their own project goals and more critically examined assumptions about wide adaptation. Influential factors in this period included new administrators of the coordinated wheat project; new analyses of wheat stability; and ongoing failures of green revolution wheat varieties—including the “over-success” of two Mexican-derived wheat varieties, Kalyansona and Sonalika.

In the second and third parts of this chapter, I document the expansion of CIMMYT's wheat program in the Middle East and Turkey. CIMMYT shifted many of their staff from the RF's Indian agricultural program to Turkey and to lead new

international research centers in the mid-1970s. I also examine CIMMYT's shift to research on rainfed dryland areas, including the Plan Puebla for maize in Mexico and the formation of the International Center for Agricultural Research in the Dry Areas (ICARDA).

Part 1: Indian Wheat Research in Late 1960s and into 1970s

By 1968, the narrative of the wheat revolution was solidified in India. Indian farmers had adopted new semi-dwarf wheat varieties at rapid rates, and overall wheat production increased (*Wheat: Total Production, Total Area, and Area Under High-yielding Varieties (HYV) by States*, 1970). But soon after 1968, both the excitement over semi-dwarfs and the panic over the food crisis faded. Top-level scientists, administrators, and policy-makers recognized that the existing wheat program had mostly benefitted large farmers with access to controlled irrigation and cheap fertilizers. While wheat production did increase, the concentrated investment in irrigated areas led to disparities in equity. Consistent with existing social policy, many of these actors pushed for more research directed at rainfed and marginal agriculture, not to mention the large area of dryland wheat in central and southern India that had not adopted new wheat varieties.

External criticisms of Indian wheat research addressed both social and food security aspects. Socially, critics pointed to the disparity of regional and economic impacts, beginning with Francine Frankel and Wolf Ladejinsky's respective studies on the inequities caused by the green revolution (Chakravarti, 1973; Cleaver, 1972; Frankel, 1969; Ladejinsky, 1969, 1970). But perhaps more worrisome, a 1973 report by USAID

found that when comparing overall grams of cereals produced to the population in India, the Green Revolution had not made a significant impact. RF staff wrote in response:

Further increments of gain can be achieved through continued emphasis on high yielding varieties, fertilizers and irrigation in systems designed to maximize production per unit area over time. But, as indicated above [by the USAID report], India has made no real progress in improving her people-food equation in the decade of 'the green revolution' and there's no new agricultural technology on the drawing board as glamorous and promising as was the HYV's at the beginning of the decade. (Rockefeller Foundation Indian Agricultural Program, 1973)

Despite these limitations, the green revolution had nonetheless become canonized among agricultural scientists as preventing mass starvation in Mexico, India, and Pakistan.

Kalyansona and Sonalika: wide adaptation gone too far? In 1967, the Indian Council of Agricultural Research's Central Variety Release Committee approved the release of two new semi-dwarf wheat varieties, known as Kalyansona and Sonalika.⁴⁰ Indian scientists selected Kalyansona and Sonalika from segregating lines of Borlaug's semi-dwarf wheats from Mexico (Nene, 1970; Swaminathan, 2009). These varieties quickly replaced the direct introductions from Mexico—Sonora 64 and Lerma Rojo 64—which both had an undesirable red grain color.

Promoted on a wide scale throughout north India for their yield potential, farmers rapidly adopted Kalyansona and Sonalika in both northwest and northeast India and

⁴⁰ The Central Variety Release Committee and State Variety Release Committee were the two governing bodies over the release of government-approved seeds in India. For more information on the process of seed approval and notification, see Chapter 6 of Witcombe, Virk, and Farrington, 1998.

under both irrigated and rainfed conditions. For over a decade, these two varieties dominated wheat production in north India (Singh & Pawar, 2006), until the release of several new varieties in the mid- to late-1970s including HD2009, WL711, and WH147 in the northwest, all three of which spread quickly, and UP262 in the northeast, which took almost another decade to become popular in the northeast. C306, the tall Indian variety, also remained popular in both the northwest and northeast for rainfed conditions.⁴¹

Kalyansona and Sonalika exemplified Borlaug's research program for wide adaptation. But as one can imagine, the dominance of one or two varieties is not optimal for disease resistance. From 1967 to 1977, sixty-eight wheat varieties were released in India, and only five of these were approved for multi-zonal release⁴² (Munshi, 2004). Yet these multi-zonal varieties ruled over zonal or state-specific varieties, creating a monoculture vulnerable to pests and disease (Nene, 1970). M. V. Rao, coordinator of the Indian wheat program, stated in 1975, "Large tracts in the country are occupied by very few varieties primarily Kalyansona and Sonalika. This is a dangerous situation and if any new race of any pathogen comes up in severe form, the situation would be disastrous" (Rao, 1975, p. iv).

Other scientists observed that Kalyansona and Sonalika were thermosensitive (sensitive to temperature) during their growth stage. This could result in reduced yields,

⁴¹ It is not surprising that C306 remained popular in rainfed areas: according to results of the regional trials in India, C306, used as a check against new varieties, frequently appeared in the top one or two best performing varieties on a zonal basis for rainfed conditions from 1964 through 1989. Based on data aggregated from coordinated trials.

⁴² Approved by the Indian Council of Agricultural Research's Central Variety Release Committee.

an especially significant problem if farmers were not growing other varieties of wheat to offset their losses. Rao stated in 1977:

Varieties like Sonalika, Kalyansona suffered the most due to high temperature. On the other hand locally bred or selected materials... were least affected. This only shows that we should not neglect the local germplasm which got evolved over a period of time and which developed its own buffering mechanisms to face the adverse weather like the one we experienced this year. (Rao, 1977, p. 1.2)

J. P. Srivastava found similar results, and he wrote to the RF's R. Glenn Anderson in 1972:

I have data indicating differential response of varieties to higher temps in the early stages of plant growth. The indigenous Indian varieties grow and tiller normally in spite of above normal temperatures. Short duration varieties like Sherbati Sonora, Hira, UP301, UP310, Sonalika and Safed Lerma, at higher temps, completed their vegetative growth rather quickly and entered into reproductive growth. This resulted in reduced tillering, stunted growth, small ears, increased sterility and low yields. (Srivastava, August 9, 1972)

And despite Kalyansona's apparent wide adaptation, group of authors from Punjab Agricultural University found that Kalyansona was adapted to favorable environments rather than stable over environments, though it did have a higher than average yield across environments (Bhullar, Gill, & Khehra, 1977). One might wonder whether these biophysical problems might cause scientists to reconsider wide adaptation as a primary goal of the wheat program.

The limits of research. The 1970s also highlighted new problems in the wheat research system and administration. First, Kalyansona and Sonalika's dominance highlighted the gap between the varieties scientists released, and what varieties the government seed company decided to multiply and sell. In 1974 Rao asked "why many of the new varieties we are recommending for release did not catch up with the farmers. Is something wrong with our present system of testing and release?" (Rao, 1974, p. x). Rao also noted that:

In certain states the spread of the new varieties is very fast while in others it is indifferent, as a result, farmers unaware of the new varieties are growing only the old varieties. A critical and unbiased review of the whole system of varietal release, multiplication, popularisation, replacement and implementation of a sound policy in this respect is warranted. (Rao, 1980, p. 18)

Scientists were aware of issues within their own wheat research system, but perhaps due to the structure of the system, individual scientists did not consider it their responsibility to become involved in promoting new varieties—that was considered the job of "extension." But a later CIMMYT report found that one of the weak links in India's seed production system was the lack of "breeder's seed," which is seed that plant breeders are supposed to provide to the seed companies for further multiplication (CIMMYT, 1984).

Anderson reviewed the Indian wheat program near the end of his tenure there, and his notes were not optimistic. He wrote to Borlaug that the coordinated wheat program was not officially funded through the government until the very end of 1969 (Anderson, December 20, 1969), causing him to be "literally ashamed to go out and ask the various

cooperators to carry out essential actions when they have no money” (Anderson, November 22, 1969). In comparison, the coordinated rice project and other crops were funded by the Indian Council of Agricultural Research (ICAR) at least as early as 1965 (Sen, February 17, 1967). It is not clear why funding for the coordinated wheat program was delayed for so long: whether it was a reliance on RF staff or a political issue.

Anderson ended up leaving India in May 1971 to work with CIMMYT in Mexico, effectively ending the RF’s direct involvement in the Indian wheat program (though it was continued somewhat through other RF and FF field staff). On a visit back to India in November 1973, Anderson found a lack of labor for the coordinated trials, and the trial plots were “only half plowed after harvest of the corn crop,” not irrigated or leveled, and a forty percent shortage of fertilizer needed for *rabi* (the season in which wheat is planted) crops in the country (Anderson, November, 1973, p. 1). He also found that due to the lack of manpower and coordination, more varieties were being released than needed, due to fractured research efforts.

Poor administration and stifling bureaucracy also limited the Indian wheat program. For example an internal RF report stated that the Indian Agricultural Research Institute (IARI) “minimize[d] contributions of research in the coordinated cereal improvement projects made by stations other than IARI or its regional stations,” in favor of research done at the IARI headquarters in Delhi (RF Indian Agricultural Program, n.d.). The same report mentioned the “duplication of effort on research both within and between Divisions... Even within a given Division research on a particular crop is often far from coordinated” (RF Indian Agricultural Program, n.d.). There was also an issue of

ineffective leadership from S. P. Kohli, the wheat program coordinator, who was “being forced out by ICAR” as well as IARI in 1970 (RF Indian Agricultural Program, 1970). Kohli soon moved to work in the Middle East with the United Nations Development Program and M. V. Rao, a geneticist at IARI, took over as coordinator.

Finally, K. Kanungo and P. E. Naylor, in a report for the World Bank, wrote a scathing review of the coordinated crop improvement programs in the mid-1970s. They found that the “common denominator of the gaps identified in almost all types of research activity is the *weakness of the link between research and the actual needs of farmers in their specific location and environment*” [emphasis added] (Kanungo & Naylor, ca. 1975, p. 5). According to them, the new coordinated system had in fact centralized research and supplanted local research efforts, while scientists had little incentive to address local issues. Kanungo and Naylor proposed developing decentralized, regional research facilities “on the basis of agro-climatic suitability in order to strengthen a reorganized” research system (Kanungo & Naylor, ca. 1975, p. 6). Based on this review, the World Bank and ICAR started a National Agricultural Research Project in 1979, which continued until 1996 (Balaguru, Venkateswarlu, & Rajagopalan, 1988). This program, funded by the World Bank, was meant to support state-level and interdisciplinary agricultural research efforts in India, and one of its first tasks was delimiting 127 agro-climatic zones within India.⁴³

Biophysical limitations on the Green Revolution and an increase in research on rainfed conditions. In India, rainfed agriculture typically means areas that still receive adequate rainfall, but are not irrigated (prevalent in the northern wheat belt),

⁴³ A map of the 127 agro-climatic zones is available at <http://www.imdagrimet.gov.in/node/290>

whereas dryland specifically refers to unirrigated regions in central and southern India (Byerlee, 1992). At a 1968 dryland wheat conference held in Bangalore, scientists presented arguments and research results that were scarcely seen in the mainstream documentation of wheat research in north India. In Bangalore there was no celebration of the wheat revolution. K. C. Naik stated, “any talk of Agricultural revolution in an area subject to the worst scarcity conditions in the country and in a year when 17 out of the 19 Districts [in the state of Mysore] were in the grip of drought would be unrealistic, if not flippant” (Naik, 1972, p. 3). He spoke further that “it is high time States like Mysore shift the focus on drought and dry farming rather than imitate and adopt programmes which are designed exclusively to help the farmers with assured irrigation facilities” and that a focus on irrigated agriculture would lead to further economic inequity (Naik, 1972, p. 3).

Scientists from Madhya Pradesh found that “light insensitive varieties also require a precise management, particularly timely irrigation schedule” and that in rainfed areas that dominated the state, “photoinsensitive varieties, under such conditions are not suitable. The local varieties appear to be better adapted to such variable conditions” (Sisodia, 1977, p. 173). These views from scientists in dryland India were consistent with what economist Bandhudas Sen wrote about “the dependence of the high yielding varieties on irrigation” (1974, p. 25). He wrote further:

We are not suggesting that the high yielding varieties cannot be grown at all on unirrigated land. They can perhaps be grown in areas supposedly blessed with ‘assured rainfall’. But the area officially classified as under assured rainfall does

not mean much... nor is the distribution of rainfall in these areas over the season such as to meet the exacting demands of the new varieties. (Sen, 1974, pp. 25–26)

Sen, among others, found that the semi-dwarf varieties were *not* widely adapted across rainfed conditions, especially in dryland India.

From the beginning of the Borlaug's involvement in Indian wheat research, he had planned on concentrating on the irrigated areas and then addressing rainfed areas at a later time. This was consistent with other programs like the Ford Foundation's Package Programme, which focused on rural areas with assured irrigation or adequate rainfall. Saha has unveiled that despite India's focus on social equity, a Food Grains Enquiry Committee report in 1957 also favored concentrated efforts, but this was rejected by Nehru's Planning Commission (Saha, 2012). As Cullather described, both "Swaminathan and Borlaug openly advocated a 'betting on the strong' approach" of testing on large, irrigated plots (Cullather, 2010, p. 201).

Initially, RF scientists justified the lack of research for wheat varieties adapted to dryland conditions that prevail in central and southern India. Anderson wrote in 1966, "In the case of varieties for dry land production, less attention is being paid to this aspect until the shortage gap is filled. It is not being completely neglected but there is simply a preference for production of varieties for high yield under high fertility and irrigation" (Anderson, March 2, 1966, p. 2). Despite this focus on irrigated areas, the RF scientists had some hope that the varieties developed under irrigated conditions would still be adopted in rainfed areas, as their wide adaptation would suggest. But in 1970, the RF's Guy B. Baird wrote that, "the impact of the dwarf wheats has been limited almost entirely

to the irrigated areas; the improved technology has not yet materially affected the two-thirds of the wheat acreage under rain-fed (generally dry land) conditions” (Baird, February 4, 1970). In the late 1960s and early 1970s the RF scientists realized their international wheat, maize, and rice programs had only reached irrigated areas. Soon after, they expanded their national and international programs into dryland research (see Chapter 4).

