

## **The Rockefeller Foundation and Agriculture in Peru**

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In January 2002 I visited the Rockefeller Archive Center for the second time. Two years previously, I conducted doctoral research there having spent eighteen months doing anthropological research on agricultural development and indigenous knowledge in the Southern Peruvian highlands. The latest trip was intended to gather more data for a book on Peruvian rural development, highland culture, and power. Rockefeller Foundation involvement in Latin America comprises part of the historical section for this work, which is mainly concerned with the contemporary plethora of agricultural development non-governmental organizations in the Andean region, and their impacts upon indigenous populations. The development models deployed by these NGOs are generally derivatives of the “green revolution” which can be traced further back to U.S. philanthropy in Latin America. Mine was a successful visit, finding much information that had been missed the first time. I would like to extend my gratitude to the Center in general for its generous support, and to all the staff there who helped me out. The report that follows is a story of the Rockefeller Foundation's (henceforth RF) agricultural and scientific undertakings in Latin America, particularly Peru, in the postwar period.

When a Rockefeller Foundation team of experts toured Peru in 1953, it reported that a “social schizophrenia” divided the country into two separate populations. On the one side, they found that “those who live in the coastal plain are progressive, intelligent and able, and have made real progress in developing their cities and certain of their industries.” On the other side, separated by “an almost insurmountable mountain chain,” were “the majority, chiefly the indigenous

populations, [who] live in the sierras and the mountain valleys,” a region “largely underdeveloped” whose residents “are not only poor, and poorly nourished, but their poverty and malnutrition are accentuated by the almost universal custom among them of chewing coca which contains the alkaloid cocaine.” One man on the team, a North American agricultural scientist, P.C. Manglesdorf, believed that “the principal agricultural problems of Peru are part and parcel of the entire social problem of the native Indians.”<sup>1</sup> Coastal Peruvians were apt to agree with the foundation that the “bajo nivel cultural” [low cultural level] of Indians was the principal problem: ever since conquest the “casta española” had been at pains to distinguish Indian “otherness” lest this elite, too, be compromised by racial *mestizaje*.<sup>2</sup> But pointing the finger at the Sierra did not indemnify those coastal Hispanics: for from the Rockefeller Foundation's (RF) perspective, even this group of “Latins” had much to be desired. Where Indians proliferated, underdevelopment in its innumerable guises was seen to flourish irredeemably like a social disease; but for the RF this did not mean that the coastal elite, despite their native version of science, universities and their significant conglomerate of research and experiment stations, could effectively deal with the problems. Although the educated Latins were further along the evolutionary track than those anonymous Indian “masses” whose agricultural practices were deemed so retrograde as to not warrant investigation, Peru's scientific elite was still thought to be in urgent need of guidance from above.

With a singular emphasis on Peru's “better class” as a vehicle of change, the RF launched a program there based on its Mexican experience. Yet as in Mexico, if basic research in Peru was “still in the embryonic stage,” scientists, too, were envisioned as embryos of sorts awaiting maturity.<sup>3</sup> The social context, however, was imagined replete with contaminants -- personal, moral, administrative and political. Scientists were hampered by their own preoccupation with “self-satisfaction,” their tendency to become “laboratory or herbarium hermits,” and by their vocational

surroundings in which “agricultural research often suffers from ignorance, indolence, politics, insecurity of tenure, lack of physical facilities, lack of adequate salaries, and an attitude that work in experimental plots is demeaning.”<sup>4</sup> Little of merit could be said for science and agriculture in Peru or in the rest of Latin America. In the name of the national interest to promote “complete and lasting understanding and friendship with our neighbours to the south,” the RF was geared to extend its philanthropic hand to a place which had “for many years been on its mind and conscience.”<sup>5</sup> Beginning in the 1940s, the RF took it upon itself to nurture these embryos of scientific promise and, concomitantly, to redefine the very “culture of science” in which scientists could evolve.

The aim of this report is to show how the RF attempted to construct the subject identities of Latin American scientists in the mold of those of North America who, as Deborah Fitzgerald has pointed out, embodied the land-grant college experience of the United States.<sup>6</sup> To achieve this, the RF not only had to steer the individual methods and practices of particular scientists, it also had to systematically propagate a discourse delineating the parameters within which proper science could be pursued. On these new grounds of demarcation, most of Latin American scientists and scientific institutions fell outside the borders of “good science”; and to gain respect and funding from the RF, Latin subjects had to redefine themselves, their surroundings and, to be sure, their ways of practicing science and speaking about science. Latin Americans had to begin to deny their own mischievous Latin proclivities if they sought to place themselves in good stead for RF support. This report tells part of the story of how that old divide between “us” and “them” was reinvented to suit the new agenda of the foundation, and how Latin American scientists were now compelled to cross what was by all accounts a reinvigorated chasm separating Latin science and the science of the United States.

Navigating the new divide was not only a question of scientific methodology; it was also a question of character, class, gender and ethnic origin -- locations which increasingly came to be identified through "culture."<sup>7</sup> But as becomes evident in this report, the language and practice of agricultural science tended to not only filter out and render marginal certain types of social groups, particularly indigenous farmers, through its unwavering focus on the material objects of knowledge; they also tended to obscure and mystify many of the strongly racial and class-based underpinnings of the new science. Differences in race and class were routinely euphemized not only by a notion of what it meant to "have culture," or to lack it, but also through geographic, topographic or demographic juxtapositions: to come from the Sierra carried a very different meaning than to be of the coast both in terms of race, and class locations. The very notions of development and underdevelopment, and the "cultures" in which they originated or the "civilizations" to which these evolutionary conditions properly belonged, concealed more than revealed a society stratified according to constructed racial and class-based identities. It is upon this world of differences that the RF acted asymmetrically, building scientific identities to suit a foreign scientific agenda that, as Bruce Jennings observes, were powerfully structured to favor the dominant groups within and between societies.

