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Making up Mosquitoes: Gene Drives, Malaria, and Reproductive Transformation in the Field

Author
O'Connor, Anne

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Making up Mosquitoes: Gene Drives, Malaria, and Reproductive Transformation in the Field

By

ANNE O'CONNOR
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Approved:

Marisol de la Cadena

Mario Biagioli

Joe Dumit

Committee in Charge

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Abstract

This dissertation is based on five years of fieldwork with the UC Davis Vector Genetics Laboratory in Davis California. Following particularly their collaboration with a University of California consortium project led by UC Irvine, it is focused on the laboratory's contribution to the project of creating the first open-field trial of genetically modified organisms bearing gene drives, a genetic element which alters patterns of genetic inheritance. These organisms are a version of the *Anopheles gambiae* mosquito, which is the main African vector of malaria. That disease continues to kill nearly half a million people on that continent every year, most of them children. Even with the very recent introduction of a possible vaccine, with only about 30% effectiveness, the project is premised on the conclusion that existing prevention and treatment efforts are, and will continue to be, inadequate. The research team believes that this project, if it progresses to implementation on continental Africa, has the potential to eliminate malaria. The Vector Genetics Laboratory has been tasked with undertaking the fieldwork portion of this project, or bringing organisms and phenomena demonstrated in the laboratory to fruition in the world. This text highlights a simple point which has nevertheless been neglected in most conversations around gene drives and similar genetic engineering projects. The trial, like some others using similar technologies, depends on the manipulation of existing populations and their reproductive networks. This new mode of controlling living organisms is offered as a more elegant and effective way to change other living beings in the world. Rather than doing things to them, we have them do them to each other. Gene drives are perhaps the clearest example of this strategy, and it is widely recognized that this opens up new possibilities for the manipulation of living beings. I argue here, however, that this strategy also re-theorizes ideas of control in practice, and scrambles existing discourses about novelty and change through what it does to

sexual reproduction. While we tend to think of the new mosquito as the novel element of the equation, my fieldwork shows that it does not figure centrally in the planning of an open field trial. That is because it is not very likely to do a new or unexpected thing all on its own. The crux of this new technology is that it intervenes at the point of reproduction—the space where the researchers anticipate they may be surprised, where something unpredictable may happen, is at this sexual interface between laboratory mosquito and wild-type. Because they know a great deal about the former, but very little about the latter, it is in the wild half of this equation that attention is centered. This matters for the kinds of stories we are building around this and other projects using gene drives. Because this is likely to be the first use of the technology outside the laboratory, its outcome carries tremendous consequences for future uses, both planned and as-yet unimagined. I argue that implementation, far from being after-the-fact, is actually the main event. The project is not the mosquito; it is what the mosquito can do with the world. Investigations of mosquito worlds, thus, take on a new and greater significance: they are not the passive ground upon which change is enacted, but active doers catalyzed in a particular way by sex.

Contents

UCIMI Overview

This dissertation describes a project led by the UC Irvine Malaria Initiative (UCIMI). The UCIMI is headed by PI Anthony (Tony) James, a Distinguished Professor of Microbiology and Molecular Genetics at UC Irvine. Tony has been prominent in the field of mosquito genetic engineering since its inception in the 1980s. Valentino Gantz was Tony's postdoc at Irvine when he developed important techniques for the design of gene drive systems. Tony's laboratory in Irvine, in collaboration with Ethan Bier's, were involved with designing and cage testing the new mosquito at issue in this trial. Ethan Bier is a Distinguished Professor in Cell and Developmental Biology at UC San Diego. Also involved with laboratory work is George Dimopolulos, at Johns Hopkins Bloomberg School of Public Health Department of Molecular Microbiology and Immunology. This mosquito is a version of the *Anopheles gambiae* mosquito, the main African vector of malaria, which carries a genetic alteration making it refractory to the malaria parasite, and a gene drive which passes this trait on to all, rather than half, of its offspring. Mathematical modelling work for the project is conducted by John Marshall and members of his laboratory in the School of Public Health at UC Berkeley.

Participant observation fieldwork for this project was conducted primarily with the Vector Genetics Lab (VGL) at UC Davis beginning in 2017. Headed by PI Greg Lanzaro, the VGL is in charge of the Field Science portion of this trial, the details of which are discussed at length here. Greg is a Professor of Pathology, Microbiology and Immunology in the School of Veterinary Medicine. He holds a doctorate in entomology from the University of Florida, and his primary research focus is on vector genetics. His laboratory publishes work touching both on issues of basic evolutionary genetics and on empirical findings about the genetics of disease-bearing mosquito populations. Greg and his lab work closely with Anton Cornel, an experienced entomologist and a Professor in the Department of Entomology in the College of Agricultural and Environmental Sciences at UC Davis. Ana Kormos, also based in the Davis area, is the Community Engagement Manager for the UCIMI. The VGL currently has two postdocs: Melina Campos and Robert Ditter. Abram Estrada is the Insectary Manager, and Marc Crepeau and Hans Gripkey are staff researchers. Former researchers and postdocs who contributed to the work discussed here include Shaghayegh Soudi, Ilyas Rashid, Erik Blosser, Hanno Schmidt, Oscar Kirstein, Mark Hanemaaijer, Travis Collier, Will Sharpee, Yoosook Lee, Irka Bargielowski, Allison Weakley and Kendra Person.