Indian wheat scientists were not unaware of the problems of semi-dwarf wheat and their limited spread to irrigated areas. J. S. Kanwar, Deputy Directory General of ICAR’s soil and water division, began corresponding with various groups in the US that supported dryland research around 1967. Kanwar presented his views at various conferences, highlighting the lack of attention paid to rainfed and dryland wheat production, and the need for better irrigation systems. In 1973, Kanwar became the First Deputy Director General of the RF’s International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) in Hyderabad, which was formed in 1970 and focused on semi-arid crops.⁴⁴

Administrators in India and of the RF also recognized the socioeconomic inequities between irrigated and rainfed farmers caused by the green revolution. Sterling Wortman (RF’s Director for Agricultural Sciences) wrote to Guy B. Baird (director of the RF’s Indian Agricultural Program) in 1969:

The importance to India of her irrigated area is quite obvious. Do you have any information on the number of farmers who are largely dependent upon unirrigated

⁴⁴ ICRISAT’s mandate is for chickpea, pigeonpea, groundnut, pearl millet, sorghum, and other millets.

wheat as a means of livelihood? I think that we must increasingly consider in our projections not only total national output, but as well, the number of people needing benefit. [emphasis original] (Wortman, September 15, 1969)

Ralph W. Cummings, formerly of the RF in India and at USAID at that time, wrote to Bill Wright in India inquiring about a program to evaluate “the problems of rainfed lands and small farmers” (Cummings, June 2, 1969). RF staff in India also mentioned in 1969 that Indian policy-makers were interested in implementing something like Plan Puebla, the CIMMYT project for rainfed maize, in India, due to “growing concern about the lot of the small farmer” (Baird, December 10, 1969). According to W. David Hopper, “knowledge of the Puebla experiment is fairly widespread among senior Government officials here” (Hopper, December 2, 1969).

India’s Fourth Five Year Plan for 1969 to 1974 was the first national plan to mention the need for rainfed-specific research, though the fourth plan overall reinforced concentrating resources in the northwest. This, among other factors, led to the formation of a coordinated dryland research program, proposed and initiated in 1970. In January of 1970 the Government of India held an “All India seminar on dry land farming,” and at the 1970 ICAR Advisory Board meeting, A. B. Joshi emphasized “the importance of dry farming and the widening gap in production between the irrigated and unirrigated arid areas,” thus “a coordinated scheme on Dry Farming has been formulated” (Government of India, January, 1970; Joshi, 1970, p. 6). According to the diary of an RF officer, the All-India Coordinated Research Program on Dryland Agriculture was launched in coordination with the Government of Canada, and the initial research of the program

focused on screening varieties under rainfed conditions, soil conservation practices (i.e. tillage), intercropping, and dryland crops (Starnes, [Diary notes], September 25–28, 1972).

While my archival research in India did not focus on this topic (rainfed research in the 1970s), later documents help display the efficacy (or non-efficacy) of rainfed research in India in the 1970s. Rao stated in 1978 that one of the general problems facing wheat research and production was:

Raising wheat yields under unirrigated conditions... In the last several decades we must have attempted thousands of crosses to improve yield by genetic manipulations. The progress is very limited and the locals or selections from locals or varieties bred long time ago are still hard to be beaten... What should be our approach to improve wheat yields under these unirrigated conditions? (Rao, 1978, pp. 1.12–1.13)

That year, a National Drought Screening Nursery was initiated for wheat, perhaps inspired by CIMMYT's first Drought Screening Nursery in 1976 (CIMMYT, 1992). Rao, as we have seen in his other statements this chapter, raised some formidable questions. By 1993, however, scientists were still aware that “we have given little more than lip sympathy to rainfed wheat. Is anyone multiplying seeds of rainfed varieties?” (Swaminithan, 1993, p. 35). But as Saha pointed out, in the same volume, one wheat scientist stated “we have found it necessary to focus on the farmer in the irrigated, productive areas because they are more rewarding. We have a result-oriented program and that has to continue” (quoted in Saha, 2012, p. 172).

While India's agricultural researchers have given "lip sympathy" to developing rainfed varieties, many scholars point out the continuing marginalization of rainfed research and investments (Saha, 2012; Shankar, 2011; Witcombe, Virk, & Farrington, 1998). Some scholars blame the centralization of research funding under ICAR for the lack of research on rainfed problems. They argued that projects with wide national coverage override those like rainfed and dryland agriculture, which "tend to be region specific" (Easter, Bisaliah, Dunbar, 1989, p. 1203). This is an unfortunate consequence of the philosophy of centralization and wide adaptation as described in Chapter 3.

A changing landscape of fertilizers.

Fertilizer supply was a continuous problem for Indian wheat production in the 1970s, due to rising energy prices and the low levels of domestic production. Though fertilizer consumption tripled from 1964-5 to 1970-71, the average in 1970-71 was still only 12 kg/ha, and varied widely between states (Sen, 1974). E. E. Saari of the FF in India wrote to Anderson in 1972: "Fertilizer supplies (N and P) are short by 30%... There is a big push to increase the acreage but it will take about a million hectares to make up the fertilizer deficit" (Saari, September 28, 1972). Anderson himself noted that in 1973, fertilizer supplies were forty percent short for *rabi* (fall-planted) crops such as wheat (Anderson, November 1973).

In part because "farmers are not adequately fertilizing these wheats" in north and northeastern India and in part because of new experimental data, Indian scientists rolled back their recommended fertilizer levels from Borlaug's suggested 120 kg per hectare of nitrogen (Joshi, 1970, p. 1). A. B. Joshi wrote in 1970 that "experimental evidence now

clearly shows that the new wheat varieties, especially Kalyansona, should not be given less than 80 kg. of nitrogen per hectare” (Joshi, 1970, p. 1). In an undated report from the 1980s, scientists from the Wheat Project Directorate (WPD) sent Borlaug a report to justify their use of lower fertility levels in the coordinated trials:

In earlier years trials were laid out using 120 kg.N/ha which has been reduced to 100-80 kg/ha. as most of the crop in the country is sown with the use of very small doses of fertilizers. This has also affected overall performance of trials. It has been observed that the most successful varieties in India are not the ones with the highest yield potential under the best managed conditions but ones which perform well over a wide range of cultural environments specifically lower fertilizer levels and fewer irrigations. Some of the examples are WL 711, WH 147 and HD 2189. Even Sonalika is of the same type. This has necessitated [sic] development of genotypes which can do well at moderate fertility levels. (Wheat Project Directorate, n.d., p. 2)

Borlaug responded that “none of them are justifiable reasons for low yield in experimental plots where one is trying to measure the true genetic potential of new genotypes” (Borlaug, January 6, 1987). Borlaug’s wheat empire in India had begun to crumble, as the limits of wide adaptation to agronomic conditions were recognized in India. CIMMYT scientists now realized that the key to green revolution production was higher levels of fertilizer, and that “you can not produce high yields without N [nitrogen]. I think this is a dangerous opinion” (Anderson, 1973, p. 61), directly contradicting

Borlaug's claim that Lerma Rojo's best feature was "its wide adaptation—fertilized or unfertilized" ([Oral history of Norman Borlaug], 1967, p. 196).

Changing concepts in plant adaptation and evaluation in Indian wheat science. If one could very coarsely summarize Chapter 3, it would be this: a shift in focus from combined genotype by environment (GxE) interactions to a focus on a stable genotype under favorable conditions. We saw a shift from the 1960's view that:

Yield in wheat, as in other crops, is the ultimate expression of the interactions between the genetic constitution of the crop variety and the environmental factors in which it is grown. The yield is, therefore, at its best when the plants of a particular variety are in harmony with the environment... (Sikka & Jain, 1960, p. 154)

...to the view that "wide adaptation is genetically controlled and that varieties do not require to be bred for the particular conditions. This indicates that minimizing G x E interaction is important for the choice of environment" (Roy & Murty, 1970, p. 516). The latter view was directly adapted from Borlaug and Krull's arguments for widely adapted wheat. But later in the 1970s, Indian wheat scientists regained a more nuanced view of GxE interactions and how they contribute to yield. This included a shift to thinking about yield not as a genetic property that was transitive across environments. This shift led to some focus on specific adaptation of wheat in the later 1970s.

S. P. Kohli, soon before his departure from IARI, wrote a scathing review of Indian wheat breeding in 1969. He asked, "actually what is 'yield' and... to what extent might this be regarded as an inherited character?" (Kohli, 1969, p. 25), in contrast to

prevailing methods that examined yield “only to be a direct ‘gene-controlled plant character’” (Kohli, 1969, p. 28). He referenced scientists’ study of genetic combining ability in wheat, which Kohli argued should only be applied to hybrid crops, and which lacked nuance in discovering what traits were actually responsible for increases in yield. Kohli stressed in his paper the importance of environment on yield, which he felt had been overlooked by Indian wheat breeders. He used the example of the tall, Indian variety C306 versus two semi-dwarf varieties to show that at low fertility environments, C306 had a higher yield than the semi-dwarfs. This seemingly obvious example, using data from the coordinated trials, was somehow still counter to the mainstream thought of Indian wheat breeders.

Kohli’s critical review may have influenced Indian scientists’ shift to more nuanced views on adaptation and environment. Another component of the shift appears to be scientists’ use of more complex statistical analyses of yield in the context of different environments, such as the Finlay and Wilkinson’s influential model that allowed scientists to quantify adaptation, also known as a stability analysis because it measured the phenotypic stability of traits (such as yield) under different environments. In other words, the Finlay-Wilkinson model could measure GxE interactions.

Keith Finlay himself influenced Indian wheat scientists in the later part of the 1960s, visiting India in 1966 to lecture on adaptation (Anderson, October 24, 1966). Then in 1968 he helped organize the Third International Wheat Genetics Symposium, along with M. S. Swaminathan. At least two influential Indian wheat scientists, J. P. Srivastava, from Pantnagar, and H. K. Jain, of IARI, met with Finlay during their conference travel

in Australia, where Finlay gave a lecture on “The significance of adaptation in wheat breeding” (Finlay, April 15, 1968).

Interestingly, while RF and Indian wheat scientists used Finlay’s model to support wide adaptation, they did not actually often apply the model to data in the 1960s. In fact when used, the model often indicated that so-called widely adapted varieties were adapted to high fertility conditions. Instead, scientists used the model to justify the structure of the All India Coordinated Wheat Improvement Program, which promoted varieties with high average performance over locations, rather than high performance under a specific set of conditions. It is not clear why applications of the Finlay-Wilkinson model to wheat were so scant in the 1960s. Other crop scientists applied the stability analysis to millet (Athwal & Singh, 1966), gram (lentil) (Chandra, 1968), sorghum (Rao & Harinarayana, 1969), and rice (Ram, Jain, & Murty, 1970). All of these scientists worked at IARI’s division of genetics in Delhi, or at Punjab Agricultural University in Ludhiana and Hissar, which were the major research hubs in the northwest (excluding Pantnagar). Romagosa and Fox (1993) reflect that the analysis required transfer of punch cards between institutions, which in the 1960s, limited which research centers could perform the analysis.

Then the 1970s brought a renaissance of studies on wheat stability, producing nearly 200 papers by 1980 (Bhullar, Gill, & Khehre, 1977; Das & Jain, 1971; Gupta, Virk, & Satija, 1980; Luthra & Singh, 1974; Mohan, Das, & Jain, 1974; Verma, Chahal, & Murty, 1978). Wheat scientists applied the stability analysis to intra- and interplant variation in wheat’s characteristics, to see how that variation was inherited and how it

expressed in different environments. Kohli criticized these studies for being academic rather than practical, chastising “mathematicians and biometrical geneticists, who are after simple mathematical ‘models’” rather than contributing to improved breeding practices or better varieties (1969, p. 28).

After the stability analysis entered the toolkit of Indian wheat scientists, scientists began to reconsider or recognize the limits of wide adaptation starting in the later 1970s and into the 1980s. S. Tara Mohan, P. K. Das, and H. K. Jain in 1974 showed the contingency of earlier scientific assumptions. They wrote,

Kalyan Sona the best known variety from the adaptation point of view, is characterised by a relatively large G x E interaction. This finding suggests that capacity to generate variability under diverse environments may be an important attribute of a well adapted variety. The finding calls for a review of our earlier concepts on adaptation. (Mohan, Das, & Jain, 1974, p. 1124)

Similarly, Bhullar, Gill, and Khehre 1977 studied the heritability of stability, and wrote extensively about Kalyansona:

The general experience is that this variety has wide adaptability, as it is grown throughout the world, particularly in South Asia, the Middle East and North Africa... At the same time it is known to be highly responsive to high fertility conditions... The wide adaptability of this variety, therefore, may not be due to its inherently greater stability but due to its distinctly greater yield potential compared with previous commercial varieties. (Bhullar, Gill, & Khehra, 1977, p. 44)

If the reader has accepted the arguments I presented in Chapter 3, these results are not surprising. In 1978, Rao, director of the coordinated trials, presented data indicating that wheat had different responses to environments, finding that “the two environments bring about tremendous changes in the yield attributing characters... The studies revealed that *selection parameters have to be different for different environments*” (emphasis added) (Rao, 1978, p. 1.10). These findings challenged the supposed inherent yield advantage of widely adapted varieties. It is astounding that in 1978, it was considered a radical proposal that wheat yield was affected by environment, but this demonstrates the pervasiveness of Borlaug’s argument for wide adaptation.

Not until the later 1980s did scientists to seriously again consider local or specific adaptation as a desirable research pathway. This was consistent with broader trends in the agricultural research community in the 1980s, which was pressured to address the problems of marginal farmers through locally adapted research (Douthwaite, Keatinge, & Park, 2001). In a book celebrating the 25th year of India’s coordinated wheat program, then-coordinator J. P. Tandon and former coordinator Rao wrote, “till recently the workshops considered proposals for the zonal releases only. But now it has been decided to identify varieties with more specific adaptability also for a state or parts of it” (1986, p. 11). On the next page, however, they revealed their still-skeptical stance towards decentralizing research among India’s states, stating:

Although very elaborate and extensive system of thorough testing and identification of suitable varieties has been developed under the AICWIP [All India Coordinated Wheat Improvement Program], yet some of the states continue

to organise regional based programmes. The utility of such programmes has been questioned but *agriculture being a state subject, it can not be banned*. Moreover, possibilities of developing varieties with very specific adaptability cannot be denied and some times there may be a need to develop varieties for specified regional problems. However, it is interesting to note that example of state released varieties having become popular any where are very rare. [emphasis added] (Tandon & Rao, 1986, p. 12)

This reveals that although Tandon and Rao seemed to appreciate specific adaptation as a way to address “specified regional problems,” they did not view it as particularly useful. They also dismissively referred to the state mandates for agriculture as simply “it can not be banned,” implying that agricultural research was better coordinated on a national or zonal level.

