

**Regimes and Randomization:  
The Politics of Research in Contemporary Kenya**

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During the height of the Kenya African National Union (KANU) single party rule in the 1980s, political scientists working in Kenya routinely reported being denied permission to conduct research and were extremely constrained with respect to where they could go, who they could talk to, and what questions they could ask (Schatzberg 1986; Widner 1992). In part this is due to the content of their research questions. Western political scientists studying Kenya during the 1980s were typically interested in issues related to the operation of the KANU regime under both Daniel arap Moi and his predecessor, Jomo Kenyatta, the manner in which the regime stayed in power, and its effects of single party rule on economic outcomes (Bates 1984, 2005; Barkan and Holmquist 1989; Holmquist 1984; Mueller 1984; Widner 1992). However, the lack of freedom to conduct research was not limited to foreign interlopers. Kenyan academics were similarly limited with respect to what research projects they could undertake and what they could do with the findings (Kipkorir 2009). Journalists, civil society advocates, and opposition politicians often bore an even greater brunt of the government's scrutiny, frequently facing censorship, arbitrary detention, and exile for daring to criticize the ruling party (Throup and Hornsby 1998; wa Thiong'o 1985; Mutahi 1991; Ogola 2005; Waliaula 2010).

Following the reintroduction of multiparty politics in 1992, there was a gradual (but not unidirectional) improvement in the protection of civil rights and liberties (Throup and Hornsby 1998). By the time I first traveled to Kenya in 2006, the arbitrary detentions and censorship of the 1980s had largely become a memory and the country had experienced its first election in which the incumbent party lost an election and the opposition took office. This increase in freedoms and liberty coincided with the relaxation of restrictions on the ability of Kenyan and foreign academics to conduct research, which, along with a relative absence of the violence and instability that had plagued much of the region, led to Kenya becoming an attractive destination for field research.

In particular, throughout the late 1990s and early 2000s, three distinct bodies of Western-based social scientific field research blossomed in Kenya. The first was an interdisciplinary body of primarily UK-based area-studies research, which drew on Kenya's excellent archives and extensive interviews to construct detailed political and social histories of the colonial and post-colonial periods (Berman and Lonsdale 1992; Anderson 2005; Elkins 2005). The second was a body of research in anthropology, human ecology, and agricultural economics that combined detailed ethnography with a variety of quantitative methods to examine the dynamics of land use and property rights change, primarily in pastoralist communities in arid and semi-arid parts of the country (Lamprey and Reid 2004; Lesorogol 2003; Little 1996, 1998; Little et al. 2001; Mwangi 2005, 2007; Rutten 2002). The third area of research expansion was a set of randomized evaluations of policy interventions by development economists, primarily based in western Kenya (Duflo and Kremer 2003; Duflo, Kremer, and Robinson 2004; Duflo et al. 2006; Glewwe et al. 2004; Glewwe, Kremer, and Moulin 2009; Miguel and Gugerty 2005; Miguel and Kremer 2004). Although plenty of Political Science graduate students and researchers headed to the Kenya during this period along with the historians, anthropologists, and economists, no distinct body of research or community of scholars emerged focused on Kenya emerged in US-based political science.<sup>iii</sup>

When I began the process of formulating research questions and developing a research design during a pre-dissertation trip to Kenya in the summer of 2006, there was a relatively limited literature on Kenyan politics in mainstream political science and a corresponding absence of a cohesive community of political scientists working in the country. As a result, I assimilated into the social circles of each of the various interdisciplinary research programs that were ongoing in Kenya at the time, and was influenced by the theoretical concerns and methodological tools of each of these communities. In doing so, I drew on the similarities between this diverse set of research communities. First, all three communities took a broad view of institutions, providing tremendous insight into the role of social norms and community governance institutions in day-to-day life in rural Kenya. A second commonality between the research approaches of the historians, anthropologists, and economists was a focus on long-term,

contextual fieldwork and a focus on individual and community-level motivations, behavior, and outcomes. Finally, all three of these research communities emphasized forms of long-term collaboration between foreign researchers and Kenyan researchers, communities, and policymakers, creating an environment in which there are many opportunities for research to become part of public debate and the policy process.

These commitments fit well with the orientation of my own intellectual home in political science, which was the emerging community of faculty and graduate students at Yale focusing on the micro-level study of order, conflict, and violence (Kalyvas, Shapiro, and Masoud 2008; E. J. Wood 2003; Kalyvas 2006). My initial vision for my research was to bring the this literature's emphasis on the microfoundations of institutions and individual behavior to bear on a series of questions and puzzles within the literature on state-building and state weakness in Africa (Boone 2003; Herbst 2000; Jackson and Rosberg 1982). During my pre-dissertation trip, I honed these interests into a set of research questions through my interactions with Kenyan and foreign researchers, government officials and NGO workers, and Kenyan citizens in various parts of the country. As a result of this trip, my central research question became, "How do informal institutions such as kinship affect the ability of states to exercise political authority over populations within their territory?" and I planned to answer this question through a focused micro-comparative study of security and land-use in several pastoralist communities in the Laikipia region of North-Central Kenya.

At the same time, the diversity of social science research taking place in Kenya fit well with larger methodological currents taking place in political science, which had come to stress problem-centered research, methodological pluralism, and pragmatic interdisciplinary (Green and Shapiro 1996; King, Keohane, and Verba 1994; Laitin 2003; Brady and Collier 2004). In this light, the wide range of social scientific research underway in Kenya proved to be a fruitful source of inspiration and instruction about how to draw on multiple sources of data to obtain inferential leverage over my core research questions. In the end, my research design combined archival research and oral history, in-depth participant observation, and a randomized field experiment.

My aim in this paper is to draw reflexively on the fieldwork that I conducted in Laikipia (and elsewhere in Kenya) in 2006 and 2007 to discuss the relationship between politics and fieldwork. My central argument is that the politics of research in contemporary Kenya represents a paradoxical contradiction: although the liberalization of the Kenyan regime over the past 20 years has permitted the expansion of political science research in the country, engaging in the practice of research itself reveals myriad authoritarian currents that run strongly against tendencies to classify contemporary Kenya as a democracy.<sup>iv</sup>

In particular, I draw on experiences and insights from each of the methods that I used throughout my fieldwork to construct two analytic lenses: regimes and randomization. Taken together, these two lenses shed light on different types of anti-democratic, and in many cases anti-political currents that are revealed by and associated with the conduct of research in this context.<sup>v</sup> In addition to being a personal, subjective reflection on the practice of one instance of political science fieldwork in Kenya, I hope that these reflections contribute to broader debates within political science (and other social sciences) concerning research design and mixed methods (Lesorogol 2005; L. Paluck 2010; Brady and Collier 2004; Woolcock, Rao, and Bamberger 2010), the relationship between normative political theory, political participation, and institutions (Fung 2007; Wedeen 2003, 2007; Krohn-Hansen and Nustad 2005; Collier, Hidalgo, and Maciuceanu 2006; Gray 1977a), and the ethics and politics of field research (Maclean 2008; E. J. Wood 2006).