Acronyms

VGL……………………………………..……………………….. Vector Genetics Laboratory

UCIMI …………………..…………………….University of California Irvine Malaria Initiative

GEM………………..…………………………………………… Genetically modified mosquito

SNP …………………………………………………………Single Nucleotide Polymorphism

CRISPR………………………………. Clustered regularly interspaced short palindromic repeats

DRA ……………………………………………………………………...Drive Resistant Allele

Introduction: What is Already There

Mosquitoes, sporadic newspaper headlines lines like to remind us, are the deadliest animals on earth. It is estimated that, of the approximately 108 billion people who have ever lived, mosquito-borne diseases have killed nearly half of them (Winegard 2019). Malaria is among the most devastating of these diseases, continuing to kill nearly half a million people every year, most of them children. The parasite, and by extension the mosquitoes which carry it, have changed the human genome (Kwiatkowski 2005). Humans, in turn, have played the primary role in shaping the species of mosquitoes which most typically spread diseases to us. They are primarily anthropophilic, meaning that they preferentially (or sometimes exclusively) feed on us. Their bodies and behaviors are shaped around humans. This dissertation is about one species of mosquito in particular called *Anopheles gambiae*. It is the main African vector of malaria, where the vast majority of cases of that illness occur today. As we will see, it becomes tricky at some level to delimit *gambiae* as a clearly defined taxonomic object. What is never in dispute, however, is its love for us. It does not live apart from human beings, and it has not for many thousands of years, since before it even was *Anopheles gambiae*. Humans and mosquitoes have shaped each other profoundly, not only our bodies and our genes, but our ways of life and our histories. Mosquitoes and the diseases they spread have played key roles in war, for example. They have shaped topologies of human habitation. Increasingly, they are articulated as major impediments to economic growth. Our mutual imbrication runs deep, dating to the beginnings of human agriculture according to most experts. It may be impossible to imagine *Anopheles gambiae* or other species of anthropophilic mosquitoes without people. We are, however, very capable of imagining ourselves without them.

This dissertation is about a project the aims of which are, in some ways, not very new. A group of Californian scientists, organized through the UC Irvine Malaria Initiative, would like to create a world without malaria. They believe that, unlike their many generations of ancestors pursuing the same goal, that they finally have the tools to do so. Those tools are products of genetic engineering. Specifically, a modification which makes *Anopheles gambiae* refractory to the parasite and a gene drive, which spreads this trait rapidly through populations. We will learn more about how these techniques work in subsequent chapters, but the real interest of this dissertation is in how these new techniques, and the frameworks within which scientists describe them to themselves, reframe ideas about novelty, emergence, and control. As we will see, scientists describe their approach as a form of genetic biocontrol. This is an approach to the management of nonhuman populations and ecologies which borrows ideas from an older paradigm called biological control, which emerged among Californian entomologists around the turn of the last century. The thread which connects these evolving discourses of control is the co-optation of drives and behaviors, rendered as natural, towards human ends. Genetic biocontrol brings this technique into the bodies and genes of the organisms, and works within a natural world which, from the molecular to the ecological level, is selfish, competitive, and not inevitably balanced. This approach differs from other attempts to reduce mosquito populations or disease transmission between us because it centers not insects or pathogens themselves, but relationships as tractable to intervention.

I have been conducting participant observation with the UC Davis Vector Genetics Laboratory, as well as with other laboratories and individuals participating in the UC Irvine Malaria Initiative (UCIMI) project since 2017. This work has consisted in attending weekly lab meetings, conducting interviews with participating scientists, and observing and participating in daily laboratory and insectary work. For the UCIMI trial, sex and reproduction are central. The refractory gene is spread to a next generation when a genetically edited mosquito mates with one which has not been edited—here called wild-types. The consequences of sex are also altered: when a mating like this occurs, the offspring produced do not inherit genetic material from each parent according to the normal rules of Mendelian inheritance. Instead of half receiving the refractory gene, as would be statistically normal, the gene drive ensures that nearly all of them do. This shifts sexual reproduction. It opens the processes, from copulation to genetic recombination, up to individual scrutiny and manipulation. From being a fact of life, mosquito sex is newly becoming a question. What can sex become? What can it make? The new perspective on sex catalyzed by how researchers develop and think about gene drives also has implications for long-established distinctions between laboratory and field. Coverage of this trial, both within and outside of it, tends to follow a familiar narrative which centers the engineered mosquito created at UC Irvine. According to this narrative, we are now beginning the phase of implementation, when the new mosquito is tested. This involves also the collection and processing of baseline data, a description of what is already there in a site before the intervention of releasing the mosquito. If its performance is verified, if the engineered genes spread through a wild-type population as they do through a caged population of laboratory colonized mosquitoes, the project can move on to a larger site and, eventually, to continental Africa. This mosquito, they say, holds the key to eliminating human malaria in continental Africa, where it continues to kill nearly half a million people every year, most of them children.