In the next chapter of the 25th anniversary book, R. K. Agrawal lamented the lack of new widely adapted varieties, finding that based on the “revival and again discontinuity of National Trials, it seems that the Indian wheat breeding programme has either not succeeded to develop varieties of wider adaptability like Kalyansona and Sonalika for irrigated condition and like C306 for rainfed cultivation” (Agrawal, 1986, p. 60–61).⁴⁵ But into the 1980s and 90s, Borlaug was still very much convinced that trials should be conducted at high fertility and aimed at wide adaptation. Borlaug wrote to Tandon in 1984 that,

The fact that some varieties such as Sonalika, Kalyansona, HD2009 and WL711 are grown commercially over wide areas, indicates that it is possible to breed

⁴⁵ The National Trials were created to test one set of varieties across the entire country.

varieties with broad adaptation... This would seem to me to indicate that there are altogether too many zones that are used in the varietal release programs and that these could be greatly simplified by fusing a number of smaller ecological zones. (Borlaug, October 17, 1984).

By 1984, Indian wheat scientists operated on the basis of nine agro-climatic zones, increased from the initial five zones. Thus while Indian scientists recognized the limits of wide adaptation, Borlaug continued to push wide adaptation and the consolidation of agro-climatic zones.

In 1993, Borlaug feared that international research centers were moving away from wide adaptation (Swaminathan, 1993). While it was perhaps on the decline in International Rice Research Institute (IRRI) and other centers, in the 1990s CIMMYT reaffirmed its commitment to wide adaptation. Within India, the initial excitement over wide adaptation faded and troubling questions emerged. Although scientists held multiple and nuanced views over what type of adaptation was best for India, wide adaptation remained codified in the wheat research testing and system.

Part 2: CIMMYT in the Middle East

In 1972, the Government of India ended its collaboration with USAID and several US universities over political tensions with the US (Busch, 1988). The RF also began phasing out the Indian Agricultural Program in the mid-1970s, believing that the Indian research system was sufficiently developed and no longer required outside intervention. The RF ended their involvement in the wheat program in 1970, when Anderson was transferred to Mexico to work with CIMMYT. This was not an adversarial break up, and

CIMMYT and the RF would continue to collaborate with Indian researchers through the international wheat program and the newly-formed research center, the International Crops Research Institute for the Semi-Arid-Tropics (ICRISAT), in Hyderabad (RF Indian Agricultural Program, June 1, 1972). Ralph Cummings, former director of the RF's Indian Agriculture Program, soon became director of ICRISAT. The RF distributed some of their field staff from India to their four international research centers. Other staff were sent directly to Turkey, for the RF's newest country-wide wheat program (RF Indian Agricultural Program, December 1, 1973). The RF program in India officially terminated in 1976.

After the success of the wheat revolution in India, both the RF and Ford Foundation (FF) planned to intensify their research in the Middle East (Cullather, 2010). In the late 1960s, CIMMYT's international reach expanded, in 1968 to Pakistan, UAR, Tunisia, Morocco, and Argentina, and in 1969 to Turkey (Wortman, August 11, 1970).

We have already seen in Chapter 2 that the RF in Mexico had been involved in wheat research in the Middle (Near) East starting around 1960. The RF, working with the Food and Agriculture Organization (FAO), brought scientists from the Middle East to Mexico for training, and working with local and FAO scientists, also established several wheat nurseries around the Middle East. CIMMYT had hoped in 1963 that "the wheat research work in India might serve as the eastern anchor for cooperative activities through the Near East countries" (Moseman, February 25, 1963, p. 71). In the mid-1960s, both the RF and FF considered starting a regional agricultural research center in the Middle East. The RF sent scientists Elvin Stakman and John Gibler to the Middle East in

the fall of 1966 to prepare a proposal on a regional wheat program there (Remenyi, 1978). According to a later evaluation by the FF, the RF was “posed to initiate a wheat research center, involving a 10-year commitment of around \$4 million” in Lebanon, with RF wheat breeder John Gibler as its director (Remenyi, 1978, p. 21). Then war broke out in Lebanon in June 1967 and the RF withdrew completely. The FF already had staff in Beirut, so they established a research center called the Arid Lands Agricultural Development (ALAD) Program. The RF lacked agricultural field staff in the Middle East until 1970 in Turkey, discussed later this chapter.

While the RF and FF’s decisions to work in India in the 1960s were politically motivated by the Cold War, their expansion into the Middle East in the late 1960s appears less ideologically-driven or related to Cold War politics. Fueled by their success in raising wheat production in India, the foundations hoped to extend the green revolution to wheat-producing countries in the Middle East. There are certainly shades of neo-imperialism in these foundations’ drive to expand to throughout the Middle East. And given the political instability throughout the region, foundation scientists and administrators may have already internalized the Population-National Security Theory to an extent that the reasons for their expansion was more implicit than the previous decade (Perkins, 1997). In other words, these foundations may have implicitly assumed that higher food production would lead to greater political stability.

Despite wanting to simply extend the green revolution into the Middle East, CIMMYT and the foundations came upon new challenges that led to shifts in their research programs and to create new institutional arrangements. Turkey, for example,

was a major wheat producer that was beginning to adopt semi-dwarf wheats, but farmers in the low-yielding, unirrigated plains of Turkey grew winter wheat, of which semi-dwarf versions were not available. I will explore the international shift to rainfed and dryland research in the remainder of this chapter.

The Arid Lands Agricultural Development Program. The FF started the Arid Lands Agricultural Development (ALAD) Program in Lebanon in 1968. Although the FF founded ALAD, it was also important to the trajectory of CIMMYT and RF wheat programs. First, ALAD's infrastructure and staff directly contributed to the formation of the International Center for Agricultural Research in the Dry Areas (ICARDA) in 1977, which collaborated with CIMMYT on wheat research. Second, Robert Havener, once-director of ALAD, later became the third director general of CIMMYT from 1978 to 1985. Finally, Eugene Saari, who worked with the FF and RF in India, joined ALAD and CIMMYT staff from 1973 to 1975 and then remained affiliated with CIMMYT until his retirement (Remenyi, 1978). Because of the lack of published secondary sources on this subject, much of this section draws on one 1978 report, "ALAD: An evaluation" by Joseph Remenyi, an economist with the FF, which I accessed from the Ford Foundation archives, located at the Rockefeller Archive Center.

The FF's Forrest F. Hill (then-vice president for overseas development) had suggested a regional agricultural center in the Middle East in the early 1960s, and David E. Bell, who succeeded him in 1966, continued on that path. Important to the FF was to not create another CIMMYT or IRRI, but rather to follow the FF's outreach/extension and community development-based model for their agricultural programs (Remenyi,

1978). As previously mentioned, ALAD was founded after war broke out in Lebanon in 1967. In the immediate aftermath of the war, Hugh Walker, an FF representative located in Beirut, lobbied for the creation of ALAD. ALAD was actually started in Lebanon before it was approved in the FF's main office in New York, but officially, the FF initiated ALAD in February 1968. Walker was ALAD's first director, and then Havener took over in 1971. ALAD continued until December 1976, when it had been decided that ICARDA would succeed ALAD.

ALAD focused on agriculture issues of the Middle East and North Africa, especially the countries from "Afghanistan to Morocco and Turkey to Sudan and Ethiopia" (Ford Foundation, December 1973; Remenyi, 1978, p. 1). Noting that "the national agricultural institutions in the region lacked the capability or interest in testing, evaluating and modifying them [CIMMYT and IRRI varieties] to fit local conditions," ALAD at first focused on extending "the benefits of the green revolution to the Middle East through adaptive research" (Ford Foundation, December 1973, p. 2; Remenyi, 1978, p. 2). One of ALAD's early goals was to "lead with wheat" based on CIMMYT's successful international wheat programs (Remenyi, 1978, p. 61). ALAD initially focused on irrigated crops, especially irrigated wheat, where training and materials were easily available, but later expanded to coarse grains such as sorghum and millet. According to Remenyi, a review of the program in 1971 brought the "realization that the region demanded location specific research" for disease, rainfed agriculture, water management, etc. (Remenyi, 1978, pp. 2-3). This focus on location specific research would carry on to ICARDA. ALAD also preceded ICARDA with its focus on what is now called

participatory agricultural research, a way of involving farmers in agricultural research (Ceccarelli & Grando, 2002). Remenyi wrote in 1978:

In the nursery programs especially ALAD was innovatory [sic]. Responsibility for selection of well adapted high yielding varieties was not kept at the ARI [Agricultural Research Institute in Lebanon] for ALAD staff. They sent their cooperators seeds of early generation materials that would segregate when grown out. The cooperators would then need to select out the best varieties, making them real partners in the regional breeding and improvement program. (p. 13)

This is an early example of what is called participatory plant breeding, which became more popular in the 1980s.

ALAD and RF/CIMMYT in the Middle East. As mentioned, ALAD aimed to “lead with wheat” and according to Remenyi, their wheat program was influenced by CIMMYT and IRRI’s model of research (1978). Remenyi wrote that, “ALAD functioned essentially as a relay station for CIMMYT and other ‘green-revolution’ technology and training centers” (Remenyi, 1978, p. 61). This is consistent with what scholars now call the “transfer of technology” or “linear model” of agricultural development—where technologies are assumed to be easily transferrable across sociocultural contexts. ALAD also complemented the work of CIMMYT in Mexico and Turkey: ALAD focused on spring wheats while RF’s Turkey program focused on the separate class of winter wheats.

The RF and CIMMYT became more directly involved in ALAD a few years after it was established. Borlaug was appointed as an advisor to ALAD, and in 1971 the RF provided Leland R. House, who had worked with the coordinated maize program in

India, as staff for ALAD. Saari, also coming from a position in India, joined ALAD in 1973 under a complicated directive: he was considered staff of CIMMYT, ALAD, and FF all at once.

Saari had worked with the FF and RF in India and helped establish a regional disease screening nursery, which would soon be extended to the Middle East. In 1969 the FF, through ALAD, started establishing wheat nurseries in the Middle East (Anderson, 1973). But realizing the overlap between their programs, FF and RF, FAO, and CIMMYT consultants establish a set of unified wheat nurseries for the region in 1971 (*Points discussed between the wheat program staff, May 31, 1971*). One of these nurseries was the Regional Disease and Insect Screening Nursery, which grew out of the Indian coordinated wheat program due to fear of disease spreading through green revolution wheat varieties (Remenyi, 1978). Collaboration between the FF, RF, and CIMMYT continued throughout the 1970s in the Middle East with the establishment of ICARDA, which was first proposed in 1973. Part 3 of this chapter will address ICARDA in further detail.

CIMMYT's wheat program in Turkey. In 1969 CIMMYT—in collaboration with Oregon State University (OSU), the RF, and USAID—started a bilateral agricultural program in Turkey, aimed at training Turkish agricultural scientists and increasing Turkish wheat production (CIMMYT Board of Directors, September 25-26, 1969). CIMMYT's board of directors found that “Turkey itself represents an important wheat production area of the world” and “the program could serve as a CIMMYT ‘outpost’ for... a larger area” (CIMMYT Board of Directors, September 25-26, 1969). The Turkish

wheat program was funded by both the RF and USAID, but CIMMYT and OSU provided much of the staff and the coordination of programs.

According to a USAID report in 1969, Turkish wheat production had improved from 1958 to 1967 due to improved agronomic practices, but using only local varieties. Then in 1966, a group of “progressive farmers” purchased some Sonora 64 seeds from Mexico and by 1968 two percent of Turkey’s wheat acreage was planted under new varieties (Kronstad, 1969, p. 32). The coastal areas of Turkey, which were well irrigated, rapidly adopted semi-dwarf wheat varieties in the later 1960s and early 1970s.

Turkish wheat production faced problems of overall low yields, plant diseases such as rusts and septoria (a fungal infection), and that wheat grown in the majority of the country was rainfed winter wheat. This was important because the wheat varieties produced by CIMMYT thus far were spring varieties of wheat. Winter wheats differ from spring wheats in that they need a cold period before they develop. There was also a significant disparity between the coastal wheat-region of Turkey—which had a moderate climate, was mostly irrigated, and grew spring wheat—and the Anatolian Plateau, which contained the majority of wheat production. The Anatolian Plateau was mostly rainfed, and due to its different climate, could only grow winter wheat. Because “most of the winter wheats introduced into Turkey are not well adapted,” CIMMYT initiated an international winter wheat screening nursery in Turkey in 1972 (Klatt, May 25-June 17, 1971, p. 10; Klatt, September 21, 1972). Engaging in Turkish wheat production, CIMMYT pushed beyond the agro-climatic constraints of its program on irrigated, spring wheat.

In 1970 the RF chose Bill C. Wright as the project leader for Turkey, transferring him from India. CIMMYT then brought on Arthur Klatt in 1971 as a wheat breeder, and J. M. Prescott as a pathologist. Prescott was stationed in India before coming to Turkey, and Klatt with CIMMYT in Mexico (Wright, October 5, 1971). CIMMYT's Turkish wheat program shared similarities with the Indian wheat program beyond its staff: the initial philosophy and methods of the program were based on coordination of research and creation of uniform conditions (Cummings, Rodenhiser, & Gibler, 1968). The program also aimed, according to Klatt, to "test our materials under varying environmental conditions... and also identify lines with broad adaptation" (Klatt, [Letter to Zoltan Barabas], n.d.). By 1974 Floyd Bolton (an agronomist appointed by USAID), Michael Lindstrom (a soil scientist), Charles K. Mann (an economist), and Dwight Finrock (appointed to help develop the extension stations) had also joined the RF/CIMMYT staff in Turkey.