### **Regimes- Authoritarian Legacies and the Conduct of Social Science Fieldwork**

Although most academics, public commentators, and Kenyan *wananchi*<sup>vi</sup> would classify contemporary Kenya as a democracy, the practice of fieldwork in the country draws attention to the long history of non-democratic politics in the country and underscores the enduring impact of both British colonial rule and the post-colonial party-state.

### *British Colonial Rule- Administration, Boundaries, and Separation*

Recent historical research and archival evidence from Kenya indicates that the broad emphasis on “indirect rule” in discussions of British Colonial Rule obscures several important aspects of the institutional structure of the colonial state in Kenya. In particular, recent research has shown that many aspects of British rule in Kenya were in fact quite direct, and involved the creation of a substantial administrative apparatus in peripheral areas. Although the British colonial government in Kenya followed the broad imperial policy of delegating substantial aspects of local governance to chiefs (Meek and Lugard 1937), in Kenya these chiefs were incorporated in a much larger administrative bureaucracy, the Provincial Administration (Branch and Cheeseman 2006; Gertzel 1966).

The Provincial Administration was the portion of the executive branch of the colonial government that had jurisdiction over the African population of the colony, and was led by the Chief Native Commissioner (who was appointed by the colonial Governor). The local European representatives of the Provincial Administration, District Commissioners and District Officers, were responsible for recruiting and overseeing chiefs, as well as for coordinating security, justice, and development activities in the Native Reserves (Branch and Cheeseman 2006; Gertzel 1966). Although the total size of Kenya’s Provincial Administration was initially small (as in other British colonies), the size increased steadily throughout the 1940s and 1950s, in reaction to needs for increased taxation in the aftermath of the two World Wars, as well as the security needs related to the Mau Mau Emergency (Berman 1992; Branch and Cheeseman 2009).

There are a number of reasons why the Provincial Administration in Kenya employed a more direct form of rule than many other British colonial governments in Africa. First, in a large number of native reserves, the position of chief had little basis in local precolonial institutions, meaning that chiefs often had to call upon DCs to generate compliance with taxes, grazing plans, and other laws and policies imposed by the government. For instance, in the semi-nomadic Maa-speaking foraging and pastoralist communities in the Mukogodo area of Laikipia (my primary fieldwork site), there were

no chiefs in a pre-colonial period. In these communities, governance over issues of land use and security was carried out by groups of elders, meaning that governance by a single chief was largely imposed and invented by the British and had little legitimate basis in local institutions and practice.<sup>vii</sup>

Second, in the many areas in which settler ranches bordered Native Reserves, the provincial administration was intimately concerned with the maintenance of boundaries. Because the creation of Native Reserves was the basis for alienating African lands for commercial development by white settlers, the movement of African populations to many of these reserves rarely had widespread support within communities. In some cases, reserves were created by “voluntary” signing of treaties between leaders of a given community and British authorities; in other instances, military force was required to generate compliance with the creation of reserves. Given the generally weak legitimacy of the creation of Native Reserves, settlers frequently reported instances of unauthorized grazing and cattle theft on their lands to the Provincial Administration, and frequently requested additional resources to ensure the maintenance of these boundaries (Anderson 1986, 2005; Brantley 1981; Elkins 2005; Ellis 1976).

In addition, partitioning the African population into separate reserves for each tribe often meant that the British (often unknowingly) created and codified social and territorial boundaries in social systems that had previously been marked by substantial fluidity and permeability (Ranger 1997; Posner 2005). In Laikipia, this meant distinguishing between cattle-herding Maasai communities, who were to be moved to reserves in southern Kenya, and hunting/foraging Dorobo communities, who were to stay in the Mukogodo native area (Hughes 2006, 2005; Cronk 2002, 2004). Archival records indicate that this policy was based on an assumption that “Dorobo” was a tribal community that was completely separate from Maasai and Samburu pastoralists (Spencer 1959). As a result, British administrators believed that it was possible to distinguish indigenous Dorobo communities, who they believed deserved to be in the Mukogodo reserve, from Maasai and Samburu pastoralist families and clans, who colonial authorities believed were attempting to live illegally in the Mukogodo area (Cronk 2002).

As a result, a considerable amount of correspondence within the Provincial Administration regarding the Mukogodo area focused on distinguishing which of the five major communities living in the region were “true Dorobo”, and which were invaders who needed to be forcibly moved to other areas.

My own oral history interviews, supported by the ethnographic and historical research of other scholars, indicates that the Dorobo label was less a marker of a distinct social and political grouping, and was instead a more general descriptor of one specific set of subsistence strategies that were part of a larger multi-ethnic social grouping in the region encompassing the Laikipia Plateau that included pastoralists, hunter/foragers, and farmers (Cronk 1989; Jennings 2005a, 2005b; Galaty 1982). In this interpretation, there were dense social, economic, and political linkages between Maasai and Dorobo communities, with families and clans moving between pastoralism, hunting, and farming communities as part of a reaction to fluctuation in herd sizes due to war and epizootic diseases. As a result, British policies that were designed to separate these communities in to distinct tribes living on native reserves in disparate parts of the colony were very difficult to enforce. In the long run, British policies designed to protect and preserve “Dorobo” identity and practices may have had the unintended effect of encouraging cultural change and assimilation, to the extent that all five of the Maa-speaking communities in Mukogodo (now Laikipia North District), primarily identify as Maasai pastoralists (Cronk 2004; Hughes 2005).

Given these dual goals of managing chiefs and maintaining spatial and social boundaries, Provincial Administrators in Kenya often formally and informally turned to social science research to help classify, identify, and manage so-called “native” institutions and “customary” laws (Abel 1969; Humphrey 1947). In Kenya, as in many other British Colonies, the nascent discipline of anthropology provided colonial administrators with a set of concepts and purportedly objective research tools for classifying and categorizing the social, cultural, and political systems of subject peoples (Asad 2002; Berman 1996; Peters 1994; Pinkoski 2008; Stocking 1991). Colonial administrators frequently were aware of the findings of anthropological theses,



dissertations, and monographs based on research in Kenya, and in several cases, anthropologists were asked to conduct research on the behalf of the administration. In addition to drawing on the work of professional anthropologists, many provincial administrators (along with Missionaries, explorers, and settlers) engaged in amateur ethnography, drawing in part on the concepts and methods of the emerging discipline (Weule 1909; HRT 1904; Farler 1879).