The project is thus conceived of as phased from small to large, from a cage to a continent. The work described here all occurred around an important mid-point: an island. Part of the fieldwork for this project was the identification of appropriate island sites to test the mosquito, and the establishment of infrastructure there for mosquito collection, scientific work, and bureaucratic, political, and public health collaboration. Relationships between the VGL and two countries are described here: Sao Tome and Principe and the Union of the Comoros. Lying off of the West and East coasts of Africa respectively, each country meets the UCIMI's physical and entomological requirements for a testing site, and representatives of both countries have been engaged in negotiations with the VGL about hosting a test release of the mosquito. Each site is thus framed as an intermediate level within the overall goal of the project to move from the laboratory to the world overall. Genetically isolated, biologically diverse, and topographically variegated, they are to be transformed by this work into experimental sites. The islands, however, do not remain passive locations for scientific work to take place. It is rather the islands themselves, and the mosquito populations which inhabit them, which actively create the project. The center of their work is not the invented mosquito, but what it can become with local mosquitoes which both hold and evade laboratory scrutiny.

This dissertation chronicles what the researchers describe as the implementation phase of this project, but it offers the suggestion that this narrative may not capture something else that is quite new about this work. The centrality of sexual reproduction does something, not only to the movement patterns of genes, but also to the way we narrate to ourselves sources of novelty in the world. After five years of fieldwork with the Vector Genetics Laboratory, it is clear that the engineered mosquito itself holds relatively little attention. Fully sequenced and partially designed, it is one of the few mosquitoes about researchers believe they know quite a good deal. This is not true of wild-type mosquitoes which, unlike the laboratory mosquitoes they work with in the insectary, are seen as reservoirs of strange and confounding genes, behaviors, and possibilities. Because the project emerges from the sexual interface between the two, it is in fact the multifarious plasticity of the wild-type mosquitoes which enters center stage as researchers introduce this project to the world. It is not what the mosquito does, but what the world does with it. As we will see, mosquito lives are a reflection of human worlds and they adapt and change in astoundingly creative ways to evade, harass, kill, and love us. It is this very changeability which is posed both as a threat to the project—because it may lead to the emergence of what are called Drive Resistant Alleles—and as its primary source of effectiveness. By sounding out the underlying logic of this strategy we re-focus attention not on the laboratory and the innovations of human minds that happen there, but on the creativities seen to be latent in the world itself, already glimmering behind the surface, potentially ready to become anything.

This trial is likely to be the first-time organisms bearing gene drives are released outside of the laboratory. Its trajectory thus has broad relevance for a number of other potential projects which aim to use gene drives to re-design ecologies to prevent disease or decrease invasive species' numbers. It may be that this first trial opens the door to many potential uses of the technology. This project has obvious practical significance for future biotechnology applications, but it also has a more diffuse conceptual influence. I make the case here that the researchers described here frame the field not as a site on which innovation is applied but as an active resource harboring change potential. In a world where the constant search for the new renders an increasing sense of effeteness, to re-locate change potential in the immanent situations already around us is to reinvest the world with the terrible promise of the radically new. To think futurity through a new biotechnological object or technique may then mean focusing not, or not only, on the object itself but on what it draws forth and changes. Implementation, in this story, is not an after-effect of innovation but the main event.

Driving Genes

People's ability to change the genes of other organisms has expanded significantly in recent years, in large part due to CRISPR, a technology which makes such editing much easier and more precise. Genetic engineering advances continue to garner a great deal of critical attention. Much of this discourse is focused on potential uses of these technologies in applications to human beings, and potentially to "humanity" (Greely 2021; Glaze-Crampes and Pruitt 2021; Mahoney, Probs., and 2018, n.d.; Goldman 2021; Kirksey 2021; Isaacson 2021). Genetic editing offers the promise to ameliorate or prevent genetic diseases. The line between disease prevention and optimization (most dramatically posed by fears about "designer babies"), however, is a thin one. Who gets to decide appropriate and inappropriate uses of such technologies? Will we have a future in which richer people can buy themselves physically superior children? Discussions about genetic engineering, especially as they relate to human germline editing, often focus conversations on the future of "the human race." Are we capable of changing what it means to be human? The births of twin girls Nana and Lulu in China carrying genetic modifications thought to increase resistance to HIV has catalyzed such debates. The scientist responsible, He Jiankui, has been universally condemned and reviled, currently serving a prison sentence for his actions. Much of the vigor with which He has been expelled from the scientific community reflects a prevailing anxiety about the potential impacts of public opinion on the future of genetic engineering and a sense of urgency to solidify norms ahead of the specter of regulation. The coalescence of these discourses around humanity and "the human race", however, elides the many other applications of these technologies which are likely to be more influential to our lives in the future. Genetic engineering is already widely used in plants, especially for the production of agricultural products. It is likely to become ever more so in respect to animals as well. The UCIMI trial is far from the only project aiming to use genetic engineering to control populations of non-domestic organisms, and the impacts of these technologies on environments and ecologies is likely to be profound. The changes that this may bring to human life is both more likely to come to pass than extensive genetic engineering of humans themselves (especially human germline editing, which overstates the extent to which there is a static human germline to be edited), and likely has the potential to be more influential to human lives and ecosystems.