The RF/CIMMYT program in Turkey was a major site of wheat research in the early 1970s, and has received relatively little attention from historians. The project there coincided with criticisms of the green revolution, thus researchers were under pressure to prove themselves against these critics. One of these criticisms was that green revolution varieties had less stable yields over time than did traditional varieties, due to drought susceptibility of the new varieties or other factors (Michaels, 1982). CIMMYT countered this by making "yield stability" one of their core goals in the 1970s. Yield stability meant stability over time, not just space, and was touted as a complementary characteristic with wide adaptation. When it first became a topic of interest, it was implied to be a result of

wide adaptation. For example Anderson stated to the US National Academy of Sciences in 1972, “Stability of yield, therefore, is of paramount importance. If we develop varieties with wide adaptation which are able to do well in widely separated geographic areas, the effect of variations in climate from year to year at any one location can be expected to have considerably less effect on yield” (Anderson, October 11, 1972, p. 17). This conflation of time and space may have been due to Keith Finlay and others using yield stability and wide adaptation interchangeably (Finlay, 1963). But in the rest of my dissertation, yield stability will mean temporal yield stability, which is the common usage at present.

Klatt, the CIMMYT wheat breeder in Turkey, spoke at a 1973 CIMMYT wheat symposium about yield stability. He opened, “Since the science of varietal improvement was initiated, we have worked to increase maximum yield potential. Today, I would like to discuss this as well as a new topic, stabilizing minimum yield levels” (Klatt, 1973, p. 104). Working with winter wheat varieties in Turkey, Klatt noted that it was more difficult to obtain a stable, minimum yield there. He explained:

The main reason is the climatic instability of the winter wheat growing regions. Most winter wheat areas are characterized by large annual fluctuations in temperatures and precipitation and also large monthly fluctuations. This creates a need for yield stability as well as a need for increased yield potential. (Klatt, 1973, pp. 104–105)

In contrast, CIMMYT’s economist in Turkey, Mann, wrote to a statistician at the University of Readings, “since for a long time people have talked of greater yield

stability with improved technology, it seemed important to distinguish between higher average yields and lower year-to-year variation” (Mann, September 8, 1977). But Mann, along with other critics, pondered whether “because of the interactions between the package and moisture, *annual variation may actually become greater rather than smaller*” [emphasis added] (Mann, September 8, 1977). This remained a major point of contention throughout the 1970s and 1980s.

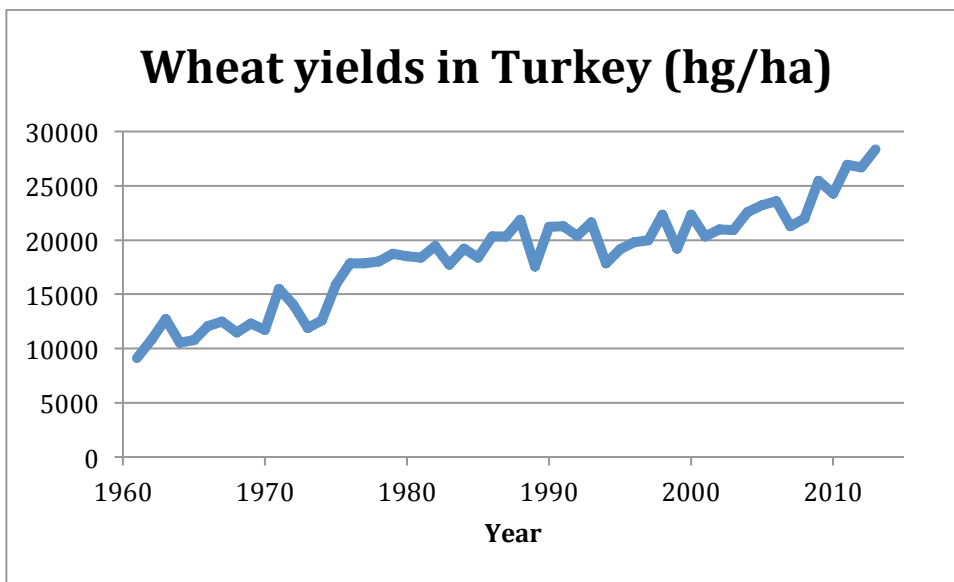


Figure 11. Wheat yields in Turkey from 1961 to 2012. Data retrieved from FAOSTAT.

Ultimately, Turkish wheat production increased due to more efficient (and not necessarily intensive agronomic practices, and not improvements in germplasm (i.e. so-called genetic gains) (Figure 11) (Villareal, & Klatt, 1985). In fact as of 2001, no semi-dwarf variety of winter wheat has been released for Turkey’s rainfed areas (Braun, Zencirci, & Altay, 2001). Because of the limited rainfall and climatic variability of central Turkey, CIMMYT scientists decided to focus on improvements in soil moisture conservation rather than promoting new varieties, which did not perform substantially

better than local varieties there. Two scientists at the later-formed ICARDA, E. E. Saari and J. P. Srivastava (who both worked in the Indian wheat program), explained:

There are two distinct but interrelated factors for improving and stabilizing the production of winter cereals. One factor would be to tailor the desired genotypes to exploit a given set of agroclimatic parameters and the second would be to execute agronomic techniques and methods to maximize the production of the existing crop cultivars presently available. (Saari & Srivastava, 1977, p. 3)

RF/CIMMYT scientists in Turkey chose the latter technique because there was no widely adapted winter wheat. Winter wheat, by its nature, winter wheat has certain agro-climatic requirements (cold exposure) that spring wheat does not. This is consistent with the later finding that “between areas where photoperiod sensitive or photoperiod insensitive varieties have a clear adaptive significance, the annual variations in climate make it extremely difficult for breeders to produce varieties with good adaptability to changing environmental conditions” (Worland et al., 1998, p. 385). This supports my argument that wide adaptation was only a result of daylight insensitivity and fertilizer responsiveness. Turkey significantly increased its fertilizer consumption starting in 1974, as seen in *Figure 12*.

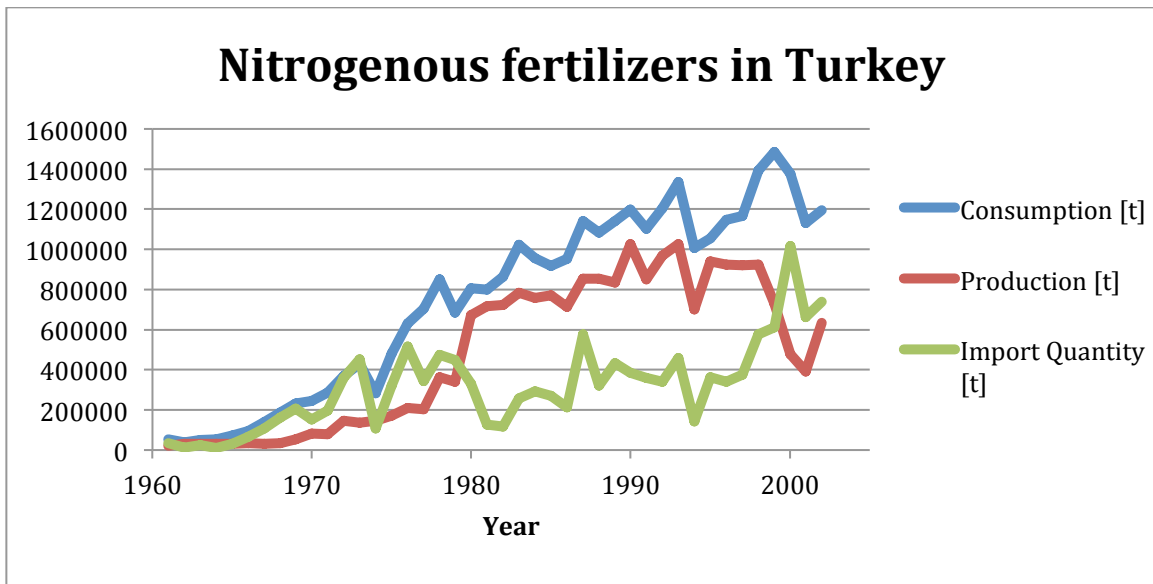


Figure 12. Consumption, production, and imports of nitrogenous fertilizers in Turkey from 1961 to 2002. Data retrieved from FAOSTAT.

In 1976 the RF/CIMMYT Turkey program started downsizing staff because Turkish scientists now had the necessary experience to start taking over. By 1976 CIMMYT also decided to phase out countrywide programs and focus on providing regional assistance, working with the newly formed ICARDA. The CIMMYT program in Turkey officially ended in 1982, and in 1986 both CIMMYT and the Government of Turkey joined the International Winter Wheat Improvement Program, a collaboration that ICARDA later joined (Braun, Zencirci, & Altay, 2001).

Part 3: CIMMYT and Research for Rainfed and Marginal Lands

By 1967, before the green revolution was even coined, CIMMYT and IRRI administrators and researchers began to realize that in order to continue to reach farmers in tropical and sub-tropical areas, they needed to expand their research goals to address the needs of small farmers and marginal agriculture. Up until the late 1960s, most of

CIMMYT and IRRI's research programs focused on crops under high fertility and irrigated conditions. Then in the late 1960s and early 1970s CIMMYT and IRRI started to focus on rainfed and dryland cultivation, including a shift from focusing on breeding to a focus on agronomy, as seen in the Turkey program. These organizations were responding to pressure from RF and FF administrators as well as following a desire to expand their own programs.

In the 1970s, international organizations also shifted their focus from improving aggregate production to improving rural people's livelihoods. While social commentators criticized the uneven spread of green revolution benefits and technologies, administrators from the RF and USAID began asking critical questions about how or whether research had improved the livelihoods of marginal farmers. These factors were a reaction against criticisms that green revolution technologies had led to inequities in rural areas of less developed countries.

Another factor behind the shifting goals in international agricultural research was the changing institutional context of international aid. In the 1970s, foreign assistance programs shifted from bilateral to multilateral, led by the World Bank's new president, Robert McNamara (Lancaster, 2008). The United Nations Development Program, founded in 1965, along with the FAO and World Bank began focusing on poverty reduction and "a new ethos focusing on equity" as their primary goals (Latham, 2011, p. 169). McNamara, becoming head of the World Bank in 1968, "introduced into the Bank the 'people's basic needs' approach, and shifted investments from a focus on the physical to the human part," as well as increasing overall investments from the Bank (Jefferson,

2010, p. 459). According to historian Michael Latham, McNamara had supported the modernization approach during his tenure as US Secretary of State, but he realized by the later 1960s that modernization and industrialization had not less to more political stability, even in the US (Latham, 2011). He believed that poverty reduction, more than infrastructure development and industrialization, would increase global stability

Then in 1969 McNamara proposed a new institutional arrangement to support long-term agricultural research. He felt that the global need for agricultural research and development had outstripped the capacity of US foundations (Ozgediz, 2012). This new institution became the Consultative Group on International Agricultural Research (CGIAR) in 1971, a network that brought together CIMMYT, IRRI, as well as two other RF and FF supported centers, the International Institute of Tropical Agriculture and International Center for Tropical Agriculture. The CGIAR was initially supported primarily by the World Bank, the RF and FF, and individual member countries (Pistorius, 1997).

The World Bank started financially supporting CIMMYT in 1972 (CIMMYT, 1972). But CIMMYT's primary donor organizations up to 1972—the RF and FF—were already giving attention to rural equity and rainfed research. As described earlier this chapter, the RF's Wortman argued that “we must increasingly consider in our projections not only total national output, but as well, the number of people needing benefit” (Wortman, September 15, 1969), and Cummings wondered about “the problems of rainfed lands and small farmers” (Cummings, June 2, 1969). Lowell S. Hardin, from the FF and on CIMMYT's Board of Directors in 1968, asked in 1969:

Programwise, *is it time to give greater research attention to photosynthetic efficiency of wheats under dry land and natural rainfall conditions?* If the plant type is really superior, then shouldn't this superiority be engineered into varieties which are also superior under stress?" [emphasis added] (Hardin, September 17, 1969)

This technical-sounding question reflected a rising socio-political and ethical concern for marginal farmers in rainfed areas. Hardin's question also directly challenges Borlaug's widely adapted "plant type," which despite Borlaug's claims, was not adapting to rainfed areas.

CIMMYT scientists also broadened their research program to rainfed agriculture in order to work in the Middle East. Borlaug wrote to CIMMYT's director E. J. Wellhausen in late 1967, "We now talk about expanding the CIMMYT wheat work to the Anatolian Plateau, Iranian Plateau and the Mediterranean belt of North Africa and the Near East. To be successful there we must develop experts in wheat dry land farming techniques" (Borlaug, November 10, 1967). After seven years of disregarding the needs of rainfed areas in the Middle East, Borlaug now saw the need to expand to those areas. This also coincided with a shift in other international agencies that began funding rainfed research programs in the 1970s.

Borlaug was not alone in realizing the limits of research focused on favorable environments. The authors of the report presented to CIMMYT in 1971 noted:

Until the beginning of 1968, CIMMYT's wheat program concentrated almost exclusively on irrigated wheat. With the exception of international yield nurseries

which were grown also under rainfed conditions, the varieties have been bred and produced primarily for the high producing, fertilizer-responsive irrigated conditions, which existed in a number of the Near East and Asian countries. In 1968, however, several agricultural assistance programs were initiated within the Mediterranean region in which there was a large predominance of rainfed wheat producing areas. (*A proposal for regional coordination of the Mediterranean and near east cereal programs*, ca. 1971, p. 56)

Edwin Wellhausen, the director of CIMMYT up to his retirement in 1972, also recognized the limits of existing programs, stating in 1973:

As we continue to push production in the more favored zones, we must also make a special effort to speed up the use of modern technology in the more marginal, but economically viable regions. This will mean further strengthening research activities, the development of more elastic varieties, more precisely suited agronomic practices and, above all, new delivery systems and incentives if we are to get the technology applied. We must remember that these delivery systems are going to be location specific and vary from region to region. What works in one place may not work in another. (Anderson, 1973, p. 23)

By 1972, CIMMYT had shifted their main objective to include assisting the “development of food grain improvement programs... which will benefit the largest possible number of farmers, especially in developing countries” (CIMMYT, December 1972). While still concerned with the “optimum production environment,” a CIMMYT report stated that this meant only,

To eliminate, insofar as possible, variations in seedbed and moisture, and thus permit each experimental seed line to express its full production potential. This does not mean that CIMMYT is trying to benefit the irrigated, mechanized farmer, but only that CIMMYT provides a dependable first step for breeding and experiments. (CIMMYT, December 1972)

During this time, CIMMYT became involved in two programs that were representative of these broader trends in agricultural research: Plan Puebla in Mexico and ICARDA in the Middle East.