However, one episode that I found documented in the colonial archives on Mukogodo indicates the use of anthropological research for the purpose of colonial administration did not always have the intended effects. In 1959, British anthropologist Paul Spencer (who was at the time conducting ethnography among Samburu communities) was sent to the Mukogodo area by the Provincial Administration to resolve a border dispute between two jurisdictions. The District Officer in charge of the Mukogodo area charged that the Ile Uaso (Ewaso) Dorobo were invaders from Samburu District and needed to be moved to back to that jurisdiction. The District Commissioner of the Samburu area asserted that, “the various sections inhabiting Mukogodo are, in fact, so intermingled that the Il Uaso can no longer reasonably be regarded as a separate entity” (Spencer 1959). At the end of his two week visit to Mukogodo, Spencer sided with the DC of Samburu, recommending (among other measures) that the Il Uaso not only be recognized as legitimate inhabitants of the area, but that they be given their own chief (Spencer 1959). The administrators in charge of the Mukogodo area rejected this suggestion, writing, “I do not consider their conduct as thieves deserves recognition and to encourage their existence here would be to admit defeat in moving them to Samburu” (Spencer 1959). This led to a continuation of the disagreement, and the issue was ultimately left in a stalemate and was unresolved at the time of independence.<sup>viii</sup>

Rather than being an artifact of an earlier era in Kenya’s history, the institutionalized relationship between race, power, and research that was established during the colonial period persists to this day and shapes the practice of fieldwork, politics, and development in important ways. Every single day that I spent in Kenya, I was reminded that my salient identity in that context was as a *mzungu*- the Swahili word

to denote a white person or European. Whether I was interacting with rural elders, university professors, government officials, or toddlers and schoolchildren, the color of my skin was of much greater initial importance than the fact that I was an American, a Christian, or a Student.<sup>xiii</sup> The representation of the label “*mzungu*” in Kenyan culture is not monolithic, and there are a wide variety of local variations due to the variety and fluid identities of white people who live, work, and visit a given city or region.

Although I noticed a variety of uses of the *mzungu* label during my time in Laikipia, the most salient use in the region is “**Rancher**”<sup>xiii</sup>. Given the fact that a large proportion of the European families that had settled in Laikipia in the first half of the 20<sup>th</sup> century kept their land and remained in the area after independence, they remain highly visible through out the region. Throughout the country, this community is known as “Kenyan Cowboys”, reflecting their close association with cattle ranching, a “frontier” mentality, and penchant for safari-wear and four-wheel drive land rovers (Cowboys born and bred in Kenya 2010). In Laikipia, ranchers still do engage in commercial beef production, but most have shifted their land use to wildlife conservation and tourism, turning the region into Kenya’s largest destination for safaris outside of the major national parks.

In Laikipia, the political salience of the rancher label stems from conflict between pastoralist communities and ranchers over access to land. For the last ten years, a variety of local civil society organizations have rallied communities under the banner of the reclaiming the traditional land rights of indigenous communities, using the term “Laikipia Maasai” to encompass the set of pre-colonial peoples who lived in the region. These organizations have advanced a variety of legal challenges in national courts and UN bodies demanding that the substantial properties owned by ranchers be redistributed to the communities living in the comparatively small group ranches on the territory of the former Mukogodo Native Area. This campaign briefly turned violent in 2004, when demonstrations demanding the return of lands led to invasions of private ranches by community members. During these invasions, one community Elder was shot and killed by police (Hughes 2005). Ranchers have pushed back by changing the terms of the

debate, arguing that Maasai cattle herding practices lead to overgrazing and land degradation, and threaten wildlife. In this context, the rancher community portray themselves as the most effective stewards of the environment and have started a number of ecotourism partnerships with the local communities, which support the attempt to reposition themselves as agents for development and poverty reduction for all residents of Laikipia.

Within the socio-political landscape of Laikipia an alternative *mzungu* identity label was attached to me: **Researcher**. This seems so intuitive that it hardly merits articulation; after all, I was in Kenya to do fieldwork. However, both the short-term and long-term history of research in Kenya and Laikipia ensured that this label was neither neutral nor straightforward. I was hardly the first researcher to work in this area. On the contrary, enough research projects have been conducted in the area that two distinct modalities of the researcher identity label exists, each with a set of expectations and associated power relationships.

The largest body of research in Laikipia is primarily on ecology and wildlife biology. Due to the fact that there are no public game parks in Laikipia, these wildlife researchers are largely identified with ranchers, leading to an implicit expectation that these researchers and ranchers drive similar kinds of cars, wear similar clothes, and frequent the same restaurants, bars, and shops in Nanyuki. Although this close association with the rancher community provides access to many research sites, as well as to social circles with familiar language, culture, and standards of living, researcher affiliation with ranchers can also close doors. Although many individual ranch owners (and researchers affiliated with these ranchers) maintain excellent relationships with local communities, others are viewed with distrust and distance. In addition, among some rancher-affiliated researchers, this social assimilation corresponds to an internalization of rancher opinions and positions regarding local politics and development.

A second set of *mzungu* researchers in Laikipia are closely affiliated with the network of Laikipia Maasai civil society groups and NGOs active in the region, including

those that are actively involved in the land rights campaign. Host groups provide researchers with an office in Nanyuki (the largest town in the region), translators and research assistants, and access to interviewees in the rural communities. In return, these organizations expect assistance in grant and report writing and expect that the findings of research will be useful to the organization's mission. Although this method of affiliation has the advantage of facilitating entry into local communities, this is also highly mediated by the group's leaders, to the extent that I have on multiple occasions heard group leaders refer to particular researchers as "our *mzungu*". Even when the group does not have an explicitly political agenda, this can deeply affect the long-term trajectory of the research project; researchers may end up talking only to community members from one village or clan and will often end up hiring the relatives and friends of the group leaders as their research assistants and enumerators. In addition, many of these local civil society groups have limited resources and staff of highly varying ability and integrity, which can lead to researchers being stranded with relatively little support and no idea to how to go about their research.