One of the most important advances made possible by CRISPR are gene drives. Discussed at greater length in Chapter One, gene drives are a technique for manipulation of inheritance. At the level of populations, they offer the possibility to redesign species, "sculpting evolution" in the language of one of its designers. What if we could change not just individual organisms at will, but species themselves? How will species categories change in the face of this opening? Because gene drives change the rate at which genetic traits (engineered or not) spread through a population, their deployment is not only a question of individual bodies but of populations, species, and ecosystems as a whole. Both in extent and duration, gene drives change the frame of possibility for future forms of life. This dissertation addresses this speculative re-framing through an analysis of what is likely to be the first open release of gene drive bearing organisms outside of the laboratory. As we will see, this trial is often posed as a test case for the technique in general, either opening the floodgates or ruining it for everyone.

Gene drives bring genealogies to the fore, posing not the manipulation of bodies but of lineages as a possible entry point for remaking forms of life. Drew Endy, a synthetic biologist at Stanford, offers a taxonomy of contemporary genetic strategies as falling into either the camp of “lineage with intent” or “lineage agnostic” (Wei, Jackson-Smith, and Endy 2020). In an ongoing discussion group we are both part of, Drew poses his own work, which aims to construct life de novo from the ground up, as an example of the latter. Gene drive techniques are resolutely representative of the former. As we will see, they are based not on the idea of creating life, but of repurposing it. This distinction becomes crucially important to scientists’ theorization of their work, especially in reference to their ideas about wildness and nature. While the small field of genetic engineering of mosquitoes aimed in the 1980s at such “ground up” strategies of constructing mosquitoes from scratch, the field has been entirely captured by the certainty that such strategies are not only impossible but foolishly difficult. Mosquitoes, both in the laboratory and the field, are rather seen to hold both the potential for extensive manipulation and vast reserves of mystery. As we see in Chapter Two, much of this mystery is located in what is still “left” of the non-manmade parts of a mosquito, and in the un-edited wild-type mosquitoes with which they mate. We will see the ways in which these reserves are posed both as threat and resource, asking how their emergent framings contribute to new consensus on the natural world, its rhythms, and its potentials.

Real Aliens in the Field

Asking these questions through and with mosquitoes creates certain kind of effects. I will argue that pursuing this lineage with intent strategy involves the foregrounding and reconceptualization of relationships between mosquitoes. But what of our relationships with them? Mosquitoes are not easy creatures to feel about. They are neither cute (Suzuki 2015) nor charismatic (Jamie Lorimer 2007). They do not have faces, at least not the kind which we can encounter (Levinas and Nemo 1985). *Anopheles gambiae* is particularly tiny: measuring only about 3 millimeters, the disparity in scale between us makes most of our encounters not with the mosquitoes as individuals but as anonymous collectivities. It is in their numbers, and their swarms, that we meet them. Not as beings in groups, but as aerial flows, atmospheric disturbances, small threats by the ankles, in "their liminal position between animal and environment" (Beisel, Kelly, and Tousignant 2013b).

As one entomologist in the Vector Genetics Laboratory put it, mosquitoes are "the closest thing you can get to an alien." Thinking with these "fundamentally alien life forms" (Langlitz 2011) enacts a dynamic tension between radical alterity and connection. From the inception of modern science, insects have served as metaphors of the sociality of humans and nonhumans alike (Rodgers 2008; Clark 2009), with "queens", "workers", and "soldiers" acting out social roles rooted in the natural order of the world. Mosquitoes are also amenable to scientific inquiry because of their small size and rapid reproduction which poses them, especially those domesticated into model organisms in the laboratory, to serve as ideal "biological breeder reactors" through which to conduct a variety of scientific investigations, particularly in the realm of genetics (R. E. Kohler 1994). Insects are ubiquitous "companions to science" (Beisel, Kelly, and Tousignant 2013a), but this intimacy has not lessened their strangeness. Knowing a great deal about insects often tells us little what it is like to *be* them (Crist 2004). Jussi Parikka suggests in *Insect Media* that this recalcitrant alienness is an important part of what makes insects so good to think with: in resisting domestication to human concepts of the world, they inspire a "critical ethos of difference and ecosophy" (Parikka 2010).

Researchers at the Vector Genetics Laboratory think it is ridiculous to imagine what it is like to be a mosquito. They do not seek this sort of understanding. They rather understand mosquitoes in reference to very specific questions, the pursuit of which require data about certain aspects of some mosquitoes' bodies and lives. What are the population sizes in various villages, and how have they changed? What distributions of genes are present there, and how do they "flow" between places? What are the average lifespans, habitats, and behaviors of mosquitoes in place? While to many of us mosquitoes may be anonymous or collective beings, to the researchers they are often more like a substance or a medium through which they study the dynamics of genes. As Kaushik Sunder Rajan points out, the life sciences are increasingly becoming information sciences, and this is especially true for population genetics. The mosquitoes we encounter in their work are very often what I term "informational mosquitoes." The informational mosquito is an assemblage of data acquiring the form of an insect body. It is visible as lines of code on computer screens, and it is produced by the physical dissolution of mosquito bodies by equipment housed in various laboratories. These pieces of equipment, from beakers with metal balls in them to centrifuges, dissolve the body into liquid substances which are translated to genetic coding data. For the population geneticists especially, it is these data with which they work much more often than with living intact mosquitoes. The informational mosquito does not replace mosquitoes as living organisms in their work, but it stands alongside them as a distinct object of knowledge.