Plan Puebla—Motivations for it and Lessons from it. As Chapter 1 hinted, CIMMYT's Plan Puebla (sometimes known as the Puebla Project) was the first modern agricultural program in Mexico aimed specifically to help rainfed farmers (Arce, 1987). Proposed by Wellhausen in 1966, Plan Puebla aimed to increase maize yields for farmers in the Puebla region, who did not have access to irrigation (Wellhausen, October 14, 1966). This would demonstrate, counter to critics, that irrigation was not necessary for farmers to increase their crop production.

A 1969 CIMMYT report described Plan Puebla as “CIMMYT's answer to the question: how can the large traditional-agriculture sector be transformed into modern farming?” and that if it succeeded, scientists could “bring the green revolution to thousands who have heard of it or seen its benefits— in the fields of the large farmer and on state-owned lands” (Bruner, 1969, p. 20, 25). Thus, Plan Puebla was a case study for whether the benefits of the green revolution could be extended to marginal lands. This is obvious in the follow up reports on Plan Puebla, titled, “Plan Puebla: Transferable and

Generalizable?” and “The lessons of Puebla and the potential of the Puebla approach” (Hertford, August 17, 1970; Myren, 1972).

The imperative of Plan Puebla was obvious in the correspondence between Forrest F. Hill of the FF and Borlaug in 1967. Hill wrote:

Sometime I want to talk to you about the problems of production on Ejidal lands. Del Myren recently sent me a paper for comments in which he talks about the need for incorporating Mexico's low-income farmers into her modern agricultural economy. If this cannot or is not being done in an environment as favorable as the Yaqui Valley [where CIMMYT was located], it certainly is not going to be done under less favorable circumstances. (Hill, June 29, 1967)

In this, Hill implied that if Plan Puebla could not succeed in this area of Mexico that had adequate rainfall, then it was not likely to succeed in in less ideal agro-climatic conditions.

CIMMYT researchers worked with the Agricultural University of Chapingo and smallholder maize farmers in the state of Puebla to enact the plan in 1967 (Redclift, 1983). While rainfed, Puebla faced reliable rainfall and was considered relatively favorable for maize production. Researchers promoted the package of Green Revolution technology, including fertilizers, improved seed, and agronomic management. Plan Puebla aimed to dramatically increase maize yields “by adapting existing technologies to ecologically specific growing conditions” (Redclift, 1983, p. 552). Ultimately, Plan Puebla did increase the yields of farmers in the Puebla region in a short period of time, but with two interesting results: farmers very often did not adopt the “complete package”

of recommended practices, and farmers found that local seed was more responsive to fertilizer than CIMMYT varieties (Redclift, 1983). CIMMYT's economic analysis of Plan Puebla also found that farmer net income increased due to increased yields, and farmers who had access to credit especially benefitted (CIMMYT, 1974).

But by 1973 CIMMYT discontinued the Plan Puebla, to the disappointment of RF-affiliated advisors (Wortman, October 26, 1971). A 1972 report by Delbert T. Myren, formerly of the RF and then at USAID, outlined some of Plan Puebla's constraints. One of the main findings of Plan Puebla was that CIMMYT's maize varieties were not competitive with local varieties, which as Wellhausen stated in Chapter 1, were selected over time by Mexican maize farmers for their adaptability to stress conditions. Myren wrote that "in contrast to the experience with the 'green revolution' in wheat," for maize, "identifying improved germ plasm for rainfed production is much more complex than it is for irrigated conditions. It also appears that up to now most breeding programs have focused on selecting material for optimum moisture conditions" (Myren, 1972, p. 10). He indicated that the high yield potential of CIMMYT maize varieties was the result of a "good fit between the characteristics of that variety and the particular environment" rather than an inherent adaptability (Myren, 1972, p. 10).

Myren concluded that contrary to CIMMYT's program of centralized and shuttle breeding, "in the Puebla area the variability in soil and climate made it impossible to select one central spot that would give results applicable to the whole area" (Myren, 1972, p. 12). This led CIMMYT to decide, "that this project is too far afield from its main

interest in plant breeding” (Myren, 1972, p. 14.) Plan Puebla was subsequently adopted by the Mexican government as a program for rainfed agriculture (Redclift, 1983).

Plan Puebla was also CIMMYT’s first experience in social and economic research. Alberto Arce attributed Plan Puebla to paradigmatic shifts in agronomy and the influence of rural sociology on agricultural research (Arce, 1987). One of the innovations of Plan Puebla was that it brought an agricultural economist to CIMMYT’s team. Don Winkelmann was hired as CIMMYT’s first economist and social scientist, and provided feedback on Plan Puebla. His review focused on not its success or failure, but on the aspects of the program that could be transferred to other cases, as well as the “spill-over effects” of site-specific research, a conversation that would carry on into the 2000s (CGIAR Science Council, 2006; Winkelmann, November 13, 1970).

Dryland research in the Middle East and India: ICARDA. Plan Puebla was CIMMYT’s first coordinated attempt at increasing yields for rainfed maize. For wheat, the project in Turkey was CIMMYT’s first attempt at improving rainfed, winter wheat. These programs shared the similar characteristic that CIMMYT varieties did not result in higher yields in these specific locations, thus scientists turned to improvement in agronomic practices to raise yields.

An international rainfed spring wheat research program was not started until 1977 with the formation of ICARDA.⁴⁶ ICARDA, or an earlier version of it, was initially proposed to CIMMYT’s Board of Directors. In late 1971, a “proposal for regional coordination of the Mediterranean and near east cereal programs” was presented to the

⁴⁶ CIMMYT and ICARDA collaborated on wheat adapted to marginal areas starting in 1979, focused on Africa (Byerlee & Morris, 1993).

CIMMYT Board of Directors' executive committee (*A proposal for regional coordination of the Mediterranean and near east cereal programs*, ca. 1971). Though the authors are not listed, this report was likely based off of Stakman and Gibler's report from 1966 and 1967 on the Middle East. The report noted existing collaboration between the RF, FF, and USAID in the Middle East region. The CIMMYT Board of Directors were reportedly "enthusiastic" about this proposal (Finlay, September 20, 1971).

The next step towards a dryland research center in the Middle East occurred and an administrative level about CIMMYT: the newly formed CGIAR. The Technical Advisory Committee (TAC) guides the CGIAR's decisions about their direction of research (Pistorius, 1997). This committee evaluated the *Research review mission to the Near East and North Africa* by Skilbeck *et al.* (1973), who proposed a new center in the Middle East to the TAC in 1973. A subcommittee was formed in 1975, led by David Hopper of the International Development Research Centre of Canada (formerly of the FF in India), to go about establishing ICARDA (CGIAR, January 3, 1975).

The CGIAR gave ICARDA a global mandate for dryland field crops, especially barley, but also including durum wheat and triticale, and later, bread wheat (Yau, 1992). As for ALAD, which would provide the infrastructure and staff for ICARDA, Remenyi wrote that, "it was inevitable that the majority of its resources were optimal in environments that could most benefit from these [green revolution] technologies, typically the irrigated wheat areas. Little in the way of rainfed wheats was to be found" (Remenyi, 1978, p. 61). ICARDA would be "a dryland agriculture successor to ALAD" (Remenyi, 1978, p. 62).

ICARDA was officially formed in 1976, with funds from Canada and sixteen total donor countries and institutions, and located in Aleppo, Syria (IDRC, 1976). Most of the ALAD staff was transferred to ICARDA when it began operating on 1 January 1977 (Remenyi, 1978). Its initial objectives were “to improve the level and stability of production of the staple food crops of the region,” especially for rainfed conditions (Russell, January 1975, p. 1). Though the FF disagreed with giving ICARDA an international mandate for one crop, ICARDA was given international responsibility on the fundamental research, germplasm collection, and varietal improvement for barley, durum wheat, lentils, and broadbeans (Russell, January 1975). ICARDA was also to focus on the socio-economic well-being of rural populations by hiring sociologists and economists.

ICARDA was in many ways an extension of CIMMYT/IRRI model, but changing demands of the 1970s broadened its research program to rainfed areas and to explicitly consider socio-economic factors. Despite its similarities to CIMMYT, IRRI, and other international research centers, ICARDA was a deliberate shift away from the Green Revolution focus on only irrigated regions, and highlighted agronomy and socio-economic analysis over strictly germplasm improvement. ICARDA grappled with this new direction, as M. B. Russell, an FF consultant, asked, “Should ICARDA devote a major part of its effort and resources to programs which will not yield an identifiable production such as a IR-8 or dwarf wheat? If so wheat criteria should be used to measure Program effectiveness? Will such criteria be accepted as valid by donor agencies and the general public?” (Russell, January 1975, p. 31).

Conclusion

While this chapter pulls together crop improvement programs from four different continents, it highlights in each case, a shift away from green revolution research that focused on favorable environments. The programs were a result of, to varying degrees, external pressure from critics, donors, and administrators, and also internal desires to expand the global reach of CIMMYT. Related, Indian wheat scientists began to question the dogma of wide adaptation, nonetheless constrained by the rigid structure of the coordinate program. Finally, this chapter contributes to the generally understudied history of wheat improvement in the Middle East and North Africa.

CHAPTER 5

INDIAN AGRICULTURAL SCIENCE POLICY

Introduction

It is now obvious that wide adaptation helped justify a research system focused on favorable environmental conditions in India. Unfortunately, these conditions do not exist in the majority of India's agricultural landscape. Even as national and international research goals have shifted towards promoting equity of rural populations and alleviating poverty, the existing Indian wheat research system has embedded many unhelpful assumptions about the connections between wide adaptation and these social goals. The Indian wheat research system is still very much in line with the policies implemented during the green revolution that assume that technologies are widely adaptable. Wheat breeders can assume that the benefits of their research will spillover into more marginal production areas, and are not required to participate in the agricultural innovation system beyond developing and releasing varieties. On the policy level, changes in the organizational structure of Indian wheat research have had limited impact since the 1970s (Raina, 2003). Policies and programs implemented through a variety of mechanisms have not led to a more responsive, client-oriented, or self-evaluative system (Raina, 2003).

Scientists today, especially in countries like India with a long history of adhering to wide adaptation, must decide whether the green revolution narratives will hold up under climate change and other constraints, new and old, to food security. While wide adaptation had some success during the green revolution by increasing overall food production, those gains have stagnated and have not improved overall food security. In

this final chapter, I will analyze the current state of agricultural science policy in India, and give suggestions for how science policies can lead to improved food security.

Two recent publications (Acharya & Das, 2012; Mishra, Ravindra, & Hesse, 2013) are a call-to-arms for investment in India's rainfed areas, especially the northeastern region that suffers from a number of problems including low crop productivity, lack of electrification, and pervasive poverty. According to an Indian coalition called the Revitalizing Rainfed Agriculture network, "state-directed policies relating to the Green Revolution have resulted in a situation in which any kind of state support for the agriculture sector becomes effective only when there is availability of water for agriculture" (Acharya & Das, 2012, p. 105). These authors argue that the Green Revolution policies and technologies cannot be simply extended east, but that new institutional systems are needed. Mishra, Ravindra, and Hesse also identifies the need for a complete overhaul of the Green Revolution strategy of top-down, resource intensive agriculture in favor of a "location-specific, decentralized" system that will help revitalize rainfed areas (Mishra, Ravindra, & Hesse, 2013, no page number). Despite these calls for change, which date back to the Green Revolution,⁴⁷ the Indian wheat research system is still seen by many as excluding rainfed research. A recent article from India's Economic Times found that ICAR spends only 13% on rainfed research, and rainfed areas receive 6-8 % of national agricultural subsidies (Srinivas, 2012). Nor is there any indication that an increased investment in rainfed research would actually benefit end-users in areas, such as Bihar, that are marginalized from the research and extension system. Based on

⁴⁷ India's Fourth Five Year Plan for 1969 to 1974, was the first five year plan to draw attention to rainfed areas.

area alone, this suggests a severe underinvestment, as rainfed lands account for 68% of India's agricultural land (Mishra, Ravindra, & Hesse, 2013).

History of Location Specific Research in India

As agricultural scientists in India realized that the benefits of the green revolution had been limited to irrigated areas, the concept of location specific research gained popularity in the 1980s. In many ways, location specific research is shorthand for research that specifically focuses on small and rainfed agricultural systems. These systems typically face more agro-climatic variability and have a lowered capacity to control that variability through irrigation or other management practices. Many of the people who advocate location specific research also call for more specific adaptation of crop varieties and technologies. Historically, calls for specific adaptation and location specific research have been linked with client-oriented research and participatory breeding programs (Annicchiarico, 2002). These programs have become increasingly popular in developing countries since the 1980s (Annicchiarico, 2002).

The idea of delimiting research programs to a smaller regional level emerged in India in the late 1960s, in response to green revolution-era research. The famous agricultural economist and Planning Commission member Samar Ranjan Sen wrote in 1969 that “only recently, the experience of IADP [Intensive Agricultural District Program] has led to the recognition of the importance of delimiting agro-climatic zones *within the district* for purposes of policy and programme formulation” [emphasis added] (Sen, 1969, p. 33). In his 1969 book, *Modernising Indian agriculture*, Sen wrote extensively about the need for research “directed to local situation at the field level and in

the various agro-climatic regions to meet local needs” (Sen, 1969, p. 123). He wrote further:

To be really effective it is necessary to evolve specific programmes to suit the agro-climatic conditions in each zone and to communicate the specialized, localised aspects of this programme to all the farmers in the zone... Needless to say that programmes, even if excellently prepared at the national level or state level, do not exactly fit into the local conditions at the operational level (Sen, 1969, p. 46).

It is not clear how influential Sen’s ideas were at that time, but in the wake of the green revolution and over the next few decades, calls for more location specific research entered the discourse of Indian agricultural science policy. Economists Kalirajan and Shand wrote in 1982 that the All India Coordinated Rice Improvement Program “recognized in recent years that paddy breeding research must be decentralized to become location-specific” (p. 538), though there was no similar action from the wheat program. This was largely because rice-growing conditions in India varied significantly more than wheat-growing conditions.