My initial affiliation in Laikipia was in fact with one of the local civil society organizations working on the land disputes. I was actually unaware of the dispute and their role in it until after I had already been identified on "their side". This choice was highly contingent- one of my Kenyan friends in Nairobi (himself a civil society activist from the Turkana communities in Northern Kenya) knew the NGO's chairman and put me in touch with him. Despite the fortuitous nature of this choice, it had a number of path-dependent effects on the trajectory of my research. First, my initial affiliation with this organization all but ensured my exclusion from rancher social networks and associated research opportunities. For instance, during my very first interview with the head of a local conservation organization (who is a rancher), he became visibly uncomfortable when I introduced myself and told him which organization was hosting me in the region, and went to great lengths to convince me that his organization is not a "rancher organization" but seeks to ensure sustainable development for all residents of Laikipia. Second, among community members, announcing my affiliation with my host group created a set of perceptions about what my research was about and expectations

about what it would accomplish. On multiple occasions, interviewees asked me “What is this research going to do to help us get our land back?”

Finally, despite the effect of my affiliation with this organization on rancher and community perceptions of me, the actual support that I received from the organization was limited, in part because the organization had recently fired its two core staff members on accusations of corruption and public drunkenness. Concurrent with this scandal, the organization’s chairperson was traveling in Europe and working on projects in other parts of the country. For all intents and purposes, I was on my own. In order to move forward with my research, I drew on the friendships that I had formed with several high-school educated Laikipia Maasai youths, who in turn introduced me to their friends in each of the villages in the region.

Without any systematic support from my purported host organization and rapidly diminishing research funds, I had to significantly modify the way that I conducted research on a day-to-day basis. I started to use the same means of transportation as my research assistants, which was a hodgepodge of walking, hitchhiking, and riding in the back of trucks transporting livestock. While in Mukogodo, I stayed in the homes of my friends’ parents and participated in both daily household activities, such as caring for livestock, and community ceremonies and celebrations, such as marriages and male circumcisions. As a result of this need to practice research in an unorthodox way, I attempted, with varying degrees of success, to conduct research under my own auspices and to forge an identity as a researcher that was distinct from both the “rancher researchers” and an “organization’s *mzungu*”.

### *The Kenyatta and Moi Regimes- Researchers and The Bureaucratic-Executive State*

While the institutions of the colonial state in Kenya continue to shape politics and research in the ways discussed above, they also exert an indirect influence in contemporary Kenya as a result of the ways in which they were both maintained and transformed during the post-colonial regimes of Jomo Kenyatta and Daniel arap Moi. Some of Kenya’s representatives in the independence negotiations attempted to dismantle

the centrally controlled Provincial Administration and increase the role of representative democracy at both the local and national levels through regional governments and an independent, bicameral legislature (Anderson 2005; Southall and G. Wood 1996; Gertzel 1970). However, shortly after independence, the Kenyatta regime took actions to overturn these reforms in favor of a renewed reliance on the Provincial Administration, in part as a counterweight against potential political opposition (Branch and Cheeseman 2006; Kipkorir 2009). These actions resulted in the creation of what Branch and Cheeseman have called a “Bureaucratic-Executive” state, which they argue was similar in many important ways to Bureaucratic-Authoritarian regimes in Latin America and elsewhere (Branch and Cheeseman 2006; O'Donnell 1988). In contrast to many other post-colonial African regimes, the Kenyatta government had access to an extensive administrative bureaucracy that deeply penetrated nearly every rural part of the country (Branch and Cheeseman 2006; Gertzel 1966).

By subsuming the Provincial Administration directly into the Office of the President, Kenyatta was able to tie the provincial administration's social purpose to his own personal legitimacy as a leader of the independence movement (Branch and Cheeseman 2006). During this period, the Office of the President was a much more important vehicle for the distribution of patronage and the cooptation of opposition than the one legal political party, KANU, which remained underdeveloped by comparison. Many scholars have argued that this changed throughout the 1980s, due to Daniel arap Moi's increased reliance on KANU as a tool of social and political control (Widner 1992). Although it is the case that party loyalty and membership became an increasingly important basis for appointments to the Provincial Administration, Moi also did little to dismantle the linkage between the Office of the President and the extensive network of rural offices and administrators. Throughout Moi's rule during both the Party-State era of the 1980s and the KANU-dominated multi-party era of the 1990s, the Provincial Administration continued to play an important role in both implementing rural development projects and ensuring that KANU stayed in power (Throup and Hornsby 1998; Fox 1988; Leonard 1991; Rutten, Mazrui, and Grignon 2001).

Examining the regulation of research in postcolonial Kenya provides a fascinating case study of how the Bureaucratic-Executive state operated in practice, and the ways in which this institutional structure has persisted even after the transfer of power from KANU to NARC in 2002 (Branch and Cheeseman 2006). During both the Kenyatta and Moi regimes, the Office of the President controlled the process of approving and regulating research by both Kenyans and foreigners. Applicants were required to first obtain a formal affiliation with a Kenyan university (or another approved research institute, such as the National Museums of Kenya). They then had to submit a detailed research permit application form to the National Council for Science and Technology (NCST), which operated directly under the Office of the President (Butler 2001). The process of approving permits was frequently quite slow, and in many cases permission was denied outright. Researchers were then expected to take both this permit and a letter from the Office of the President to the District Commissioner in charge of the area where they would be working, who would write them a letter of introduction that could be used when conducting research locally (Schatzberg 1986).

However, following these procedures was no guarantee that a researcher would be able to conduct a research project as planned. Schatzberg (1986) sheds particular light on this through his narration and analysis of a fieldwork trip to Kenya in the early 1980s that was ended abruptly by the intervention of the Provincial Administration. Although Schatzberg followed the required procedures and received what he thought was sufficient clearance to begin fieldwork on the study of authority relationships in the town of Eldoret, his research permit was revoked and he was asked to leave the country before he had even started to conduct research in earnest (Schatzberg 1986).

The arbitrary use of bureaucratic authority to regulate research during this period was not limited to research on topics related to politics. For instance, the ability of the Office of the President to grant and revoke research permissions is also at the root of a well-publicized archeological dispute regarding the discovery of what may be the oldest fossil remains of a human ancestor (Balter 2001; Butler 2001). In particular, the dispute centers on the question of whether British archaeologist Martin Pickford had valid

clearance to conduct archeological digs in particular sites in the Rift Valley. A competing team, led by Yale professor Andrew Hill argues that Pickford's discovery is illegitimate, due to the fact that Pickford has had his research permission revoked twice, once in the 1970s, and once in the 1990s (Balter 2001). In his own defense, Pickford alleges that in the first instance, his loss of research clearance was due to political interference, and that the second letter revoking his permit was a forgery (Butler 2001).