This is where fieldwork comes to matter. It is in fieldwork practices, in which scientists physically travel to locations inhabited by wild-type mosquitoes, that the informational mosquito rubs up against its embodied counterpart. We will resist the temptation, in this text, to treat the latter as more "real", recognizing the scientists' insistence that it is in fact the former which have more to tell them about life ways and bodies. One primary contention of this dissertation, however, is that the excursions made by members of the Vector Genetics Lab outside of the laboratory spaces of the UCIMI into the spaces of their potential field sites, The Union of the Comoros and Sao Tome and Principe, matter in complex ways which deserve clear articulation. I hope to demonstrate that this is a project which does not happen in the laboratory and get implemented in the field, but that the field itself exercises a kind of agency which demands a restructuring of narratives of innovation. Especially in relation to the UCIMI trial, the researchers are operating in an economy of hype (Rajan 2006), capitalizing on the projected utility of a project they have not yet even properly begun. But unlike other hype-oriented biotechnological work, especially projects related to human bodies and genes, the hype in this work is not located primarily in the qualities of the mosquito they can create, but in what it becomes through reproduction.

Informational mosquitoes are bloodless entities. Housed in enormous spreadsheets of data, they are bodies dispersed—their various aspects appearing as lines in different columns for various genes of interest. They do not inspire annoyance or disgust; they will not touch you or bite you or drink your blood or infect it. By transforming living mosquitoes into informational mosquitoes researchers are able to see relationships among mosquitoes that would be impossible in their embodied form. The informational mosquito *tells them things*, they say. Mosquitoes just flying around don't tell them much more than they tell other people. Living mosquitoes are opaque. Even an experienced specialist like Anton cannot tell morphologically identical species (like *Anopheles gambiae* and *Anopheles coluzzii*) apart, nor would he know anything about where they come from. It is only by crushing, dissolving, and spinning the body at high rates of speed that it becomes the informational mosquito which, bloodless, speaks.

When researchers make their journeys into "the field", however, there is of course blood. Elided from much of the day-to-day work of population genetics, blood is the substance binding humans and mosquitoes in the networks this project ultimately aims to interrupt. They drink our blood, infect us, disgust us. We sometimes see the blood burst out of them when we kill them. Smeared against your own flesh, the blood of the mosquito you kill might be yours or someone else's, mingled with the body of the insect itself. Out walking through villages, or tramping through swampy refuse, blood is very much a connection point between scientists and mosquitoes. Blood disappears and reappears in phases throughout their work, with the informational mosquito bloodless both literally and metaphorically. The mosquitoes most linked with blood are the wild-types, or the ones that just live in the world and bite people. Blood as an idiom of relationality (Carsten 2011) falls in and out of usefulness and researchers switch between different ways of thinking mosquitoes and, I argue, different mosquitoes they think with.

Fieldwork is the linkage point between computer models and geographic terrains, between information and blood, and between the researchers functioning as scientists and as persons. For those with years of experience in the field, like Anton and Greg, their bodies are permanently changed by their fieldwork experiences, their blood altered by years of biting. For all of the researchers who join on field trips, their bodies enter discussion in different and more prominent ways than in the laboratory. They are bitten, they sweat, they attract mosquitoes. These fieldwork expeditions have included not only entomologists and others negotiating with people in The Union of the Comoros or in Sao Tome and Principe, but also people from John Marshall's modelling laboratory in Berkeley. John thinks it is important for him and his postdocs to have an idea of the "real" places they are modelling, and not only the abstracted points of data they harvest from the Vector Genetics Laboratory. Much of the experiences researchers discuss and share after their fieldwork trips does not make it into their scientific papers. If scientific rationality is maintained not by controlling what scientists think, but how they publish (Strevens 2020), it is important to keep in mind here both the interconnections and the distinctions maintained between the researchers as authors and as bitten, bloodied bodies. We will see how relations between laboratory mosquitoes, especially the genetically engineered Irvine mosquito, are at the center of the group's work. This meeting point between laboratory and field is conceived as an interface not only between sexual mosquitoes, but also between types of placed-ness and ways of knowing for people.

As we will see, mosquitoes are not "natural" for the scientists, but they are at least as much not a "cultural" phenomenon. The researchers are rather concerned with the pragmatics of causes and effects. A mosquito forced to commit too much incest may be "unnatural", but this comes to matter only as it bears on his representativeness for a study. A village in which one bleeds and itches and experiences a heightened awareness of one's body in discomfort may be more "real", but this does not mean it serves as a more durable or enlightening reference point for reality. In tracing how researchers do fieldwork, and how they move in and out of the laboratory, I am interested to track how the lives of working scientists actually look in engagement with the world. Their fieldwork is a good place to understand the diversity internal to Western Naturalism, where researchers navigate multiple ways of knowing within the project itself. I will argue here that relevant distinctions, for them, tend to be less about nature or culture, objective or not, and more about levels and types of control.