The World Bank-support National Agricultural Research Project (NARP) ran from 1979 to 1996, after being renewed once in 1986. This program was created in response to the limitations of green revolution-style research, which operated on a highly centralized basis and failed to address local variability, especially in rainfed conditions. NARP aimed to support decentralized, location specific research through the state agricultural universities and extension (Balaguru, Venkateswarlu, & Rajagopalan, 1988).

It also aimed “to give special emphasis to cereals, pulses and oil seeds under rainfed and mixed farming conditions” (World Bank, 1997, p. ii). Related to the NARP, in 1988 India’s Planning Commission created the Agro-climatic Regional Planning program in order to support location specific research and development, based on the relatively new farming systems approach as well as emphasizing inter-generational sustainability (Singh, 1990).

But because of the dominance of wide adaptation, research organizations have struggled to support locally focused research. Even 10 years after NARP was implemented, centralization and wide adaptation were still dogma in Indian wheat research. A 1989 report on the state of India’s State Agricultural Universities found that the “government requires all donor funds to be channeled through ICAR, which delays or even prevents extramural funding of projects. In addition, it means that research projects that receive high priority are the ones which have a broad national coverage” (Easter, Bisalial, & Dunbar, 1989, p. 1203). The authors highlighted “the need to decentralize decisions concerning research budgets,” and concluded that the existing system was biased against rainfed agriculture due to its location-specific nature (Easter, Bisalial, & Dunbar, 1989, p. 1203). Even with repeated calls to focus research on rainfed and dryland areas, these efforts have had only marginal success.

To help explain why efforts at organizational reform have had limited impact, science policy scholar Rajeswari Raina has written extensively on the barriers to institutional change in Indian agricultural science. She argued that, “by the mid-1970s, agricultural science had become one of the administrative inputs locked into India’s food

security goals by weeding out the variety or organizational formats and sources of funding and eulogizing the achievements of the green revolution” to the detriment of state level agriculture (Raina, 2011a, p. 109). Raina has ultimately found that repeated calls at organizational change have failed because they do not address the existing norms, values, and incentives of research that were created before and during the green revolution. She has suggested that scientists and administrators need to adopt new norms that encourage the researcher to “cater to the local farming communities, respond to different biotic and abiotic stress, and foresee and warn the state and stakeholders... about possible risks” (Raina, 2011a, p. 112).

Today, Indian wheat scientists still fervently discuss the merits of wide adaptation versus decentralized, location specific research. Polarized views on either side exist, as well as those holding hopes of a more pluralistic system that can address country-wide as well as region-specific problems. In order to further investigate the current context of values towards various research models, in 2013 I conducted nearly fifty interviews with current and retired wheat scientists from a variety of research institutions in north India (see Introduction for interview methodology). The results, which are explored in the next section, indicate that Indian wheat scientists hold a great diversity of views on the current research system. While wide adaptation has been the dogma of Indian wheat science for fifty years, many (roughly half) of the scientists I interviewed expressed a desire to see more location or condition specific research, through methods such as breeding for specific conditions. Currently, only the well-funded states such as Punjab and Haryana

have this capacity. This research capacity is severely lacking in rainfed (northeast) and dryland (central and south) areas in India.

Interviews: Overview

My interviews covered a broad range of topics related to wheat research in India (see Appendix 1). From these interviews, I learned a great deal about how the All India Coordinated Wheat Improvement Program presently works, and also a bit about its history, from more senior scientists and administrators. One of my primary goals was to elucidate on what scale scientists conducted their plant breeding, pathology, agronomy, and quality research, i.e., on a national, zonal, state, or district level. I asked this directly through question 10 of my interview: “Does your research focus on a wide area or specific location?” Responses could be coded from twenty-five of the interviews I conducted.⁴⁸ The coded responses to interview question 10 are given in Table 5. This question and the follow up, question 11, “Do you consider factors like micro-climate and marginal environments in your own research?,” elicited nuanced and extremely varied responses from the scientists on the present state of location/condition specific research as well as their personal views on the topic.

In both questions 10 and 11, I let scientists define for themselves what they meant by wide area, specific location, micro-climate, and marginal environments. To some, specific location meant the district or even sub-district level. To others, a state or multi-state agro-climatic zone represented a specific location. The majority of scientists responded that they considered microclimatic factors in their research program, or that

⁴⁸ I chose these interviews for coding based on whether the scientist answered all of the questions and allowed me to record the interview.

they hoped in the future to consider microclimate in the research. Scientists in northeastern India noted that there were several location specific challenges there, including a lack of late-sown (short duration) varieties. Other challenges in the northeast included terminal heat stress, moisture stress, the need for more effective extension, and the need to recommend the right varieties to farmers and to take old varieties off the market.

Research by area. As **Table 5** shows, most of the wheat scientists that I interviewed were working on research delimited by an agro-climatic zone. These agro-climatic zones are determined by the All India Coordinated Wheat Improvement Program, and as described in Chapter 3, the concept of delimiting research by broad agro-climatic zones was codified during the RF's involvement in Indian wheat research in the 1960s. India is presently divided into six agro-climatic zones for wheat. Because my study was restricted to the Northeastern and Northwestern Plains Zone, most of my respondents were also working in this area. Five of the scientists interviewed worked in northeast India while the remaining respondents were located in the Northwestern Plains Zone. Scientists who responded that their research area was the state were those working at a state agricultural university. The two scientists who responded that they work on the basis of specific conditions were both located in Bihar. Those who were working for the whole country were located at the Directorate of Wheat Research (DWR; headquarters of the All India Coordinated Wheat Improvement Program). These responses are consistent with the mandates of the different research organizations.

Table 5

Coded Responses to Interviews with Indian Wheat Scientists.

Question: Does your research focus on a wide area or specific location?
Specific conditions only: 2
By state: 3
By agro-climatic zone: 15
General conditions only: 5

Influence of the Green Revolution. Many of the scientists' responses reflected the influence of the green revolution and Borlaug's concept of wide adaptation. It is clear that wide adaptation still remains a major goal of the wheat-breeding program. One respondent said, "in the All India Coordinated program our aim is to have varieties with the wider adaptability" (S22). A scientist at the Directorate of Wheat Research said, "our mandate is complete India. We are not confined like in state universities, which have location specific mandate" (S6). Other statements reflected opinions such as "every breeder likes to go for developing the material which can fit everywhere" (S1). Several scientists supported research on a zonal basis only, stating that, "In my opinion we should breed for larger area... The conditions for wheat cultivation do not vary much" (S3) and that "we need varieties that should not be very specific to any particular location, but should have a wide adaptability" (S9). Others noted that even though their focus was at the zonal level, "it applies to whole India" (S18).

Breeding methods and philosophies also reflected the historical influence of Borlaug's belief that wide adaptation means a stable phenotype, regardless of environmental context. While they were not asked to define wide adaptation, scientists generally referred to wide adaptation to mean a variety that can be grown over a wide area and that is resistant to different types of stress. For example, "if a varieties having

wider adaptability, it means it is not much influenced say by climate” (S1). One scientist stated, “whether it is due to disease, biotic stresses, abiotic stresses, whatever the stress is, we breed so the losses to the stresses are minimized because of inherent capacity of the variety” (S6). Another scientist mentioned shuttle breeding as a way to breed climate-resilient varieties, stating:

Our main challenge is to select and breed the varieties which are adapted to wide range of climates. So being climate resilient varieties, a particular variety can yield higher under higher temperature regimes, under low irrigation, or even after if the cooler period is gone. We are using this shuttle breeding program to select such varieties. (S11)

The interviews also reflected that many scientists were satisfied with the prevailing system of “working for the wheat improvement for optimal environment” (S18). One scientist noted that, “better performing genotypes having wider adaptability are selected on the mean basis only. Environment effect is not taken into account” (S14). Although the same scientist stated that “varieties developed for high yielding environments” did not perform well for marginal farmers, because “they’re not putting input so they’re not getting yield” (S14). These statements and those reflected in the previous paragraphs are extremely resonant of the green revolution-era arguments seen in Chapter 3. There are strong institutional values towards wide adaptation and breeding under favorable environments, as I have claimed throughout my dissertation. They also reflect Borlaug’s bias towards the “inherent capacity” of the genotype and a neglect of the environmental context of yield.

Location specific research for microclimatic variation. Despite widespread acceptance of wide adaptation as a main goal of the wheat improvement program, many of the scientists expressed a desire to see more location specific research and *in situ* breeding and research of wheat varieties. Scientists reflected statements such as “in future I’m hopeful that we are going to consider the microclimates at the district level” (S15) and “because our zones are so large... if you propose a single variety for such a large area, it’s not a good practice” (S6). One research administrator stated that the “varietal development and testing program needs to be very careful today according to a smaller or different type of agro-climatic significant difference... maybe the number of varieties will be more... but the productivity, I believe personally, productivity may go up” (RA1). One scientist in northeast India stated that, “I would like to recommend the varieties suitable for specific conditions. Because the conditions differ very widely, if you go for 15 km, 10 km, there is difference” (S2). A wheat quality scientist noted that, “the zones that they have made are basically from a yield point of view or similarity of the agro-climate. But that doesn’t suit for me as a quality person... we get location specificity in case of quality” (S19). These concerns were closely tied to ideas that the agro-climatic conditions within a zone, state, or even a district are too varied to address with a one-size-fits-all breeding program.

These scientists conveyed varying levels of dissatisfaction with the present system of breeding and testing, where breeding is done under favorable conditions, and then finished varieties are sent out for multi-location testing. One scientist described the system of multi-location testing as “just looking into our selection of varieties and if

something clicks, by chance, we send it” (S19). In other words, varieties are not bred for specific conditions, and this assessment for their fit under rainfed or other conditions is only post hoc.

Among other critiques of the coordinated system, one researcher argued that the testing and release program should be opened up to more players:

It will be better if we open our system of testing and releasing the varieties. ...*our system of the coordinated trials has evolved about 20 to 30 years before, when there was limited players in the seed production and the private players were not even present*, but now that the country has grown very fast, the number of private players, particularly for the seed production, are interested, so you should propose or let the system be open to accommodate a large number of varieties.... *I presume it's suicidal to promote a single variety to be grown in a zone like the northwestern plain zone, where one variety at a particular point, say PBW343, CIMMYT's strain, was dominating over 9 to 10 million hectares, and that variety during its period of time, was susceptible to yellow rust, but by chance it escaped during its period of dominance... you are serving on the plate the condition for epidemic by growing a single variety... so you must... even through other routes than the coordinated program, to develop the strain and get it released to the farmers, so that diversity is created. [emphasis added] (S6)*

This researcher also noted that in the current system, the best one or two varieties are released in a zone, and usually only one is promoted by the extension program, leading to a wheat-growing area more vulnerable to abiotic stresses.

Finally, based on my interviews, it appears that very little research, if any, is done *in situ* for specific conditions or locations (other than for favorable conditions). Despite this, many scientists conveyed a desire to see more *in situ* wheat breeding and testing programs. A scientist reflected:

To select for the area for which we are targeting, we should do our whole plant breeding in that area, meaning we should not put our finished material in that area... Then we will be able to get better yield. If we do the breeding for drought in irrigated areas ... then we might get some of the genotypes but that might not be true for all. (S14)

One scientist noted the vast difference between the breeding station and the farmers' field, "whatever we breed at the research institution we give all required input, and then we say this is the yield potential, but the same may not be reflected in the farmers' field" (S5), particularly in Bihar where most farmers do not irrigate more than once or twice a season.

A retired administrator also remarked on the historical evolution of the wheat research program, and how that has codified several problematic assumptions.

You see, exactly thus far, with the introduction of the high yielding varieties and the urgency to increase production the focus was in the well endowed, irrigated areas, very little attention to rainfed areas. *Also the general philosophy was that there are always spillover effects* and therefore—and there are good examples where those spillover effects have affected—for example, some of the marginal varieties were bred for the irrigated areas, they found a place in the rainfed

areas... The elements of development are the same: varieties plus fertilizers. Even that addressing the specific needs of a region was never on the agenda. And this was true even for the agricultural universities. Because largely when we received international germplasm the flow was from the center to the ICAR institutes to the coordinated projects down the line. Now primarily a lot of work they're doing under the local conditions, but... ultimately bigger funding comes from the central sources. *The specific focus on rainfed areas, my own feeling is... limited. And at least definitely varieties are not bred with those situations in mind. The kind of region-specificity is one of the major changes looking forward that should happen.* [emphasis added] (RA2)

While I previously stated that there are strong institutional norms of wide adaptation, my interviews reveal that many scientists value location specific research. They may feel a personal obligation to address the problems of rainfed farming, or they may feel that the current system has led to the release of inferior varieties. Despite this pluralism of views, however, these perspectives are marginalized by the centralized research system. This is one of several roadblocks to agricultural innovation and food security, which I will discuss in the next section.

Roadblocks to Agricultural Innovation and Food Security

The roadblocks to food security in India are complex and deeply embedded. These roadblocks occur at nearly every step of the food system. On the production side, constraints include abiotic and biotic stress, climate change, slow varietal turnover, and lack of extension in marginal areas. Recently, several Indian wheat scientists have

brought up the recent convergence of climate change with the stagnant growth of varietal yield potential (Joshi et al. 2007, Yadav et al. 2010). Yadav et al. point out that gains in production from the green revolution are stagnating because adoption of semi-dwarf varieties, fertilizer use, and irrigation have reached a saturation point (2010). Climate change is just one of many ecological threats to Indian wheat production. Other ecological threats include decreased groundwater levels in the Punjab and Haryana, terminal heat stress, and the global threat of the Ug99 pathogen, which 90% of wheat varieties are susceptible to (Aggarwal et al. 2004; Joshi et al. 2007, Ortiz et al. 2008, Singh et al., 2011; Yadav et al. 2010). While Ug99 has not yet reached India, Indian wheat production is vulnerable due to the widespread adoption of the susceptible PBW343 variety (Singh et al., 2011).

When analyzing the Indian wheat research system as a whole, a persistent problem is the poor linkages between research and extension (i.e. development of technologies and ground-level promotion and education of new technologies). In other words, there is a significant disconnect between the goals of wheat researchers and the practitioners of agriculture. Raina et al. wrote that, “The need to increase funding for and strengthen research-extension linkages in order to deliver knowledge about modern technologies has been a consistent refrain in every single review of agricultural research and extension, agricultural policy, trade and development since the 1970s (Raina et al., 2007, p. 14). From my analysis, I attribute this poor linkage to three main factors: 1) persistent reliance on the transfer of technology model of research, 2) lack of incentives

within the public sector for client-oriented research and extension, and 3) what Raina calls the “blame game.”