By the time that I arrived in Kenya to conduct fieldwork in 2006-2007, control over the process of approving and managing research permits had been formally moved from the Office of the President to the Ministry of Higher Education, Science, and Technology (MHEST) (Balter 2001). As the MHEST is headed by a Member of Parliament rather than an official from the Office of the President, this move ostensibly reflects the slowly shifting balance of power between the executive and the legislature that was expected to coincide with the end of Moi's presidency and the transfer of power to Mwai Kibaki.<sup>xiv</sup> Under this system, I easily obtained my research permit in February 2007, and experienced none of the delays, arbitrariness, or political influence that previous researchers had reported.

However, when I attempted to use my permit to visit all 7 rural Provincial Headquarters throughout the country for the purpose of accessing administrative records regarding state presence and law enforcement outcomes in rural areas, I came into direct contact with the enduring legacy of the Bureaucratic-Executive state. Following the instructions from my contact at the MHEST, I submitted my revised application, stating the type of administrative data that I wanted to collect and where I wanted to collect it. The revised permit was granted in the course of one afternoon, and I traveled to the first Provincial Headquarters on my list to start collecting the data. However, when I was there, the bureaucrat that I spoke with told me that my permit didn't matter and that he needed a letter. I called the head of the research permits office at the MHEST, who sent me a letter from his office authorizing my research. I returned to the Headquarters, and the bureaucrat turned me away once again, saying that letter was still insufficient. At the



same time, he would not provide any further information about what kind of authorization would be sufficient.

Confounded, I traveled to the next Provincial Headquarters on my list, hoping that my experience had simply been the result of the individual bureaucrat with whom I had interacted, rather than something systemic. Much to my dismay, my experience in the next Province was remarkably consistent with what had happened before. However, in that province, a Police administrator mentioned in passing that I wouldn't have any luck obtaining the data that I was requesting without a letter from the Permanent Secretary of the Ministry of Provincial Administration in the Office of the President. He gave me the name of the official to contact in Nairobi and sent me on my way. When I went to the Office of the President in Nairobi, I had very low expectations. Much to my surprise, I was given an appointment to the official that the Police Administrator had recommended. After explaining my research to him, this official wrote a letter for me to all Provincial Commissioners on behalf of the Permanent Secretary, summarizing my research project and detailing the specific files and data to which I was to be given access.

From that point onward, each visit to Provincial Headquarters (including my return trips to the first two that I had visited) went the same way. Upon presenting my letter from the Permanent Secretary to the senior administrator (usually the Provincial Commissioner or his/her Deputy), he/she would forward me to a junior Administrative Officer, who would set me up with a workspace (usually in their office), and would arrange for the relevant files to be brought to me so that I could enter them directly into my computer. I would spend the next day or two working closely with the Administrative Officers, chatting with them about their jobs, their families, and current events during tea breaks. Upon completing data entry, I would be given a letter from the Provincial Commissioner to District Commissioners that instructed them to assist me as necessary for any further data collection activities.

Comparing and contrasting my research permit experiences with those of researchers such as Schatzberg and Pickford who worked in Kenya in the 1970s and

1980s reveals both change and continuity in the underlying nature of the Kenyan state. On one hand, the extreme openness and willingness to accommodate and share current administrative documents with a foreign researcher stands in stark contrast to the environment of secrecy, suspicion, and xenophobia that characterized much of the Moi regime's approach to research by foreigners. On the other hand, other aspects of my experience indicate that Bureaucratic-Executive practices continue in contemporary Kenya, even in the face of formal institutional and legal change. In particular, the continued importance of the Office of the President in regulating research (especially related to politics and governance) and the continued key role of letters as the vessel for delegating authority within the hierarchy both indicate the persistence of the institutional framework that was the cornerstone of authoritarian rule during both the colonial and post-colonial periods.

### **Randomization- The Micropolitics of Quantitative Research**

As discussed above, my experiences collecting data on governance from archival, interview, and administrative sources illuminated aspects of the enduring power dynamics related to the core institutions of the colonial and post-colonial state. In addition, my work leading a randomized evaluation of a community-based waste management project in Laikipia revealed much more subtle dynamics related to the intersection of knowledge, identity, and power, both in the local context and in the international development/academia nexus more broadly (Ferguson 1990; Maclean 2008; Malan 1999; Sultana 2007; van der Riet 2008). In the two sections that follow, I discuss how various elements of power and positionality shaped my decision to conduct a field experiment and the actual implementation of the project on the ground.

### *Power Calculations- Discourse, Experimentation, and Action in Field Research*

Although a randomized experiment was not an initial part of my research design when I left the US to begin fieldwork in January 2007, I decided to design and implement a randomized experiment while I was in the field. This decision was a reaction both to with a number of ongoing intellectual currents in academic political science and development practice, as well as my initial research experiences in Laikipia.

During the time that I was completing my PhD coursework and preparing for my comprehensive exams (2004-2006), a growing body of research in political science was starting to utilize data from a variety of types of field experiments and quasi-experiments.<sup>xv</sup> Although much of the initial momentum for this movement towards experimental political science came from studies of American voting behavior based out of the Institution for Social and Policy Studies at Yale (Green and Gerber 2003), a growing number of graduate students and faculty working in a variety of countries around the world also started to design and implement randomized experiments (Wantchekon 2003; E. L. Paluck and Green 2009; Moehler 2010; Humphreys and Weinstein 2009; Habyarimana et al. 2007; Fearon, Humphreys, and Weinstein 2009). This expansion of experimental research from American politics to comparative politics was in part prompted by concurrent trends in development economics. Throughout the early 2000s, a variety of economists based at major American Universities (particularly MIT, Harvard, and Yale) published the results of a number of large-scale, high-profile evaluations of a variety of development policies and institutional reforms, in some cases placing these articles in top Political Science journals.<sup>xvi</sup>

The central methodological contribution of this diverse, interdisciplinary literature is the idea that the logic of experimentation—the random assignment of research subjects to treatment and control groups— can be taken out of the laboratory and used to rigorously identify causation in real-world social, economic, and political contexts (Duflo, Glennerster, and Kremer 2007; Humphreys and Weinstein 2009). The turn towards rigorous, empirical evaluation of development projects and policies has also started to transform the landscape of development policymaking. An increasing number of multilateral and bilateral donors are demanding that the monitoring and evaluation of development programs conform to the standards of best-practice experimental social science, and have been willing to provide substantial research funds to researchers who will design experiments to evaluate a given program (Legovini 2010).

Again, development economists have been at the forefront of this current form of evidence-based policy by stating organizations such as MIT's Poverty Action Lab (JPAL) and Innovations for Poverty Action (IPA), both of which were created with the purpose of using the findings of field experiments as the basis for policy advising and advocacy. One of the biggest victories of this research-policy nexus took place in Kenya, where a coalition of researchers and NGOs used the results of an evaluation of school-based distribution of deworming drugs to convince the Kenyan government to implement a nationwide policy of distributing free deworming drugs through public schools (Nava Ashraf, Shah, and Gordon 2010; Miguel and Kremer 2004).