Malaria

The UCIMI trial may be the first release of organisms with gene drives outside of a laboratory. This raises a number of legal, ethical, and political concerns. If a gene drive spreads by organisms having sex, how can people limit it to a geographical area, especially if those organisms can fly? What does that make of national sovereignty over such decisions? Moreover, while scientists have been working on various ways to "reverse" such drives, it is not guaranteed that an intervention, once established, could ever be undone. Whole species could be permanently changed. Combined with the fact that geographic containment seems quite impossible (outside of genetically isolated islands like those of the UCIMI trial, discussed at greater length in Chapter Five), gene drives raise serious political questions that cut to the heart of questions about consent, mechanisms for agreement, and risk. Add to that concerns that the genes may have unintended effects, perhaps producing unpredictable mutant organisms (Evans et al. 2019) and the fact that there is no clearly established legal framework for regulating them, and it becomes clear why many of the scientists with whom I work see politics, public opinion and the law as entirely capable of disallowing this area of research before it ever gets the chance to be tested in the "real world."

Into this general situation, malaria acquires a particular and, to the UCIMI researchers, very valuable resonance. Nearly half a million people die of malaria every year, most of them children. Many of these people have inadequate access to food, healthcare and other necessities. Malaria is and has been since at least the middle of the twentieth century a poster child for philanthropic and charitable action. It is not controversial in these spaces. Malaria is an area which carries, especially for Western scientists, a tremendous amount of moral capital. It appears as both prestigious and as ethically laudable for researchers at an American university to propose that they have developed the technology to end it. This moral capital is also important to other scientists invested in the future of gene drive research outside of the world of mosquitoes. There is a sense that a first open field trial may either "ruin it for the rest of us" or "open the floodgates", and scientists who would like to one day run their own projects or experiments are very much hoping for the latter. There is a sense that malaria should "go first" both because it is expected to garner a great deal of positive public opinion and because the "cost of doing nothing" is so high. This public good will, it is hoped, will prevent politicians from feeling moved to institute strict regulations or even prohibitions on the technology.

This context intersects with the history of anti-malaria campaigns which, in the first two decades of the twenty-first century, was experiencing something of a renaissance. At the turn of the twentieth century experts predicted an "imminent malaria disaster" (White et al 1999). As is discussed at greater length in Chapter One, an optimistic midcentury agenda to eradicate the disease had by this time been long shelved as impossible, and growing resistance both on the part of mosquitoes to insecticides and on the part of parasites to antimalarials seemed like it might spell a growing disaster. The Bill and Melinda Gates Foundation around this time, however, announced a new eradication agenda, something of a shock to the global public health community. By that time dwarfing even the WHO in terms of funding, the Foundation had the resources to make this new agenda happen, and in the intervening decades have pushed for a major resurgence of funding into both traditional antimalarial strategies like bed nets and drugs, as well as more experimental ones like the genetic modification of mosquitoes.

Malaria has existed nearly as long as human beings have. It is a disease recorded in ancient societies and formed a routine part of life and death for nearly all of human history in many parts of the globe. Malaria as it came to be in modern science, however, is distinctly located in histories of war, colonialism, and development. Malaria can decimate armies, especially soldiers coming from places with no experience of the disease (Bell 2010), and research into the prevention and treatment of malaria has been largely driven in modern history by military funding (Slater 2019).Carried by the movements of people, plants and animals and fostered by the ecological disruptions of colonialism, malaria can also act as a protection: it is much more deadly to newcomers with no natural or acquired immunity to it (McNeill 2010). Combating malaria has long been associated with projects of economic, social and political progress (Snowden 2008), a trend which continues in contemporary reckonings of the cost of malaria which is tallied in both money and lives, the loss of each said to hold countries back from development (Shepard and Ettling 1991). Malaria, now known as a tropical disease, was once endemic to the US, the UK, and many parts of Europe. Todd Andrew Borlik makes the case that Shakespeare's Caliban is figured on the fabled Fen Demon of Lincolnshire Tiddy Munn. Threatened to take revenge on newcomers wanting to drain the malarial fens to open them up to capital production, Tiddy Munn is arguably and anticolonialist figure from within the colonizing West, personifying a dynamic later dramatically expanded in colonial subjugations (Borlik 2012). Malaria prevents the movements of people and money. It closes places off. This can be protective, as when it advantages people with acquired immunity in anti-colonial struggles; it can also exacerbate disconnection from global economic circuits. Eliminating malaria has historically been a project of making spaces available to certain kinds of bodies.