This report follows what might be called an "itinerary of silencing" that suppressed, ignored and disengaged local voices in order to allow an apparently neutral scientific method to gain ground and function as an authoritative narrative.<sup>8</sup> But more than that, it is about the irony of the RF discourse of Latin American "others," an "otherness" that could be ameliorated by mimicry but never dissolved.<sup>9</sup> And so I go on to ask how Latin American science was construed in the manner of a grand continental deficiency, and how the RF sought to redeem some members of that ailing "scientific" community.

The literature to date on Rockefeller agricultural science in Latin America tends to focus on the Mexican Agricultural Program (henceforth MAP), although many of the arguments advanced in connection with the Mexican experience are equally pertinent to the RF's endeavors in Latin America generally. Bruce Jennings has described the way RF initiatives in Mexico not only transformed agricultural and social relations but also inaugurated a new "political authority" based on top-down expertise. He has argued that through a singular focus on the objects of knowledge -- seed, plants, pathogens and yields among others -- the focus both "at home" and abroad was consistently diverted from social and political phenomena toward supposedly neutral objects of knowledge.<sup>10</sup> Accordingly, the new agricultural terrain was both depoliticized and immersed within a new authoritative politics of expertise which restricted choice, excluded other possibilities from discussion while leading a new public agenda which was to profoundly restructure agro-social relations in ways propitious for capitalism and the consolidation of U.S. interests abroad. As Jennings notes, "an especially potent instrument in this process was the creation of a professional indigenous force, trained to emulate the standards and practices devised by foundation personnel."<sup>11</sup>

In the foundation's endeavors to produce a copy of North African scientific agriculture in the South, increasingly in the name of helping the immiserated out of their dire poverty, Jennings maintains that "instead of aiding societies needy, it provided a means for the permanent elimination of alternative social relations."<sup>12</sup> Yet in this report I will make problematic what Jennings and others are inclined to simplify as a familiar case of dominant interests enacting a politics of inclusion and exclusion. I will focus on how the RF tried to fashion the subject identities of Latin American scientists such that an "emulation of standards" could be realized. The principal RF

strategy involved defining and limiting the parameters of what could rightly be considered “scientific.”

Accordingly, it would appear that subjects were able to be constrained and disciplined by this discourse, or they were not. As in other colonial and postcolonial encounters, it is the heterogeneous outcomes of RF intervention that has led to scholarly debate. Why was the reception and success of the RF programs so variable? Deborah Fitzgerald has argued for Mexico that some sectors of society were more similar in structure to that of the United States and in this case producing a copy of the land-grant pattern was bound to be more successful; yet other sectors were too different, and here the MAP faced difficulties in gaining acceptance.<sup>13</sup> At these times, the words of Joseph Cotter, the RF “could not bridge the gap between cultures.”<sup>14</sup> And we might say the same of scientists: some were too different, the structures that housed them too disparate; but some were similar enough to pull it off, to mimic their northern brothers with conviction.

While these essays may fruitfully (or perhaps not) lead us to wonder about what was, essentially, the stuff of the United States and the stuff of Latin America, through which we may follow, rationalize and explain the “cross-cultural encounter,” and its “successes and failures,” they unfortunately rely somewhat heavily on the perspectives of the very RF; a North American vision of a discrepant South, able to be domesticated, incorporated, disciplined only to the degree permitted by the inherent degree of similitude. But in this report I intend to steer an offset path, upsetting that stable axiom by which the similarities and differences are assessed. Rather than assume pre-existing differences between cultures, between farming structures and production processes, between commercial and subsistence agriculture, I will focus on how difference was systematically constructed through a “discourse of others.” It was this discourse that acted as a normative and disciplining mechanism through which the practice of science and the direction of

agriculture and research could be contained within nominally “scientific” parameters. In order to gain a foothold in Latin America, then, the RF had to promote a vision not only of itself but of the lands which it was to correct; it had to build a problematic centered on Latin America -- the land, its people, agriculture, education and government. And yet it is a paradox that the RF had to not only create a “deficient other” but also had to thaw that frozen state of “otherness” in those persons, classes and places who manifested the potential to evolve beyond the confines of “themselves” to emulate American science, and eventually become mimics of American scientists so that yet another, more distant “other” could farm just as Americans did.

When RF officers began to gaze down upon Latin American agriculture and science, what they saw were myriad deficiencies and lacks which, in sum, were seen to be at odds with both the aims of the expanding international agricultural network and the “standards” established in the United States. In general, the evidence of insufficient agriculture could, quite simply, be found in low productivity: “When the yields of corn, wheat and other cereals and legumes are compared, the United States, Canada, and other modern nations outdistance the Latin American yields in almost every category.” Latin America was sorely in need of North American science for “the principal difference lies in the application of scientific knowledge to plan breeding and selection, crop cultivation and control of pests and related problems.”<sup>15</sup>

On this basis, the RF proceeded to build a discourse on Latin American science. Although the RF was wanting to model Latin American agriculture on its own agriculture, it was wont to compare the Latin context with that of Europe, where it had gained much of its experience in foreign lands. Indeed, much RF discourse in the 1940s was directed toward deciphering what Europe and Latin America had in common and what they did not. This was not merely a rhetorical device but a critical aspect of making sense of Latin America and providing a framework within

which “rational” intervention could be pursued. On every front, however, the differences outnumbered the similarities and in general, the Latins were seen to be poor copies of Europeans. If Latin America had mistakenly chosen to emulate Europe, rather than North America, it was not doing too well.<sup>16</sup>

This perceived inability of Latin Americans to copy the Europeans had a strange inevitability about it, for the entities considered European and Latin American respectively were interpreted not only according to hemispheric locations but also to qualitative preconceptions about what kinds of things properly belonged to which places. The best of Latin America could not be truly considered Latin American; they were, in a sense, *too* good to be Latin. Put otherwise, “place” as a geographic and spatial distinction could surrender to a concept of “place” as a location somewhere between pre-modernity and modernity (in that order) and, by implication, whites and “others” (also in that order). Few Latin American things *could* be good, and when those precious few presented themselves, they represented a sort of displaced “Europe” in their “European-ness” in an otherwise archetypal condition of backwardness, even degenerateness, both hispanicized and hybridized in the “other” New World.