These trends in academia and international development policy influenced my interest in designing and implementing a field experiment in a variety of ways. At a basic level, I found the move towards field experiments as a compelling and convincing way of understanding how the study of politics could in fact be scientific. In particular, the core metaphors in much of the discourse surrounding Field Experiments, including "Randomized Controlled Trials" and "Labs" served to equate this form of social science research with the practices common in biological and medical sciences. This discursive linkage resonated with my understanding of science that I had acquired throughout my general education, and which was reinforced through my graduate research methods courses. In particular, this set of images and metaphors served to emphasize that the core of research is causal inference based on the scientific method (King, Keohane, and Verba 1994). Due to the close discursive association of field experiments with the essence of conducting scientific research, it is no surprise that I (and possibly many others) internalized the norm that experimentation is "gold standard" of research design (Deaton; Rodrik 2008).

In addition, a number of major media outlets, including the New York Times, covered the research of several political scientists and economists who were conducting field experiments, such as Leonard Wantchekon, Esther Duflo, and Michael Kremer (Altman 2002; Browning 2002). These articles helped to create and reinforce a belief that the best way that academics could contribute meaningfully to politics and public

policy was by designing creative, contextual, and rigorous experiments that overturned conventional academic wisdom and one-size-fits-all policy prescriptions. This media coverage also signaled the broad legitimacy of experimental research among Western intellectual elites and policymakers, indicating that conducting an experiment could increase the likelihood of a research finding becoming part of the broader policy debate. Finally, hearing about graduate students and faculty members who designed evaluations as part of a partnership with prominent international organizations, NGOs, and government agencies helped to create the impression that conducting field experiments was a viable way to obtain research funding in increasingly competitive funding environments. Although some of these interpretations and influences may have been a result of my own idiosyncratic experiences, they also provide some preliminary indications of how the fusion of material and discursive forms of power have helped to create an international epistemic community of policy-oriented experimenters in Western academic institutions and international organizations (Barnett and Duvall 2005; Barnett and Finnemore 1999; Ruggie 1998).

Given the linkage between the rise of randomized experiments and shifting discourse about what constitutes legitimate and authoritative research, this movement towards experimental Political Science has of course not necessarily been received with enthusiasm throughout the entire discipline. Much of this pushback has come from many of the same practitioners of political theory and interpretive methods that formed the core of the movement in the discipline that argued against the dominance of quantitative methods and formal modeling throughout the 1990s (Laitin 2003; Maclean 2008). For instance, when I shared an early version of an idea for a field experiment with one qualitatively oriented Political Scientist, he/she responded: “I am very dubious of its value. Even if it is wildly successful in your terms, it seems to me that the social science value-added is pretty small... I am inclined always to try to observe action that would, as near as one can tell, have happened without your prompting or you as an observer.”<sup>xvii</sup>

Aside from revealing the intense contestation within political science about the value of field experiments, this response also raises a second issue that relates to my

decision to design and implement a field experiment. When I went to Kenya in 2007 to begin my main stint of field research, my research design was very much in line with what was suggested by this colleague- observing action that would have happened without my presence as a researcher. When I wrote my dissertation prospectus, I hoped to achieve this through a mixed-methods research design that combined participant-observation with in-depth oral history interviewing, archival research, administrative data, and household surveys.

However, shortly after I commenced fieldwork, I became increasingly convinced that such pure observation of action would be both next-to-impossible and undesirable, due to several aspects of how my position as a researcher interacted with the social and political context in Laikipia. This was in part due to some of the aspects of the intersection of race, research, and politics that I discussed above. At a basic level, given the local salience of the meanings associated with being both a *mzungu* and a *researcher*, my mere presence in rural villages was anything but inconspicuous, even after I had been living and working in the region for quite a long time.

In addition, my research activities themselves shaped and interacted with local power dynamics, because I needed to work with local youths in order to conduct interviews in Maa and complete household survey questionnaires. Both the prestige associated with working with a *mzungu* researcher, as well as the material benefits associated with being paid a salary influenced the local social status of the enumerators that worked with me, and in turn shaped how the members of the community viewed me and the research project. For example, during an initial survey exercise, the survey team started to refer to themselves as “the network of influence”. I initially interpreted this as a self-aware joke that was poking fun at the extent to which working as a research assistant was a coveted and reasonably high-status position within local communities.

The longer that I worked in the area, the more I came to understand some of the elements of truth that were underneath the surface of this joke. In particular, given the relatively small number of translators or enumerators that I could work with on a given

data collection activity, it was likely that individuals from various social groups would be excluded at a given point in time. At various points throughout the research, the presence or absence of women, youth, or individuals from locally prominent families on my research team was a constant source of comment and discussion that intersected with the substantive interviews on the history of governance in the Mukogodo area. As I discussed my research (and myself) with a broader cross-section of the community, I discovered that various facets of my identity- race, age, gender, nationality, religion, and class- were interpreted and understood in a wide variety of ways by community members, and revealed aspects of the local norms, practices, and politics surrounding each of these dimensions. Although I attempted to mitigate some of the major sources of inequity created by the material benefits of working on my project by actively getting to know and working with individuals from across a spectrum of gender, age, class, and family/clan, this still did not change the fact that my presence and research process consistently had an impact on local communities.

Furthermore, as I went forward with oral history interviews with men and women from across the Mukogodo area, I also became intensely aware that my mere ability to engage in research itself reflected and reinforced global North-South inequalities and power dynamics. This dynamic was summed up succinctly by one elderly man that I interviewed, who at the end of the interview asked me what my research would do for him, as he said he knew that I would go back to America and become a professor, while he would remain poor. One of my local research assistants put a different spin on this same idea one day while we were working on translating questionnaires. I told him what I was “interested” in understanding with that particular research exercise, and he responded by saying that foreign researchers were lucky that they could choose topics based on what “interests” us, whereas in his community every decision is decided by “need” rather than “interest”.

As a result of these experiences, I became increasingly convinced that unobtrusive observation and measurement was neither feasible nor desirable for me in this particular context. My emerging belief in the need for combining action with observation in my

fieldwork in Laikipia interacted with my prior interest in field experiments. As I started thinking about revising my research design, I did not see designing a field experiment as a step away from interpretive and ethnographic methods towards uncritical positivism. Rather, it seemed to me that engaging in the process of designing and implementing an experiment in the field bore remarkable similarities to a variety of praxis-oriented forms of ethnography and action research (Lewin 1946; Brown and Tandon 1983; Wacquant 2002, 2004, 2005). Put differently, my goal was to bridge the epistemological and ontological divides that typically separate experimental and ethnographic research by implementing a randomized field experiment that allowed me to estimate the effect of a given intervention, while at same time reflexively analyzing the process of implementing that experiment as a way to understand the way that institutions, culture, and power shape the practice of development and politics in Laikipia (Hoppe 1999).