This remains true for the UCIMI. They acquire moral capital from their broad aim to eventually eliminate malaria from continental Africa, but this goal is distant and little-discussed. Malaria in their two prospective trial sites, Sao Tome and Principe and the Union of the Comoros, however, is an ongoing conversation. For governments of both countries, cooperation with the trial is attractive because it may allow them to eliminate malaria. Malaria elimination would mean that there are no cases in the country. Both the Union of the Comoros and Sao Tome and Principe have aggressive anti-malarial infrastructures, including things like bed nets, indoor residual spraying, larvicide and drug distribution. The Union of the Comoros was included in 2016 by the WHO in their list of “e-2020 countries” believed to be capable of eliminated malaria by last year. Despite these efforts, both countries have failed to eliminate the disease completely and record zero new cases of malaria although they have dramatically reduced them (Attoumane et al. 2016). These experiences, not uncommon for countries with endemic malaria, contribute to a feeling expressed by many entomologists and public health professionals that elimination is not possible with existing tools. Intensifying those efforts, many feel, just cannot get countries to zero incidence. This makes new tools, like genetically modified mosquitoes, so attractive. No one is suggesting that GEMS (of any kind) should be used instead of existing anti-malarial efforts, but there is optimism that, in combination with them, GEMs can make zero incidence achievable.

Zero incidence, or elimination, of malaria is important to countries like The Union of the Comoros and Sao Tome and Principe not only because of the suffering and death it causes among its citizens. In 2019, 39 people died of malaria in The Union of the Comoros, and 0 in Sao Tome and Principe. As Ana put it, speaking of Sao Tome and Principe when a colleague asked her “so how serious is malaria?”, “It’s mostly about tourism. That’s where most of their investment money comes from. And even though it isn’t a big problem there, people don’t want to go on vacation somewhere that malaria even is. So, they want to eliminate it.” The difference between low malaria and no malaria is an extremely important one. For organizations like the WHO or the Gates Foundation, recording malaria elimination in a country brings the world closer to total eradication (the distinction between eradication and elimination is discussed at greater length in Chapter One). For national governments and people living in countries with endemic malaria, the difference between low and no is also very important economically. As Ana points out, wealthy tourists are more likely to vacation in a place without malaria. The long history of seeking to eradicate malaria to make areas available to flows of money and people from outside here intersects with contemporary concerns about economic growth: the limitations on such flows posed by the disease continues to be cited as a drag on development, income, and investment from abroad.

The UCIMI project takes place within the context of a reinvigorated call for malaria eradication, meaning getting rid of malaria everywhere. Elimination, or getting rid of it in certain places, is considered an important step towards that goal. This “cartographic malaria” is based on an idea of “shrinking the malaria map” and “reduc[ing] the territorial footprint of the parasite” (Lezaun 2018). Like other campaigns in the long history of anti-malarial work, malaria elimination is often visualized and narrated in geographic terms, contributing both to a story about eventual eradication and to diverse stories about progress, economic growth, and access for certain kinds of (non-immune) bodies. GEM projects like the one discussed here explicitly place themselves in such frameworks, especially in discourse with global public health. As we will see, however, they contribute new map-making practices to this cartographic malaria. I suggest that these new ways of mapping mosquitoes, genes, and parasites also contributes to a reformulated cartography of human-insect relations. Adopted by The Union of the Comoros and Sao Tome and Principe as a tool on the road to elimination, genetically modified mosquitoes also alter the terrain of anti-malarial efforts by offering different kinds of relationships, both between mosquitoes and between people and mosquitoes, as preventative.

Novelty and Innovation

The new eradication agenda dominating anti-malaria efforts today center around a call for new technologies. Based as it is on the idea that existing tools are not enough to rid the world of the disease, a call to put eradication back on the agenda has been, in large part, a call for new high-tech tools. Genetically modified mosquitoes are the most visible and highly discussed of these new tools. The mosquito designed at Irvine, which is refractory to the plasmodium falciparum parasite and spreads this trait to all (rather than half) of its offspring has become a focal point for thinking not just about malaria but about the capacities and possibilities of genetic engineering. Since the invention of CRISPR, the field has been advancing rapidly. Innovations like this mosquito, it has been said, might change the world by bringing us beyond how evolution "used to work." This project is posed as a major driver of innovation, especially in its capacity to potentially make or break future uses of gene drive techniques. It is worth pausing over the conceptual framework within which these conversations take place. I suggest that important aspects of this trial, and of gene drive techniques in a more general sense, upend deeply held notions of innovation and change.

What does it mean for the mosquito to be an innovation? Innovation, as forward moving creative change, is often posed as both laborious and inevitable. Deeply linked with a history of progressivism, it carries a subtextual story of culture establishing an ever-greater distance from and control over nature over the course of human history, a process often assumed to be currently occurring at a faster rate than ever before. Progressivism, which William Adams calls "the single most powerful influence in Western historical thought" and "the tap root of anthropology" (Adams 1998) still influences deeply our available tools for thinking about new technologies, even where it remains unacknowledged. Innovations are said to be the engines of progressive change, both as ideas and as objects like the mosquito. In scholarly work on intellectual property we can see how the idea of inventions relies upon ideas as embedded, but conceptually separable from, objects (Biagioli, Jaszi, and Woodmansee 2015). It is those ideas, and not the materials in which they are instantiated, which are considered the source of novelty. Innovations, both manifested in and caused by acts of invention, are usually accepted in American university settings, as a positive good. Lucy Suchman and Libby Bishop point out that these "received semiotics of innovation" strongly emphasize the agency of individual actors over and against naturalized environments (Suchman and Bishop 2000). They propose, in the context of American business environments, to focus instead on what they call "artful integrations" in which change is more a product of long-term reconfigurations and extensions than of wholesale transformation.