This “fixity” in the meanings attributed to place can be well illustrated by some observations made by RF advisor Elvin C. Stakman in 1947 when he undertook a reconnaissance mission to thirteen South and Central American republics. His task was to investigate the quality of education in the natural sciences as well as that of agricultural research in leading experiment stations. During his circuit he visited Estanzuela, an experimental station in Uruguay near Colonia. By his estimation, it was “one of the best in South America.” Too good to be Latin, however, Stakman observed “it is a European station rather than a Latin American one.” Seen through imperial eyes, the science practiced there was a derivative of another national descent for “the

Director, Alberto Boerger, was originally German, and some of the best members of his staff have European rather than Latin American scientific traditions.” Stakman's distinctly racialized topography of a nationally and internationally demarcated science -- original versus the Latin rendition -- became all too clear when he spoke of Argentina. “Possibly because of the fact that so large a percentage of the population of Argentina is of relatively recent European origin,” he conceded, “it differs in many respects from typical Latin American countries. Many of the institutions seem quite modern. Agricultural research is on a modern conceptual basis.”<sup>17</sup> And so, Latin America was organized into a hierarchy, with nations like Argentina at the top of the scale and others, in particular the Andean countries, at the bottom.

The RF not only continued to construct a hierarchy of nations among Latin American republics on the basis of their supposed degree of departure from a superior Europe. Somewhat paradoxically, part of the very problem of Latin American education and science was considered to be an outcome of the proclivity among Latins to imitate the Europeans rather than the more developed North Americans. Indeed, the pronounced influence of Europe on Latin American higher education was seen to have given rise to a system which catered for a cultural elite resulting in “scholasticism.” Scholasticism was associated with what one informant saw as a “complete absence of scientific and analytical method.” Emphasizing the sorry outcome of academic practices which had been imported from France, he urged that the “casual lecture-examination method” be replaced by the North American seminar method.<sup>18</sup> Ultimately, the United States was considered to harbor the best in scientific and educational practice in the world, providing a metric by which even Europe was substandard.

When the discussion turned from the natural sciences to the application of scientific knowledge to agriculture, the United States was by its own estimation indisputably in the lead.

Even before the Green Revolution, the “efficiency” and “high productivity” of American agriculture discursively co-constructed the relative “inefficiency” and “low productivity” of Latin American agriculture. The root causes were persistently turned on the university system of Latin America and the social and cultural milieu in which they were placed. “In spite of the age of most Latin American universities,” concluded Harry Miller in 1944, “practically nowhere do large, well-developed. research centers in the pure sciences exist, from which ... even trickles of significant contributions to world knowledge could have come over a long period of time, or are coming today.” Rehearsing deficiencies in the Latin American social context, Miller maintained that “science is neither fully appreciated in Latin America nor properly supported financially ... And the need for science applied to agriculture is neither recognized nor supported adequately and uniformly on a national basis.”<sup>19</sup>

It was within this framework that the performance and situation of individual countries and their institutions were assessed. In Brazil, universities were “little better than high schools,” confirmed the Rockefeller trustees in mimicry of Stakman.<sup>20</sup> Reporting to Miller after a visit to Peru’s “La Molina” agricultural college where the bare essentials of science were lacking, Charles M. Rick of the Davis College of Agriculture in California admitted, “I shudder to think of the contrast between conditions there and here at Davis.”<sup>21</sup> “Ecuador is pathetic” was the verdict pronounced by Stakman after noting the absence of a faculty of agricultural sciences at the university in Quito, and “needs everything, including a stable government, modernization of the educational system, experimental fields, living wages for teachers and investigators, and a change in many viewpoints.” Visiting a rural training school for teachers in Ecuador’s Tambillo,

Stakman remarked, “most of the students are Indians. The spectacle of an Indian girl trying to explain a problem in algebra when it was evident that she had memorized without being capable of comprehending was a conspicuous example of misdirected effort.”<sup>22</sup>

In sum, the RF “held no rosy illusions about the state of the sciences in the southern half of *our hemisphere*” [emphasis added] and recognized that “basic research there was still in the embryonic stage.”<sup>23</sup> It seems that the metaphor of the embryo, not unlike that of “development,” was well suited to elaborating the deficiencies while recovering a sense of optimism in a potential that could evolve given favorable intervention. Chile was both “tragic” and “promising.” And “Guatemala has some gratifying features,” Stakman decided in 1947 after meeting with the Secretary and Subsecretary of Agriculture, “both of whom have good concepts and considerable vision and realize the need for improvement, [which] is refreshing.” Honduras, he thought “had a model vocational school of agriculture” supported by the United Fruit Company.<sup>24</sup> And a few years later, Stakman's assessment of “La Molina” agricultural school in Peru was favorable within a continent he had habitually come to see as largely disappointing. Having met some of its students in Cusco who “complained, not altogether facetiously, that it compels them to work too hard,” Stakman gave the school its due credit: “This is an unusual and refreshing criticism about a Latin American agricultural school.”<sup>25</sup>