Transfer of Technology Model. Many scholars have pointed out that agricultural researchers very often adhere to the transfer of technology model of research, also known as the linear model of innovation (Biggs, 1990; Hall et al., 2000; Klerkx & Leeuwis, 2008; Thompson & Scoones, 2009). The linear model is defined as:

A simplistic conception of scientific and technical advance that nonetheless remains an influential driver of science policy. According to the linear model, innovation happens in the following way: basic or fundamental research contributes to a general pool of knowledge; that pool of knowledge provides a resource for engineers or other innovators, who then apply it to create products that increase productivity, drive economic growth, enhance military power, and otherwise enrich lives and benefit society. This model assumes that advances in knowledge are by and large beneficial to society, and that the benefits are both automatic and unpredictable. It also assumes a unidirectional flow of knowledge that privileges basic research above applied as the originator of all scientific benefit. (Meyer, 2011, p. 64)

In this agricultural research community, adhering to the linear model of science means that researchers do not focus on the impact of their research. Instead, they focus on producing basic science and assume that this will lead to socially desirable outcomes. Others have described this as the “loading dock model” of research, meaning “You take it out there, and you leave it on the loading dock and you say, there it is. And then you walk

away and go back inside” (Cash, Borck, & Patt, 2006, p. 484). Like the linear model and the loading dock model, the transfer of technology model assumes a linear flow of knowledge and benefits from the scientist to the end-user, in this case the farmer. It also reinforces a sharp divide between scientists as producers, extension personnel as strictly educators, and farmers as passive consumers.

We see evidence of the linear model of science in the hierarchy of agricultural sciences in India. The more basic sciences, such as plant breeding and biotechnology, are more prestigious, while the applied agricultural sciences, such as agronomy and extension, are less so. Yadav et al. wrote that for Indian wheat production, “Germplasm improvement is still paramount” (2010, p. 166). The centralization of wheat research in the northwest also represents the belief that basic research from this area can be transferred equally well to other areas. Thus, there is no need for strong state and regional research if one believes in the linear model. This is shown in statements such as, “Supporting basic and strategic research that generate ‘spillovers’ broadly” (Mruthyunjaya & Ranjitha, 1998, p. 1095). While basic research such as plant breeding is obviously important, what is the point of it if most new technologies are not reaching farmers? Hall et al. recommend that, “the public sector needs to shift from scientific research *per se* to R&D activities focused around themes relating to improved economic production” (2002, p. 174). I will further explore this at the end of this chapter.

Public Sector Research. Another major barrier to an effective agricultural innovation system in India is the public sector orientation of wheat research. Because of the public sector orientation, there is a lack of incentive to achieve specific outcomes.

Hall et al. wrote, “the broad patterns of institutional arrangements in the public sector remain unchallenged” (2002, p. 173), resulting in strong disciplinary divisions and isolation among and between organizations. This isolation between different research organization and between research, extension, and seed production is a major problem in the wheat sector.

For wheat, research is almost all performed in the public sector—by the state agricultural universities, IARI, and the DWR. Because wheat is self-pollinating and not hybridized, farmers can save their seed (Krishna et al., 2014). Thus the public sector has a mandate to focus on wheat, which may otherwise be a “market failure,” unlike hybrid vegetables and other crops that are more appetizing to the private sector. These public sector organizations help decide what wheat varieties to release, and then seed producers—which include the National Seed Corporation, state seed corporations, and private seed producers—put in requests for seeds that they will then multiply and distribute. Extension programs in India are also largely public sector (Babu et al., 2013). Wheat procurement and distribution are supervised by the Food Corporation of India, a government-run organization.

While public sector system of research, seed production, and marketing operates efficiently in Punjab, Haryana, and western Uttar Pradesh, the situation in northeast and central India is quite different (Nagarajan, 2005). Based on my own interviews in northwest and northeast India, there was a very stark difference in farmers’ access to extension, markets, and inputs between the regions. In the northeast, extension efforts are concentrated around the research stations, and the main source of information for farmers

is from seed and fertilizer sellers. Babu et al. reported that in 2003, “Sixty per cent of the farmer-households in India did not access any information on modern technologies that year” (2013, p. 162). According to my interviews, there is also a definite gap between the varieties that scientists recommend for particular areas and the varieties that are actually multiplied and provided to farmers. This is partly because some farmers demand older varieties, for example the variety UP262 (released in 1977) is preferred for its quality in northeast India. Recently, it has also been more difficult to establish the yield advantage of varieties over PBW343 (released in 1995). But the lack of adoption of new varieties also points out the poor linkages between research, extension, and seed production.

Krishna et al. wrote:

First, the research system may not be efficiently identifying and translating farmer preferences for varietal attributes into cultivars that they are willing and able to adopt. Second, the seed production system may not be producing what farmers actually demand due to a poor seed demand assessment system. Third, extension programs and other distribution mechanisms may be underperforming in their efforts to convey the genetic superiority of improved varieties to farmers.

(Krishna et al., 2014, p. 18)

In my research I observed that the first topic is seldom discussed. There was a near universal agreement that the biggest farmer preference was for yield, thus, the entire research system is built around evaluating varieties for their yield. Studies have shown that farmers do not *only* value yield, and that they will accept lower yields for higher

yield stability over time (Bellon & Risopoulos, 2001). All three of these points merit further research, as they are extremely underdeveloped in existing data and literature.

The ultimate problem with so much of the wheat sector being public is there are few incentives for the system to either set or meet outcomes (e.g. replacement of varieties, increased on-farm profits, food security). There is a variety of evidence from other sectors that just because a public sector intervenes to fill a market failure, this does not ensure a successful outcome (Bozeman & Sarewitz, 2005; Meyer, 2011; Trouiller et al. 2002). Barry Bozeman (2002) has called this a public value failure. Public-private partnerships are typically recommended to support inefficient public sector activities, and there is some evidence that privatized extension services are successful in India (Babu et al., 2013). But the general institutional barrier that the public sector faces is the lack of client-orientation.⁴⁹ But despite a shift in global research trends, Indian wheat research holds strong institutional hierarchies that discourage consulting end-users of technologies, ultimately resulting in technologies that are not adopted (Hall et al., 2002; Yadav et al., 2010).

The Blame Game. While India's wheat production has increased since the green revolution, the food security of its residents has not. Naturally, policymakers and researchers try to explain this disconnect between production and food security. What results, however, is what Raina called "the blame game." Policymakers blame scientists, scientists blame extension, etc. for not carrying through with transfer of new technologies. Raina wrote:

⁴⁹ Client-oriented agricultural research surfaced in the 1980s along with farming systems research (Byerlee & Tripp, 1988).

When it comes to credit for food production, the agricultural technologies – the very varieties, irrigation, chemicals and pesticides, and the research organizations take it all. When it comes to the blame game – about persistence of rural poverty and hunger, child and adult malnutrition, environmental and social disruption, it is the other organizations and policies – the Food Corporation of India and the Public Distribution System, state and national level schemes for rural employment and poverty alleviation, input subsidies, rural credit, irrigation policy, international trade and the WTO, that are accused. (Raina, 2011b, p. 2)

This is supported by my interviews, where extension efforts in northeastern India were often blamed. S. Nagarajan, former director of the IARI and the coordinated wheat program, wrote in 2005 that “National-level wheat production will change if only the states in NEPZ [northeastern plains zone] and CZ [central zone] take their assigned responsibility with seriousness” (Nagarajan, 2005, p. 1468), blaming lack of wheat production on the lack of “seriousness” in those areas (and conveniently forgetting about the structural barriers to research and production there). In a 1998 paper by agricultural researchers Mruthyunjaya and Ranjitha, they wrote, “extension will necessarily have to adopt problem-solving approaches, and develop methods for transferring more site specific information and technical knowledge” (Mruthyunjaya & Ranjitha, 1998, p. 1095). While this is certainly true, the already-overburdened extension system is made the sole responsible party for ensuring that location-specific information is transferred (Raina et al., 2007). And finally, I found that scientists blamed intellectual property rights (IPR) for creating a hostile system for scientists. For example, it was mentioned that

unfinished varieties simply could not be given to farmers (a critical step in participatory breeding methods) due to the IPR restrictions.

The persistence of the blame game has two primary negative outcomes. One, of course, is that parties tend to absolve themselves of blame and fail to recognize their own role in the innovation system, instead sticking to the rigid idea of the transfer of technology, or loading dock, model. Two, parties overwhelmingly fail to recognize that production is only one part of the food security equation. Scientists tend to focus on the “yield gap” between optimal and actual field conditions, and are constrained to the production paradigm. Sen’s classic text (1983) on entitlements and the distribution of food is certainly worth considering. Within India, there are serious problems with wheat procurement, distribution, and storage that must be addressed. That said, Indian scientists and policymakers must critically reconsider the agricultural innovation system—including its goals, linkages, and outcomes—in order to improve food security in the country.

Abiotic Stresses and Climate Change

Many scientists now imagine agriculture under a post-climate change landscape. The concept of adaptation plays two parts in these future scenarios: the more narrow study of plant physiological adaptations to climate, and the broader suite of socio-ecological and technological changes in agriculture and rural economies needed to adapt to climate change. In international agricultural research organizations, the first vision—of plant physiological adaptation—is most prominent (Reynolds, Pask, & Mullan, 2012). This is not surprising given the historical trajectory of green revolution research. Araus *et*

al. state, “plant adaptation is a key factor that will determine the future severity of the effects of climatic change on food production” (2008, p. 379). Scientists seek to create climate-proof or climate-resilient plant varieties, in other words, they seek varieties that can tolerate or escape abiotic stresses. This approach is supported by literally decades of institutional support that favors plant breeding and varietal improvement as a primary motivator of agricultural production. It also fits perfectly into the narrative of widely adapted crop varieties; i.e. one variety that can be grown over a variety of locations and conditions. The problem with the abiotic stress approach is threefold: 1) the current Indian wheat research and testing system is poor at identifying and releasing abiotic stress tolerant varieties, 2) new abiotic stress tolerant varieties are not likely to reach the rainfed and marginal farmers who would most benefit from them, and 3) it ignores the synergistic effects of climate change on abiotic and biotic stresses (e.g. how pathogen ranges and vulnerabilities will change due to climate change).

Indian farmers face a range of abiotic stresses, but especially drought and heat stress. A pervasive abiotic stress in the wheat-growing areas of the Indo-Gangetic Plains is terminal heat stress, which is a sudden rise in temperature at the end of the wheat season that results in shriveled grains. According to Yadav et al.,

A significant portion of wheat growing areas in India comprising southern Haryana, whole Rajasthan, Madhya Pradesh, Gujrat, eastern Uttar Pradesh and part of Bihar faces abiotic stresses particularly terminal heat and water deficiency during reproductive phase. In most of these areas except Haryana, the yield gain due to green revolution has been limited. (2010, p. 167)

Further, while the wheat producing areas of India are officially classified as over 80% irrigated, “only one-third receives full irrigation” and most wheat, by area, is grown under partial irrigation (Joshi et al., 2008, p. 437). These existing abiotic stressors are compounded by the predicted impacts of climate change (O’Brien et al., 2004; Ortiz, et al., 2008)

While abiotic stress tolerance has become a major goal of some agricultural research organizations, the national mandates for research and testing provide limited avenues for scientists to develop stress-tolerant varieties. Wide adaptation still rules implicit and explicitly in Indian wheat research. Because of the wheat research system’s orientation towards wide adaptation and the way it is codified into the wheat testing system, the identification and release of varieties targeted towards a specific set of stress conditions is severely hampered. For a new wheat variety to be released in India, scientists must prove that it is widely adapted within an agro-climatic zone. Wide adaptation is measured by the average yield over location; thus, potentially stress-tolerant varieties that have lower average yields are ignored. This constrains location specific research towards problems like terminal heat stress; further compounded by the northwestern centralization of Indian wheat research that marginalizes research in other agro-climatic regions. Research funding is largely centralized, leaving little leverage for location specific endeavors. Relying on wide adaptation means scientists are still focused on developing a few varieties, leaving a vulnerable genetic base against abiotic stress and pathogens (Krishna et al., 2014). Not only does this create a system vulnerable to stress,

but it overlooks the needs of marginal farmers who work under low inputs and high climatic variation.

In India, a senior wheat breeder reflected on challenges of wheat breeding for abiotic stresses. He stated, “We never really talked of heat tolerance. I remember the early parts of my work here, working with my senior colleagues, we didn’t talk of heat tolerance as such, neither drought tolerance as such, though we were breeding for a rainfed environment, in one situation, and a late sown environment in another situation” (S24). He hypothesized that “because much less success, historically, has been witnessed in this area compared to the biotic stresses, compared to the plant architecture... that sometimes puts apprehension in the mind of the [research] worker” to work on heat and drought tolerance (S24). He further noted that heat tolerance and climate change will be a major issue in the future, because “the way we are growing it [wheat] here is already stretching its adaptation” (S24).

Technological ways to ameliorate terminal heat stress exist, such as developing shorter duration or heat tolerant varieties. Developing so-called climate-resilient wheat varieties has been the focus of a USAID-supported project in India (USAID, 2013). But these basic research-oriented approaches to abiotic stress and climate change ignore the many biophysical and socioeconomic constraints to diffusing new varieties in heat stressed areas such as the northeastern Indo-Gangetic Plains, where research, extension, and seed distribution systems are poorly developed and networked. While the silver bullet of abiotic stress tolerant varieties is appealing, it ultimately falls into the loading

dock model of research, and fails to recognize the limitations of technological agricultural solutions in India.

Agricultural Science Policy in India

As an American historian of science, I am not in a position to recommend any specific policies for Indian wheat research. But as the result of my research and observations of the Indian wheat research system, I find the works of science and technology scholars Raina and Desai compelling in their unified call for Indian scientists and policymakers to articulate the science policy goals for Indian wheat research, and I would add, a frank look at the tradeoffs of different outcomes.