Based on these goals, I started to spend much of my downtime in the field brainstorming with my Kenyan friends about activities and projects that we could initiate and whether we could design an experiment that linked those activities to the research project we were working on. Throughout our discussions, one possible project that came up consistently was community-based waste management. As we traveled throughout Laikipia conducting interviews, I was consistently struck by the fact that there appeared to be no effective system for removing and storing waste in small rural commercial centers, resulting in substantial piles of plastic and organic waste. When I asked my friends and other residents about public waste, they said that waste removal was the responsibility of the Laikipia County Council (the elected local government), but that the Council did very little in the way of organizing trash collection or providing basic infrastructure such as trash cans or dumpsites. I was also told that civil society organizations and ranchers occasionally donated trash cans or lead community clean-ups, but that trash cans were often stolen and littering continued, leading to new accumulations of trash shortly after a cleanup.

These local explanations for the persistence of public waste raised a research question that I saw as being tightly linked closely to the core questions of my dissertation



project: Does linking the monitoring and enforcement of a waste management project to community governance institutions more effective than working with the Provincial Administration, in terms of reducing public waste and reducing littering behavior? The end result of these initial brainstorming sessions was that we developed a plan for a waste management and anti-littering program. I then submitted a proposal for funding to conduct a field experiment evaluating this project to a grant program at Yale's ISPS devoted to supporting field experiments by graduate students. At the same time, my Kenyan friends and I jointly formed and registered a local NGO called the SAFI (Sanitation Activities Fostering Infrastructure) Project. We spent several months developing and testing outcome measures of public waste and littering behavior (while continuing to conduct interviews and collect data on other topics), and by late November 2007, we were ready to implement our field experiment (Sheely 2009).

#### *Control Groups- Field Experiments and Participatory Ethics*

The evaluation of the SAFI Project Waste Management Project is relatively unique among field experiments in the extent to which it was conceived and designed in the field as part of an act of joint collective action between a researcher and members of the local community. At the same time, examining the social processes involved with designing the experiment, implementing the treatment and control groups, and analyzing and using the data from the experiment reflect some of the inherent tensions between the practice of political science field experiments and approaches to field research that emphasize participation and collaboration between researchers and subject communities (Maclean 2008; Malan 1999; van der Riet 2008). These tensions can be briefly illustrated by a set of observations describing some of the power dynamics that I observed at each stage of designing and implementing the SAFI Project experiment.<sup>xviii</sup>

At the stage of designing the experiment, power dynamics between myself and my research team may have shaped both the articulation of the research questions and the design of the interventions. My description of the process of creating the SAFI project emphasized the close, collaborative friendship between my research team and me. Throughout the process of working together over the course of several years, we have

developed enough mutual trust and respect to debate and deliberate alternative ideas and to give and accept criticism from one another. At the same time, I am also intensely aware that our relationships were (and continue to be) shaped by various aspects of my position as a foreign researcher. Especially in the early phases of my fieldwork, I found that my team went out of their way to agree with and affirm my ideas and rarely challenged any of my suggestions.

As a result, it is possible that my team members failed to voice objections or disagreement while designing the experiment, for fear of losing the opportunity to continue working with the project (along with the associated income and social prestige). As a result, what I perceived as collaborative brainstorming about potential projects may have in reality been more of a one-sided process driven by my preoccupation with trash. Similarly, it is certainly the case that focusing the experimental treatments on monitoring and enforcement by state versus community institutions was driven by my own research interests. At the same time, at the encouragement of my research team, I also included a third treatment group, which focused only on collective action by civil society groups to remove public waste, rather than any kind of third party punishment for littering. Although I was skeptical about the effectiveness of this treatment (due in part to my prior experiences with civil society groups in Laikipia), the long-term effectiveness of this treatment (*vis-à-vis* the groups in which littering was punished by Provincial Administration Chiefs or community elders) turned out to be the strongest finding from the entire experiment (Sheely 2009).

The actual implementation of the randomly assigned treatment and control groups uncovered a different set of anti-participatory tendencies associated with the conduct of experimental research in this context. On one hand, all three of the treatment groups in the experiment incorporated elements that were developed in consultative focus groups with community members, and were based on other participatory development projects in the region. In addition, the implementation of the treatments involved community meetings and collective action to clean the village center, and a volunteer committee led the ongoing management of waste management program in each village.

On the other hand, a random number generator, rather than a participatory process, determined whether or not communities received the implementation of the waste program and which variant of the program they would receive. Moreover, a key element of obtaining compliance with the randomization was working directly with Provincial Administration chiefs as the first point of contact in each community and throughout the process of implementation. Thus, although much of the long-term success of the project as a development intervention may have been due to its participatory elements, much of its success as a randomized field experiment may have been due to some of the authoritarian elements in the design and implementation.

The process of analyzing the experimental data and using the results for policy advocacy reveals a final set of power dynamics that run somewhat counter to participatory ethics. The data collection for the SAFI Project experiment involved the long-term, regular collection of data on the amount of trash on the ground and the frequency of littering behavior by a set of dispersed, committed community facilitators in each treatment and control center. The idea underlying this method of organizing data collection was for these facilitators to also be a first point of contact for organizing community feedback meetings and translating the research results into a format that was both intelligible and useful to community members. In practice, the movement of data was largely unidirectional, from community facilitators, to the core SAFI Project team, to my computer. Although I have worked on making Stata graphs and writing up village-specific impact reports in my spare time, the process of making the analysis and use of experimental data a collaborative act between myself and the communities was often subsumed to the demands of my professional and personal life once I was back in the United States.

Some of these observations may be specific to the idiosyncrasies of this particular case (the aforementioned politics of Laikipia, the personalities and positions of me and my research team). However, to the extent that the patterns in these examples are symptomatic of larger anti-participatory biases in experimental research, they raise a set

of difficult ethical and political questions for what experimentally inclined political scientists. One possible response, which might be advocated by some ethnographers, is to eschew field experiments altogether. However, this option is rather unsatisfying. Simply pointing out that there are problematic power dynamics involved in conducting field experiments is unlikely to undermine the legitimacy of the method among positivist political scientists, economists, and the broader international development community. Simply abandoning field experiments in favor of more traditional forms of participatory research may in fact limit the extent to which community-driven projects and research are viewed as legitimate and worthy of support by governments and donors, perpetuating a long-term bias towards top-down project design and evaluation.