How was Latin American science, then, to be modernized? In a Rockefeller staff conference of 1941, it was suggested that “any program would have to begin on a lower level than in Europe, although one member hastily added that “most people have under-estimated the capacity of the better class of Mexicans.”<sup>26</sup> Of course, from Mexico to Argentina, it was the “better class” that was singled out everywhere. It required no elaboration that potential lay with this elite group who were unfortunate to be surrounded by an ocean of mestizos and Indians, who represented a

sort of cultural vacuum as far as science was concerned. Thanks to this elite, Latin America was not a complete void; or as Warren Weaver, director of the Natural Sciences Division put it, “we have very little to start with, but I am confident we have something more than zero.”<sup>27</sup> But nor were the possibilities striking when viewed against Europe. Where in Europe “there were a dozen or more first-rate research institutions, many of them headed by Nobel Laureates or equally distinguished scientists, and the task was to choose among this wealth of opportunities,” in Latin America “the task has been to find the rare institution in which any original research at all is going on.”<sup>28</sup>

As had been the RF custom ever since the philanthropy's inception, the significance of the good institution was surpassed only by that of “good men,” Good men were as much men with class and character as they were good scientists or “intellectual potentialities.” Wilson Popenoe, Director of the Panamerican Agricultural School of Agriculture in Honduras, reporting back to Miller on a visit to Peru's renowned agricultural college, “La Molina,” attested that the director there, Alberto León, was “absolutely tops” and “a hustler”: León's “philosophy on the subject of agricultural education fairly bowled me over.” For their part, the professors were “pretty sound citizens” as well as “mature men with plenty of experience.”<sup>29</sup> It was these few which could apply for entry into the international community of scientists as “citizens of science” it was these few of character and sound moral caliber who could be trusted with Rockefeller support.

But trustworthiness always rested on the razor's edge with it's opposite. Commenting on the “disappointing” results of the Mexican Corn Commission in 1949, Stakman queried about its members: “Whether they will attempt to profit duly from their position and whether they will try to dominate virtually all agricultural work in order to benefit personally remains to be seen.” Mexicans, Stakman observed, were likely to “expect too much in the way of emoluments and special privileges.” Their tendency to “mix unduly their professional and personal undertakings and

their unreasonable expectations with respect to financial help must tax the patience and diplomatic skill of even the most altruistically minded individual.” Even among students, “the attitude of ‘what can I get’ instead of ‘what can I give’ is deplorably prevalent.” Forever suspicious of scientists in Peru, Stakman thought that “as in many other Latin American countries, [there is] the tendency of ... scientists to gamble for favor with political elements and to make unjustified claims of accomplishment to obtain position.”<sup>30</sup>

Yet amidst a seemingly endless array of deficiencies, why did the RF not give up in disgrace? The principal answer, explored further below, is that the construction of multiple deficiencies was crucial to legitimizing as well as implementing RF science. But another part of the answer lay in the land itself. The resources of this largely unexploited continent were vast. From Mesoamerica to the Amazon basin and the pampas of Argentina, Latin America was known for its wealth of natural resources. Besides extractables -- rubber, oil, hardwoods, tin, copper, silver, gold and labor among many others -- lay genes. The Andean region in particular became known for its wealth and diversity of tubers, notably the potato. Opportunely, resources were construed as the wealth of nation states with a notable absence of any discourse of local sovereignty of or rights over resources. Largely ignoring Indian populations, nations like Peru were often spoken of as undifferentiated entities, sorts of singular national persons, within whose borders resources were contained. The potato was likewise framed within national terms; and so in 1954 it was possible for the head of the MAP, George Harrar, to say to the Peruvian Minister of Agriculture, Jaime Miranda Sosa, “Peru probably gave the potato to the world and today Peru still represents the richest known area in potato types and genes.” Suggesting the need for “international assistance” and a role for the RF in “an intensive program of investigation on all aspects of the potato and its culture,” Harrar continued, “after making the original contribution to world food supplies, Peru has never taken the

next logical step, namely the development of this major resource for its own use and that of the rest of the world.”<sup>31</sup>

Where foreign money was at stake, “logic” often prevailed. It was a logic that directed funding to individual scientists over and above institutions. Foundation “experience in Europe and North America had shown that progress could be made if here and there ‘an exceptional man’ could be found. Once he was found, the thing to do was to back him to the utmost -- to give him training if he lacked it; to give him laboratory equipment, supplies, books and other facilities... .”<sup>32</sup> Yet the RF considered it a bad investment to indiscriminately shower Latins in funds, for this breed of men needed to be treated with caution. In view of the less than propitious characters of Latin Americans, Stakman’s advice was well heeded: “It is clear that when dealing with Latin American countries it is always wisest to establish policies before personalities become involved.”<sup>33</sup> Certainly, the RF was extremely cautious with its “investments,” whether these concerned fellowships for individual scientists or grants for equipment and supplies. Individual scientists came under considerable surveillance and if, as I show below, scientists could not be completely purged of their Latin-ness, they could at least be disciplined. But forever optimistic, as a note from Joseph Willits, director of the Social Sciences Division, to trustee Raymond Fosdick revealed in 1950, “good men do exist, and beginnings should be made around such men.”<sup>34</sup>

Cesar Vargas of Cuzco, the first Peruvian potato man to receive international support, came to the attention of the RF via Carl Sauer, a cultural geographer from Berkeley who had been asked to investigate the potentialities of people and places in Latin America. Sauer met Vargas in 1942 during an expedition of the southern Peruvian highlands. Of all the men Sauer met at the University of Cuzco, Vargas was the one who most impressed him. “The only botanist within a thousand miles of the Andes,” Sauer said of Vargas, “is a good prospect. The boy is bright, clean-cut, and looks

like a scholar to me.” Sauer took Vargas on a number of trips during which they “talked crops and Indian culture incessantly.” “He has the wit,” Sauer continued, “to see that this is the place to study potatoes, wild and domesticated. He is also bright enough to know that the potato is not only a taxonomic but a cultural item.”