In early 2013 I attended a science policy workshop in New Delhi, populated by Indian science and technology scholars and activists. A refrain established early in the day was, “there is no Indian science policy.” The same can be said for Indian agricultural science policy. The goals established by the Indian Planning Commission and ICAR/DARE (Indian Council of Agricultural Research the Department of Agricultural Research and Education) have a long history of mismatch and neglect. Current agricultural science policy is schizophrenically divided between increasing aggregate production while calling for more attention to rainfed agriculture (Mishra, Ravindra, & Hesse, 2013). As Raina described, the only goals are currently articulated as “economic growth rates or food production targets” (Raina, 2011b, p. 1). This is under the implicit understanding that “agricultural growth is a means to the larger goals of employment-led growth and poverty reduction” (Desai et al., 2011, p. 43). The Government of India has repeatedly called for a more inclusive type of agriculture, as seen in recent campaigns

such as “Bringing the Green Revolution in Eastern India,” *National mission for sustainable agriculture*, and the *National food security mission* (Government of India, 2007; Government of India, 2010; Government of India, 2014). But when scientific outcomes are only measured by agricultural growth and production, there is no way that these promulgations can succeed in their stated goals.

To begin, it is obvious that the Indian wheat science policy is in need of change. From the policy planning side, Desai et al. wrote,

Policy planners recognised the need for higher agricultural growth, but they placed too much faith in the reforms and not enough emphasis on critical policy changes. More specifically, policy neglected basic institutions such as the Indian Council of Agricultural Research (ICAR), state agricultural universities (SAUs) and departments of agriculture in the central and state governments. It relied on the market to evolve and transfer relevant technologies rather than getting it being done through institutions that governments had arduously built over the earlier decades. (Desai et al., 2011, p. 42)

They wrote further that:

The central government should aim at is visualizing the “outcomes” related to seed/breed-cum-resource centered technical change. These outcomes would relate to the productivity of field crops, livestock and horticulture; the real agricultural GDP growth; this growth as a ratio of real overall GDP growth; the change in the number of poor and poverty ratio; the change in real earnings of farm labourers; and the incidence of food inflation. (Desai et al., 2011, p. 48)

Finally, Desai et al. listed some of their recommendations, the first three of which are listed here:

- (1) The central government must adopt and encourage decentralization of the ICAR's organisation and management and funding operations, as recommended by the Mashelkar Committee and the Swaminathan Committee.
- (2) The ICAR's system and SAUs must be restructured to produce relevant research and higher agricultural education by granting them academic autonomy and the funds to effect a change in the paradigm to that of the new technology.
- (3) This paradigm requires human resources that will implement location-specific integrated farming instead of advocating reducing the gap in yield by tinkering with farmers' fields and experimental farms. (Desai et al., 2011, p. 46)

Policy makers must decide what, exactly, are the goals of continued investment in public wheat research. Are the goals simply increased wheat production? Improved nutrition? Higher exports? Higher per capita consumption? Inter-regional equity? Poverty reduction? Or, as recently discussed by IFPRI researchers, should public policy “support small family farms in either *moving up* to commercially oriented and profitable farming systems or *moving out* of agriculture to seek nonfarm employment opportunities” (IFPRI 2015, p. 29)? All of these have come up throughout discussions over the years, but there is no meaningful connection between these goals and measureable outcomes.

Raina, in turn, called for scientists themselves to “articulate the goals, demands, inadequacies, advances, and improvements needed in S&T to ensure delivery of development goods for the agricultural sector” (Raina, 2011b, p. 5). If scientists can

identify their own goals—not just research goals, but outcomes, such as varietal adoption, etc.—they can assess their own research system and critically engage with policymakers. Finally, with a nod to history, India faces the same question that ICARDA did in 1975, which is whether it should “devote a major part of its effort and resources to programs which will not yield an identifiable production such as a IR-8 or dwarf wheat? If so wheat criteria should be used to measure Program effectiveness? Will such criteria be accepted as valid by donor agencies and the general public?” (Russell, January 1975, p. 31). Indian wheat research is publicly funded, thus it does have a fulfillment to stakeholders in India. Whether they want to be measured as “a burden on the State as it failed the farmers and the farming sector,” as Kerala Agricultural University was recently called, or not, is in part up to scientists and policymakers (Sudhi, March 27, 2013).

Conclusion

In summary, I will reference Thomas Hughes’ theory of technological momentum and examine the possible ways that the Indian wheat research system can emerge from the current track of institutional path dependency. My main argument is that radical institutional change (changing of values, professional norms, and bureaucracy) is necessary to make a lasting change in Indian wheat research.

Raina, among others, has written that, “There is a pervasive inability to perceive and change these institutional rigidities within the agricultural research and extension system in the country” (Raina et al., 2009, p. 10). These institutional rigidities were formed during the green revolution, as I have shown in Chapter 3, and continued up to today due to bureaucracy, professional norms, and the rules of the wheat research system

that have all been strengthened by the green revolution narrative. Indian research administrators must seriously consider whether the top-down, centralized system is sustainable. When faced with a food crisis in 1942, 1958, and 1965, India chose the top-down approach, now canonized as the green revolution. It is true that the green revolution increased wheat production. But if Indian administrators want to support small farmers, as they have claimed since Indian independence, the current institutional arrangement of Indian wheat research must be seriously overhauled. Raina pointed out that traditional prescriptions of organizational change have for the most part failed at creating institutional change because these policy prescriptions fail to address “rules, norms or habits” (2003, p. 97). As I have shown, continued calls for more attention to marginal and rainfed research have failed because they do not change the key factors of technological momentum in the wheat research system.

I suggest that while wide adaptation theoretically would be helpful in buffering the impacts of climate change, in reality, the current system is focused on favorable conditions. In order to create a research system that can address issues such as climate adaptation, terminal heat stress, and water stress, we must dismantle the narrative of wide adaptation and the problematic assumptions that it encodes. This requires a massive shift in the policy, values, and structure of Indian wheat research, requiring an overhaul in agricultural education, policies, and administration. But if accomplished, this could open the door for research focused on location specific problems and abiotic stresses. Further than dismantling narratives, however, Indian agricultural science policy faces serious hurdles to deliver food security. These can be partially addressed by syncing science

policy goals across sectors and more closely aligning public sector systems with the demands of end users.

CONCLUSION

My dissertation is a historical exploration of how certain plant breeding practices

became embedded in Indian and international wheat science. I initially set out to study how the agricultural research institutions set up during the green revolution were responding to impending climate change. But upon my first site visit to India, I was immediately intrigued by the pervasive rhetoric of wide adaptation in plant breeding experiments, and in the prevailing notion that abiotic stress tolerance could simply be “added” to crop varieties without considering the likely tradeoffs between yield and stress tolerance. These topics are, as I have learned and shown, both linked to Norman Borlaug’s concepts of wide adaptation and plant breeding under favorable conditions that he developed in the 1960s, and his subsequent deployment of these concepts to argue that certain varieties were superior under both favorable and stressed conditions.

While my historical approach and topic (the green revolution) are by no means novel, my study reinforces existing historical work and adds new insights to the literature. I reinforce the work of Cullather (2010) and Perkins (1997) to show that the green revolution was not a linear transfer of technology, but rather a result of coinciding socio-political forces such as the Cold War, concerns over population, a faith in technological solutions, the rising status of plant breeders, new institutions in international aid and development, and policy changes in India and other countries. This narrative rebuts the typical claim held by agricultural scientists that the green revolution was primarily driven by the discovery of semi-dwarf wheat.

If there is one thing the reader can take away from this dissertation, it is that Borlaug’s use of wide adaptation was both political and flawed, and that agricultural scientists today must move beyond this outdated concept. Borlaug believed that wide

adaptation implied genetic guarantee of high yields, regardless of environment. He strategically leveraged this concept to justify the expansion of his international wheat program and intervention in countries like India. In reality, Borlaug's wide adaptation was the result of a fertilizer responsiveness and photoperiod insensitivity, *neither of which are a guarantee of high yields*. These characteristics, like many genetic traits, are context-dependent and not universally applicable. Yet Borlaug's context-free, universal phenotype is what wide adaptation came to represent. Further, I add the claim that Borlaug's wide adaptation, which allowed wheat varieties that he bred in Mexico to grow in India and many parts of the Middle East, was a lucky historical accident. We saw in the case of Turkey that there was no widely adapted winter wheat, due to the particular agro-climatic circumstances of the region. We also saw in India that wide adapted wheats were not superior to local varieties in central and southern India, and that rainfed farmers preferred the tall Indian variety C306 even after Borlaug's varieties were widespread. Both maize and rice varieties, developed under similar institutional structures and scientific principles, never displayed the same global spread due to the wider variations in maize-growing areas in Mexico and rice-growing areas in south and southeast Asia. Unfortunately, wheat breeders today still rely on the rhetoric of wide adaptation to justify research programs aimed at favorable conditions. The somewhat recent success of CIMMYT's Veery wheat reinforced the green revolution narratives of a technological breakthrough in yield followed by a rapid adoption across wheat-growing regions. But "it is impossible to speak of yield without reference to the environment" (Lewontin, 1983, p. 146). The concept of maximum yield means nothing if actual farmers' field conditions

diverge from carefully managed research station trials.

The idea of wide adaptation permeated more than just discussions of germplasm during the green revolution. In the 1960s, agricultural research transformed from a “site-specific science” to a broadly applicable set of principles. Scientists and international organizations picked up the dogma that not just plant varieties, but also packages of technology, could be standardized and widely adopted. This narrative is pervasive, and it fails to recognize the variability and location specific needs of agricultural places. These range from particular agro-climatic circumstances to sociopolitical regimes. Robert Herdt, who worked for the Ford and Rockefeller Foundations in India in the 1960s, and later for IRRI, has written an exemplary list of lessons:

First, it is critical not to underestimate the temporal and spatial variability of the biological and physical conditions in which agriculture operates; second, it is critical not to underestimate the institutional challenges of agricultural development; third, ever-renewing agricultural technology is essential and simply transferring technology from other parts of the world or from international research centers will have limited value without local adaptive research; fourth, every country needs its own people with the capacity to conduct adaptive agricultural research and to design and implement agricultural policy; and fifth, people in assistance agencies, national organizations and in rural areas are the key to successful development assistance. (Herdt, 2012, p. 180)

These lessons directly contradict the philosophy of wide adaptation and the transfer of technology model that has been widespread in international agricultural research.

Finally, this dissertation helps illuminate the challenges that Indian and international wheat research organizations have faced when adjusting to changing paradigms of international aid. When equity became important to aid donors in the 1970s, wheat researchers slowly responded by attempting to address rainfed and dryland wheat. These programs are still severely marginalized. Agricultural research organizations have also struggled to address Sen's classic finding that increased agricultural production does not lead to improved food security (1983).

I would like to recognize that this dissertation deals with a very small part of the food system and the food security equation. I focus on the history of plant breeding and testing largely because of its historical importance, and the academic prominence of plant breeders (ascending from less prestigious "botanists") in India in the 1960s and beyond. In other realms of wheat research, I have largely ignored pathology, agronomy, soil science, hydrology, physiology, extension, and, more recently, biotechnology. I do not want to leave the reader with an impression that plant breeding is the most important science, but I am recognizing that over the past 50 years, it has been promoted that way. This perhaps traces back to the forced distinction of "basic" versus "applied" sciences, where basic sciences are viewed as more prestigious. It also has to do with the historical focus on the release of new varieties and the prestige tied to developing a popular variety, versus the less showy advancements in agronomy and soil conservation.

Finally, I rebuke the idea that we should uphold the science policies developed during the green revolution because they were successful for wheat. These institutions and principles were developed over fifty years ago, in a context of post-colonial

development and modernization that no longer applies. In other words, a modern green revolution by nature must be different than the previous green revolution: we should not constrain possible future innovations to the narrow set of political and scientific ideas present in the 1940s through '60s. Further, if we do draw from Borlaug's biography, we should remember that Borlaug's ideas at the time were radical and paradigm-shifting. While I challenge both the means and ends of Borlaug's wheat research program, he has rightfully earned his revolutionary reputation by challenging the institutional norms of his own education in the agricultural sciences.

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APPENDIX A

INTERVIEW QUESTIONS FOR PLANT SCIENTISTS

1: Can you tell me about your professional background?

2: What is your job in this organization?

2.1: From how long are you working with this organization/university?

3: Are you involved in the Wheat Improvement Project?

3.1: From how long are you working with the Wheat Improvement Project?

4: What does your current research focus on?

4.1: What other crops have you worked on?

Thank you, now I'd like to ask you some questions about wheat improvement in northern India. Please feel free to give your honest opinion; I am not evaluating anything and your responses will be confidential. I will ask some questions about current issues in wheat breeding and about your own research goals.

5: What do you think are the most important traits in wheat grown in northern India today?

6: Are there challenges to incorporate those traits in the Wheat Improvement Program?

6.1: If yes, for which traits?

6.2: What are the challenges?

7: What are the challenges of getting these new varieties to farmers?

8: What traits or characteristics in wheat grown in northern India will be the most important in the next 5 years?

8.1: In the next 10 years?

9: What is the goal of your research?

9.1: *(Follow up question about specifics)*

10: Does your research focus on a wide area or specific location?

10.1: Why?

10.2: Do most other scientists at this organization have similar or different goals?

11: Do you consider factors like micro-climate and marginal environments in your own research?

11.1: If yes, how?

12: Do you consider factors like farm size, and socio-economic status of farmers in your own research?

12.1: If yes, how?

13: Do you think that climate change is important for the Wheat Improvement Program?

13.1: How?

13.2: What climate factors do you think will be most important?

Thank you, now I'd like to ask you some questions about wheat policies and interactions you have with agricultural extension and farmers.

14: Can you tell me whether government policies have affected your research?

15: Do the current policies support the Wheat Improvement Program?

16: What is something policy-makers could do to strengthen your research program?

17: Do you involve or consult farmers or agricultural extension in your research program?

17.1: (if yes) How?

17.2: Has this changed since you started your career?

18: Is agricultural extension doing their job to promote new varieties of wheat?

18.1: Why or why not?

19: What is something agricultural extension could do to promote the goals of wheat research?

20: Are farmers adopting new varieties of wheat?

20.1: Why or why not?

21: What is something farmers could do to promote the goals of wheat research?

22: Who else do you collaborate with or involve in your research program?

That is all of the questions that I have today. Please let me know if you have any questions about your participation in my study.