As an alternative to abandoning field experiments, I am currently leading an ongoing research project in Laikipia, in which the SAFI Project staff and I are exploring the extent to which it is possible to develop a model of Participant-Driven Experimentation. The core idea of Participant-Driven Experimentation is to combine the core tools and normative commitments of existing forms participatory research methodologies with the best practices of experimental research. The central question of this project is whether the participatory incorporation of community members in research design, data collection, and analysis can improve the quality of data and enable communities to use research as an empowerment tool, assisting them in planning development activities and holding politicians and civil society organizations accountable for project performance. To the extent that the results are positive, we will use this experience to develop a set of methodological and technological tools that can be used by communities, researchers, and civil society organizations to implement their own Participant-Driven Experiments.

## **Conclusion**

In this paper, I have recounted various aspects of my fieldwork in Kenya, with a particular focus on macro- and micro-level politics, power, and institutions. Furthermore, I have attempted to structure and parse these vignettes using a variety of concepts and sources from the broader body of research on Kenya, to help move the discussion away

from purely self-indulgent navel-gazing and towards a more “rigorous subjectivity” that contributes to broader normative, methodological, and theoretical debates within Political Science (Maclean 2008).

One of these possible contributions is sparking a discussion reconsidering the power and purpose of multi-method research. In the introduction to *Designing Social Inquiry*, King, Keohane, and Verba emphasize the idea that “the content is the method”, succinctly encapsulating a paradigm in which the scientific method and causal inference constitute the core of Political Science research (King, Keohane, and Verba 1994). From the perspective of this paradigm, the primary value of multi-method research is its ability to provide additional leverage in making inferences by triangulating the strengths and weakness of different methods of data collection and analysis (Laitin 2003).

In this essay, I have attempted to turn this logic on its head by drawing on the experiences and findings of my own research experiences in Kenya. The core theme running throughout my discussion of both regimes and randomization in Kenya is that each of my research methods was itself a social and political activity, and that my act of engaging in each of these activities revealed unexpected and important aspects of contemporary Kenyan politics to me. This presents an alternative vision of the relationship between content and methods in Political Science research. Rather than simply borrowing methodological tools from other disciplines based on concerns of scientific inference, the potentially distinctive role of the Political Scientist is to reflect upon both the high-level findings of research and the deeper political factors revealed by the practice of research itself, and to use the dialogue between inference and interpretation to inform the practice of politics and policymaking (Gray 1977b; Maclean 2008; Hoppe 1999).

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<sup>iii</sup> This is in part because of the organization of subfields in American Political Science. Departments encourage graduate students to focus on Comparative Politics- even African Politics is often too specific. As a result, a combination of case-selection and practical imperatives spread new field research in Africa over a range of countries- Kenya, South Africa, Senegal, Zambia, and more recently Uganda, Rwanda, Liberia, and Sierra Leone. Of course, there is also a vibrant, and rapidly growing homegrown body of research on Kenyan Politics and Policy Studies, which is a vibrant subfield of Political Science in its own right, even if its formal linkages to American academia are sparse.

<sup>iv</sup> My approach here- using my research experiences to draw broader substantive inferences about political institutions in Kenya- is deeply indebted to Michael Schatzberg's description and analysis of his prematurely ended fieldwork in Kenya in the 1980s (Schatzberg 1986).

<sup>v</sup> On the concept of "anti-politics" and its application to the practice of rural development, see Ferguson (1990)

<sup>vi</sup> Swahili for "common citizen"

<sup>vii</sup> Throughout this paper, references to material from Laikipia/Mukogodo are based on my field notes, unless noted otherwise

<sup>viii</sup> The administrative and political aims of ethnography were not limited to using European scholars to document African communities. Berman (1996) notes that at least part of the stated aim of Malinowski's sponsorship of Jomo Kenyatta's anthropological research on the Kikuyu (which was later published as *Facing Mount Kenya*) was that training in social science methods would deradicalize Kenyatta (and other African intellectuals). Although it is the case that Kenyatta's period of study with Malinowski at the London School of Economics coincided with his turn away from radical intellectuals in the UK and the USSR, the depiction of Kikuyu society in *Facing Mount Kenya* did in the long run serve as a basis for imagining a larger Kikuyu community and as a platform for the political debates and activity that would become central in Kenya during the post-World War II period (Berman 1996; Kenyatta 1962).

<sup>xii</sup> As I discuss below, the "mzungu" identity label also intersected in interesting and occasionally in unexpected ways with my age, gender, and class background. For an excellent example of the salience of "Americanness" and "Christianity" over "whiteness" in Senegal in the early 2000s, see (Cogley 2008).

<sup>xiii</sup> Closely related mzungu identity labels are *British Soldiers*, due to the presence of a British Army training camp located outside of Nanyuki and *Tourists* who visit the area to see wildlife on the ranches and climb Mount Kenya

<sup>xiv</sup> The degree of change involved in this shift should not be overstated, as the president still retained discretion over the appointment and removal of Ministers.

<sup>xv</sup> Different social scientific disciplines follow different conventions for describing the same set of methods- "field experiments" is typical in Political Science, "randomized controlled trials" is used in Development Economics, and "Impact Evaluation" and "Randomized Evaluation" are common in Public Policy circles. In this paper, I shall primarily use "Field Experiment", but shall also use these terms interchangeably.

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<sup>xvi</sup> This body of research is too expansive to review exhaustively here. The most relevant papers include: Duflo, Kremer, and Robinson (2004); Duflo, Dupas, Kremer, and Sinei (2006); Miguel and Kremer (2004); Kremer and Miguel (2007); Glewwe, Kremer, and Moulin (2009); Miguel (2004); Blattman (2009); Duflo and Udry (2004); Pande and Udry (2006); Pande (2003); Gugerty and Kremer (2008); Ashraf, Karlan, and Yin (2006); Karlan (2005); Olken (2007).

<sup>xvii</sup> This scholar has made similar statements in public and I imagine would not shy away from being associated with this statement. However, I have kept this quote anonymous, as it was part of a personal communication.

<sup>xviii</sup> This discussion is organized following Maclean's discussion of how power shapes the conduct of each stage of field research (Maclean 2008). For the purposes of space, I have omitted a discussion of the power dynamics involved with obtaining Informed Consent in the SAFI Project Experiment. A full treatment of Informed Consent, along with a more in-depth discussion of the power and politics of each phase of conducting field experiments will be included in a forthcoming article-length discussion of these issues.