Sauer himself was opposed to the direction of the MAP.<sup>35</sup> His objection to the “agriculturalist perspective” in Peru was similarly framed. Having visited a government experiment station near Cuzco “which is trying to raise more and better wheat,” Sauer objects, “The old story down here -- the ‘agriculturalist’ influence disregarding entirely the native culture ... ‘Indians raise potatoes, civilized people eat wheat bread.’” Hoping that Vargas might become a replica of himself, a self-professed cultural visionary, Sauer mused that it “wouldn’t take long to make [Vargas] into a good cultural geographer as well as a cytologist-taxonomist.”<sup>36</sup> Not that Vargas was perfect, thought Sauer. “Like so, many Latin Americans, he is also somewhat unaware as to the difficulty of arriving at generalizations” and would require assistance “to keep him from overstimulating himself as to conclusions.” Yet despite a few misgivings, Sauer deemed Vargas worthy of RF support and considered that “it might be a very salutary thing for the economy of the highland if he did thus develop.” Sauer implied he was unlike other Peruvians who “make me tired with the fashion in which they diddle around with wheat and sheep and pay almost no attention to the magnificent cultural acquisitions of their own.” Latin Americans, he went on “instead of working with what they have got” have “a quality of imitativeness that the best of [them] recognize as a widespread weakness.” For Sauer, Vargas redeemed himself through his passionate interest in potatoes.<sup>37</sup>

Vargas’ backyard potato passion preceded his acquaintance with Sauer by about five years. Yet how he chose to represent his work to the RF, from which he badly desired assistance, was in

all likelihood influenced by the “cultural” of Sauer's “geography.” This being the case, the young Vargas, incognizant of his error in allowing his own “quality of imitativeness” to mimic Sauer's concerns, petitioned the RF in 1944 to fund a trip to the Mexican experimental stations. In justifying his proposal, Vargas began by noting what might well have been acceptable to the RF: Mexico, he wrote, “as you know yourself has many problems similar to those in my own country, not only in agriculture but also in relation to the large indigenous population.”<sup>38</sup> But Vargas not only wanted to “attain a vision and learning experience of the new methods and plants being introduced,” his sights were also set on outcomes “concerning the social improvement of Indians.” In ignorance of RF sensitivities, Vargas believed it “worthwhile contemplating such problems to resolve the grave social and agricultural conditions of the Mexican and Peruvian Indians,” admitting finally that “my plan is greater and more ambitious than simply an investigation of potatoes, directed rather at confronting the agro-social problems facing our Peruvian Indians.”<sup>39</sup>

Vargas had to be disappointed by Miller's response, for the RF had always believed itself averse to the mixing of science and politics. Miller alerted Vargas to the fact that his division “could not support a proposal for work on what is generally called ‘the Indian problem.’” Miller went on to explain that this was “a most intricate matter, with many complications on the political side” and that the foundation's “interest is only in the scientific field.”<sup>40</sup> Of course for the RF, the persistent interference of politics into Latin American science was part of the very problem there, and it was precisely that fuzzy boundary that had to be clarified if something of proper American science was to evolve in the South. This is not to say that the RF considered its own actions to be apolitical. Indeed, the channeling of a North American land-grant-system of agricultural science to Latin America was an inherently political activity. Yet curiously, the resultant practice of science was, if properly realized, believed to be an activity independent of culture and politics.

Vargas' imperfections did not exclude him from consideration. Like other Latin American scientists that the foundation sponsored, Vargas simply had to be disciplined. And so he was. Vargas did some astounding back-talking, affirming his commitment to the objects of knowledge and no more. Less an apology than an attempt to remedy a misunderstanding between friends, Vargas tried to convince the RF that his plan “to go to Mexico was not exactly about investigating problems connected with political situations, which do not please me, neither in principle nor as a matter of scientific discipline.” His new commitment, he underlined, was to “study agricultural problems in relation to indigenous plants in Mexico, comparing these with those of Peru.”<sup>41</sup>

If not entirely persuaded that this had been a mere misunderstanding RF was at least satisfied with Vargas' much obliged expression of repentance. In 1948 he was sponsored to attend the Plant Breeders Conference, at which “he made an excellent impression on the RF staff.” With foundation support, the University of Cuzco subsequently provided a new biology laboratory for him, “to replace the primitive one which has served for some years.” And in 1949 he was awarded \$3000 for laboratory supplies and a two-month trip to Colombia to work with potato specialist Dr. J.G. Hawkes.<sup>42</sup> In such a way, the RF made a good scientist out of raw material which had been tainted by unscientific pretensions. A few years later, with an irony that would have surprised Sauer if no-one else, Vargas ended up in charge of wheat improvement for the Ministry of Agriculture.

For every scientist who came under Rockefeller surveillance, dozens were dismissed and innumerable others never even considered or heard of. Yet there was another most disconcerting category of Latins who did not qualify foundation support, but who remained an ongoing concern to the RF: these men were those scientists and administrators of high rank and position who, by RF accounts, did not deserve to be in such places. As “political appointees,” they lacked any “real” scientific credentials; and worse, they were true members of that deeply ingrained Latin talent for

mixing science with politics wherever they went. These men came both to personify bad science (or pseudo-science) as well as embody the kind of Latin ways that allowed politics and favor to eclipse merit and achievement. Mario Cabello was one such man. A potato farmer with a large hacienda of his own in the Sierra, Cabello rose to prominence as director of the Ministry of Agriculture's Experiment Station "La Molina." But the more the RF staff and advisors came to know this man, the less they liked him. Unlike Vargas, Cabello came to be seen as an incorrigible highlander. Indeed, the presence of Cabello at the Experiment Station came to be construed as part and parcel of the general problem of the institution as a place of scientific research which, in its turn, held back the progress of Peruvian science.

As a man of "limited technical background," Cabello's appointment at the Station came at a time when the RF was becoming preoccupied with both the potential of the potato as a world food crop and, not coincidentally, the political and organizational impediments within Peru to the development thereof. Unlike its neighbor, the agricultural school "La Molina" under the superb directorship of Alberto León (soon to direct the Ministry of Agriculture), the Experiment Station was seen to be languishing. Many of the problems were attributed to the presence of Cabello himself. Although he prided himself as an outstanding potato scientist, the RF routinely criticized the basis of his knowledge. When prestigious plant pathologist John Niederhauser visited him from Mexico in 1953, few positives could be found in Cabello's research. Niederhauser regretted Cabello's lack of tangible results in his field work. But in particular, he took issue with his "theories about potato production that were pets of his." Many of these, thought Niederhauser, "were wrong," based either on his personal observations when he used to be a potato grower or were simply heresay.<sup>43</sup> In general, Niederhauser saw Cabello not as a man of knowledge but as a man of opinions whose knowledge was devoid of a sound evidential base. Niederhauser continued:

He told me that he was sure that seed potatoes, infected with virus after one planting on the coastal plain, could be regenerated and made healthy by returning this diseased seed to the sierra. His “proof” was that the farmers in the sierra had always produced clean seed, and hence by putting the diseased seed in this sanitary environment would make it healthy. Other unfortunate opinions he had were: 1) that small tubers, even though with virus, made better seed potatoes than large ones, even though virus-free, and 2) as potatoes were rotated from one soil type to another the yields got better. In each case these opinions are based on some observations, but betrayed a lack of understanding of scientific method and a susceptibility to being convinced by misleading correlations or coincidences.<sup>44</sup>

Without question, Cabello's practice of science was a hybrid of existing Andean farming practices and experimental scientific agriculture. For the RF, it was too hybrid; it compromised the single scientific method that the RF was able to accommodate. And if potato varieties -- and ultimately their diffusion -- developed by a scientific breeder were seen as a measure of a man's worth, Cabello's *Maco*, a variety that by the creator's own estimation was “one of the best,” “has not been widely accepted.”<sup>45</sup>

For the RF and a number of his contemporaries, Cabello became a laughing stock, a man who failed to listen “in spite of explanations.” His science was never rewarded. The RF had to make do with the unfortunate mix of politics in what should have been “autonomous science.” Soon, Cabello’s claims were routinely regarded or ignored. When Miller paid him “a diplomatically necessary courtesy call” in 1953, “Cabello tried to show all of his data of potato varieties which he has bred and which he claims are resistant to Phytophthora. Apparently no one else, including Carlos Ochoa, believes him.”<sup>46</sup> In 1958, Cabello lost his office. Yet when he retreated from science -- or politics -- to those culturally retrograde but naturally rich highlands where his “opinions” had been spawned, others, inevitably, rose to take his place as exemplars of “non-scientific practice” against which exemplary scientists and impeccable scientific practice were compared. Of course in Peru’s early years of initiation into foundation science, there were by definition few, if any, agricultural scientists who made the grade and could provide a local metric.

Carlos Ochoa, already noted for his skepticism of Cabello's self-aggrandizing claims, was perhaps the first Peruvian agricultural scientist to be distinguished as a good copy of a North American model scientist.

In 1946, Ochoa was employed by the Ministry of Agriculture and later took charge of its Plant Breeding Project at the Mantaro Experiment Station near Huancayo in the Sierra, some five hours drive to the east of Lima, at the time a hazardous route through precipitous terrain on bad roads (adversities that RF men made sure to include in their descriptions of “remote” and “other” places). During this time the RF learned of him and awarded him a grant to study under Hawkes in Colombia, as Vargas had done one year before. If the RF had preoccupations about Vargas' politics, it was Ochoa's perceived eccentricities that raised foundation eyebrows. Described as a “lone wolf,” the RF was cautious of his reputation for doing things solo and in his own way, accepting short contracts and taking off on prolonged, exploratory trips. Yet his peculiar ways and his pioneering spirit also won him some tentative admiration and soon, any reservations held of him dissolved.

Returning from Colombia, Ochoa requested support to travel to the northern Sierra to collect samples of “wild potato.”<sup>47</sup> Mixing Science and politics in a way that avoided the charge of doing so, Ochoa mimicked the RFs assertions about the potential of the potato as a world food crop, stressing the imminent gains of this unexplored region. Strategically, Ochoa also incorporated Soviet/American rivalry into his petitions for foundation support by making repeated references to the collecting expeditions of the Russian geneticist, Nikolai Vavilov, who had visited central Peru. Ochoa promised he could do better than the eastern Europeans. In a bid to solve the potato's plight of blight and frost “the two principal enemies of potato cultivators,” Ochoa proposed he collect genetic material where “no other men with such a purpose had explored before.” This was the

northern part of the territory “which still represented a great gap and a question mark for which the scientific community specializing in this material awaits and expects clarification.”<sup>48</sup> For its part, the RF was partial to such representations of discovery in virgin territory and the import of these not to Indians but to advances in knowledge among the community of world scientists.

Ochoa's gifts were not simply taxonomic in the narrow sense. He was clever in defining both himself and his science in a manner that would readily gain the RF's approval. Careful to avoid any unsavory references to the “Indian problem,” he approached the RF with a “purely scientific” attitude allowing his data to speak for itself -- and for him. And so, in a curious reversal, the RF found the confidence in Ochoa to mimic him in tongue and word: Haffar, referring to Ochoa's exploits in the northern Sierra, remarked with praise that “he has already found ‘*algunas especies nuevas para la ciencia*’ [some new species for science].” Ochoa was also singled out as an honest man. “His accounting for funds was unusually well prepared,” noted Haffar.<sup>49</sup> And Ochoa had the foresight to copy the RF's most poignant critiques of Latin American science. Said Niederhauser in 1953, “Ochoa is bitter about the politics that go into the advancement of the agricultural services in Peru” and, with a slight on Cabello, he remarked, “he has inspired some awe, and perhaps fear, on the part of lesser men who have become his superiors.”<sup>50</sup> Gradually, Latin Americans themselves were becoming the authors of their own backwardness.

The RF also took pleasure in the fact that Ochoa would characteristically underestimate his own achievements while allowing others to see his merits. By the early 1950s, Ochoa had established his credentials, not only as a good scientist but one among otherwise disreputable Latins. “His prestige,” Niederhauser proclaimed in 1953, “is not the empty sort that is often found in Latin America, but is the result of the real contributions that Ochoa has made while working under extremely trying and adverse conditions.” Suggesting Ochoa as “the logical person with

whom to start a local staff for a potato improvement program in Peru,” Niederhauser went on to observe that Ochoa was worthy of more support for his honesty, dedication and prestige within potato science.<sup>51</sup>

If Cabello's engineered hybrids were destined to become little more than unyielding deposits in the gene bank soon to be constructed, Ochoa's hybrids received their due and came to reflect on the excellent science of the potato man himself. “Ochoa has a new variety, Jiruco, that is more resistant to blight and higher-yielding,” said Niederhauser.<sup>52</sup> But in the Andes, “the main problem is not late blight, but frost.” Ochoa, responding to the call, “is breeding for frost resistance, and has some excellent sources of resistance in the native species *Solarium acaule*, *S. curtilobum*, and *S. jucepzuki*.” Ochoa, moreover, was seen to be prolific as well as effective. At the Mantaro Experiment Station, his combined propagation of new hybrids and native varieties exceeded “1200 numbers, including 300 different clones and 12 different species.” Suitably impressed, Niederhauser rated his collection “the largest in South America, and certainly the largest collection of cultivated Andean varieties that exists anywhere.”<sup>53</sup> During his career, Ochoa developed over 80 hybrids. Even Ochoa's interdisciplinary ambitions which led him, worryingly, “to want to take on too much,” turned out for the better.

It may seem not only incongruous but also a poorly disguised imitation of the RF's own discourse, to pursue too intently the idea, as espoused by historians such as Fitzgerald and Cotter, that the degree of social and cultural difference between the United States and Latin America determined how “successful” RF intervention was destined to be: could the gap be bridged or was it too wide? Looked at differently, the gap was perhaps less a pre-existing one than one that was shaped through intervention, through the discursive delineation of difference. The RF drove a wedge between the science of its own “America” and “the other America” and proceeded to supply

the moral and material resources to those dubitable Latins so they could aspire to narrow the gap between the two and thus become more American. For embryonic scientists like Ochoa and Vargas, the RF provided foster care, an avenue out of a world that had come to be defined as a rather unfortunate place to be; Cabello was aborted and joined the likes of innumerable others who proved themselves too, hybrid, too political, too Latin.

To pursue the construction of subject identities consistent with its cause, the RF had to build new stereotypes of the Latin scientist with his array of debilitating traits; they had to organize a vision of a deficient institutional, political and agricultural reality that engendered an environment in which only “bad science” could be produced; and against a backdrop of a virtuous North American science, Latins were less, and seldom more, successful in measuring up. By virtue of such a framework of Latin American deficiencies and a constant, although often implicit, assumption of the ideal character of American science and the agricultural system in place there, scientists “on the periphery” were scrutinized and disciplined. To “succeed” by RF standards, they had to go some distance in dissolving that “otherness” that had so meticulously been woven into their identities by a foreign philanthropic power with foreign standards, standards which, at times, came to look convincingly like local productions. Embryonic scientists, then, identified themselves through a discourse in which their very Latin identities were problematic; and with greater or lesser degrees of “success” they sought to distinguish themselves from themselves, from those now “others” who were too Latin. Many “failed” to meet the challenge, while a few, like Ochoa, became mimics of American science and, therefore, first-rate scientists. But really first-rate? At least by Latin American standards!

## **Notes**

<sup>1</sup> G. Bohrstedt, R. Bradfield, P.C. Manglesdorf, E.C. Stakman, E.C. Young and J.G.Harrar, “Notes on South American Agriculture,” Rockefeller Foundation Archives (hereafter RFA), R.G. 1.2,

Series 300, Box 13, Folder 105, Rockefeller Archive Center (hereafter RAC), Sleepy Hollow, New York, 1952, pp.7, 29, 29.

<sup>2</sup> The quote comes from Benjamín Quijandría, Director of Peru's PCEA (Programa Cooperativo de Experimentación Agropecuaria) in a report to the RF "La selección de variedades y mejoramiento de métodos culturales de la papa en el Perú," RFA, R.G. 1.2, Series 331, Box 6, Folder 60, 1958, RAC.

<sup>3</sup> Foetal and other organic metaphors were common in the foundation's discourse of the 1940s, although by the 1950s these tended to be replaced by similarly constructed notions of "underdevelopment."

<sup>4</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>5</sup> Excerpt from Trustees' Bulletin, March 1948, "The Natural Sciences in South America," RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC.

<sup>6</sup> These agricultural colleges in the United States developed over a century through a series of legislative acts which allowed for federal funding to establish a college in each state and for these to expand and diversify through provisions for experiment stations, research, and extension capabilities. These developments were enclosed within a farming establishment which was progressively becoming middle class, commercialized and professionalized. See Fitzgerald, 1994:75-6.

<sup>7</sup> This deployment of "culture" was one that naturalized perceived differences in degree of civilization, subjecting all to the one continuum. Although it may have posed as the modern anthropological version of "culture," this was strictly anathema to the RF's operative framework. This point is borne out in Jennings, 1998:50-5, where the advice of Berkeley cultural geographer was routinely dismissed.

<sup>8</sup> Spivak, 1990:31.

<sup>9</sup> Bhabha, 1984.

<sup>10</sup> Jennings, 1998.

<sup>11</sup> Jennings, 1988:41.

<sup>12</sup> Jennings, 1988:7.

<sup>13</sup> Fitzgerald, 1994.

<sup>14</sup> Cotter, 1994:47.

<sup>15</sup> Excerpt from Trustees Bulletin, 1944. "The Natural Sciences in Latin America," RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC.

<sup>16</sup> This was a recurrent complaint of the Latin American university system which had been modeled on the French model.

<sup>17</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>18</sup> Irving Leonard to Henry Allen Moe of the John Simon Guggenheim Memorial Foundation, RFA, R.G. 1.1, Series 300, Box 1, Folder 1, RAC.

<sup>19</sup> H.M. Miller, "Report to the Inter-divisional committee on Latin America," RFA, R.G. 1.2, Series 300, Box 13, Folder 104, RAC.

<sup>20</sup> Excerpt from Trustees Report, 1953. "The Natural Sciences in Brazil," RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC.

<sup>21</sup> Rick to Miller, RFA, R.G. 1.1, Series 331, Box 4, Folder 32, RAC.

<sup>22</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>23</sup> Excerpt from Trustees Report, 1953. "The Natural Sciences in Brazil," RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC. Here, the trustees are citing an editorial from the Argentine newspaper *La Prensa*, Buenos Aires, 17/2/44.

<sup>24</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>25</sup> G. Bohrstedt, R. Bradfield, P.C. Mangelsdorf, E.C. Stakman, E.C. Young and J.G. Harrar, "Notes on South American Agriculture," RFA, R.G. 1.2, Series 300, Box 13, Folder 105, RAC, 1952.

<sup>26</sup> Staff Conference, 18/2/41, RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC.

<sup>27</sup> Weaver to Fosdick, 1941, RFA, R.G. 300, Box 13, Folder 103, RAC.

<sup>28</sup> Excerpt from Trustees Bulletin. "The Natural Sciences in Latin America," RFA, R.G. 1.2, Series 300, Box 13, Folder 103, RAC.

<sup>29</sup> Popenoe to Miller, 1951, RFA, R.G. 1.2, Series 331, Box 6, Folder 55, RAC.

<sup>30</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>31</sup> Interviews: J.G. Harrar, 19/10/54, RFA, R.G. 1.1, Series 331, Box 4, Folder 36, RAC.

<sup>32</sup> Harry M. Miller, "Agricultural Education in Central and South American Countries," 1941, RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>33</sup> E.C. Stakman, "Latin American Agricultural Institutions: Preliminary Report of Trip May 8 to July 14, 1947," RFA, R.G. 1.1, Series 300, Box 6, Folder 37, RAC.

<sup>34</sup> Willits to Fosdick, January 1950, RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>35</sup> See Jennings:56-9.

<sup>36</sup> Sauer to Willits, 24/3/42, RFA, R.G. 1.2, Series 331, Box 7, Folder 64, RAC.

<sup>37</sup> Ibid.

<sup>38</sup> All translations are my own.

<sup>39</sup> Vargas to Miller, 22/1/44, RFA, R.G. 1.2, Series 331, Box 7, Folder 64, RAC.

<sup>40</sup> Miller to Vargas, 8/3/44, RFA, R.G. 1.2, Series 331, Box 7, Folder 64, RAC.

<sup>41</sup> Vargas to Miller, 13/4/44, RFA, R.G. 1.2, Series 331, Box 7, Folder 64, RAC.

<sup>42</sup> "Grant in Aid to the National University of Cusco, Peru," 9/12/49, RFA, R.G. 1.2, Series 331, Box 7, Folder 64, RAC.

<sup>43</sup> John Niederhauser. "Report On Trip to Peru, March 2 - March 19, 1953," RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>44</sup> Ibid.

<sup>45</sup> John Niederhauser. "Report On Trip to Peru, March 2 - March 19, 1953," RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>46</sup> Harrar memo, 23/3/53, RFA, R.G. 1.1, Series 331, Box 4, Folder 36, RAC.

<sup>47</sup> Wellhausen to Harrar, 3/10/58, RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>48</sup> Carlos Ochoa, "Expedición Colectora de Papas Silvestres y Cultivadas al Norte del Perú," RFA, R.G. 1.1, Series 331, Box 4, Folder 36, 1951, RAC.

<sup>49</sup> Harrar Diary, 2/6/52, RFA, R.G. 1.1, Series 331, Box 4, Folder 36, RAC.

<sup>50</sup> John Niederhauser. "Report On Trip to Peru, March 2 - March 19, 1953," RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>51</sup> Ibid.

<sup>52</sup> John Niederhauser. "Report On Trip to Peru, March 2 - March 19,1953," RFA, R.G. 1.2, Series 331, Box 6, Folder 60, RAC.

<sup>53</sup> Ibid.

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