Working with the Vector Genetics Lab similarly calls into question frameworks of innovation, even as they are applied to this project in other areas of discourse. From the perspective of the laboratory, for example, especially the Irvine laboratory where genetically edited mosquitoes are designed, it seems reasonable to look to the new organism as an innovation and as an object capable of rendering change in the world. What world could this mosquito create? Because the designers of genetic constructs like those "in" the Irvine mosquito tend to be the most high-profile scientists on a project like this, their labor and inventiveness have dominated popular discourses and reporting on the project. My work has centered not at UC Irvine, where most of the research on how to make the new *Anopheles gambiae* mosquitoes is happening, but at UC Davis, where the Vector Genetics Laboratory has been asked to carry out the fieldwork portion of this project. In their terms, they are in charge of implementation, not invention. This distribution of labor reflects a common systematization of innovation.

Implementation follows design. Novelty is created in the design process, when a new thing is created that has the potential to change what exists. Implementation is a question of realizing the capacities for change already existing in the innovation of the design phase. It creates nothing, only makes possible the realization of the innovation already made.

I suggest here that the practices of fieldwork problematize this conceptual structure for innovation. To be sure, scientists engaged in what is considered fieldwork in the purview of the trial are very often doing what they think of as implementation. This includes locating suitable experimental sites, establishing relationships with national governments and public health authorities, and setting up infrastructure like laboratories. It also entails collecting what is known as baseline data. Baseline data refers to information collected before an intervention is made. In this case, it means assembling a picture of what mosquitoes, people, and malaria parasites are doing in an experimental site now and in the past. The goal is to have a "before" picture to compare with an "after" picture once GEMs are released and circulating. A comparison of these two descriptions, often visualized as maps showing distributions of genes, is intended to register effects of the intervention. The field is posed as the ground upon which the novel mosquito acts; it is a passive recipient to the active innovative force of the mosquito which is thought to originate in the minds of intelligent scientists. The field, which here encompasses people and mosquitoes as well as the places they inhabit, receives the intervention. It can register whether the mosquito did or did not work as project organizers envisioned.

As we will see, however, in practice the fields in which my interlocutors work gives many more answers than this yes or no binary. I argue that a close attention to the planning and early stages of implementation of this trial gives us evidence for a compelling counter-narrative of innovation useful not only here but in many other current and potential projects. This narrative revolves around an obvious point that has been relatively neglected in conversations around gene drives and other newer kinds of genetic engineering. This project, like many others, depends on manipulating populations and their reproductive networks. Gene drives change the inheritance patterns of genes, an effect primarily visible and effective to organisms in the aggregate over time. Gene drives also *delegate effects*: rather than doing things to living beings, researchers see this as a way to have them do things to each other. Gene drives are perhaps the clearest example of this strategy, which is seen to open up new potentialities for human control over the natural worlds. What I learned from my work with the Vector Genetics Lab, however, which I believe is under-discussed in debates about genetic engineering strategies for non-domesticated organisms, is that this also involves a re-location of novelty. Implementation, far from being after-the-fact, is actually the main event. The project is not the mosquito; it is what the mosquito can do with a world. Investigations of mosquito worlds, thus, take on a new and greater significance: they are not the passive ground upon which change is enacted, but active doers catalyzed in a particular way by sex.

We tend to think that the novel thing here is the new mosquito they have designed down in Irvine, and we talk about the potential capacities of this new organism to change whole global species like *Anopheles gambiae*. But when you actually look at what setting up and implementing a trial of this kind looks like in practice, you see that the novel organism is not of much interest at all. The new mosquito, or the Target Product Profile, comes up extremely rarely in the researchers' work. Paradoxically, this organism offers them little to be curious about. It is the only mosquito in their work about which they believe they know enough; there are, consequently, few questions to be asked. It is not anticipated that the Irvine mosquito will surprise them. I think it is important to highlight the fact that this is because the Irvine mosquito *does not do anything on its own*. This is at the heart of this and other new techniques for intervening on reproduction. The space where researchers anticipate they may be surprised, where something unpredictable may happen, is at the sexual interface between the new mosquito and its wild-type counterparts. And because they know an enormous amount genetically about the former, but really shockingly little about the latter, it is in the wild half of this equation that attention is centered. It is in the cryptic, unpredictable, and invisible variations of these mosquitoes that they see potential for effectiveness, unpredictability, and risk. We will see in Chapter Four how the concept of wildness emerges *through* and then eventually *as* that which resists prediction and control. Wildness, in the practices of the population geneticists and entomologists working on this project, *is* the field, both as a quality of mosquitoes, as a personal experience of life inside and outside the laboratory, and as a set of assumptions about control and its limits. We see the lab turned inside out: contrary to more traditional practices of bringing objects in from the world to understand them in the lab, it is the question of what this already-known organism can do with the world that is at issue. The sexual interface, not either mosquito on its own, is posed as the entity with potential for transformation.

I want to show here how the thing which they consider both to be the animating force of this work and the potentially destabilizing threat to it, reproduction between the GEM and wild-type mosquitoes, also shifts the figure of the field. From the ground upon which innovation is realized, the field here emerges as the active locus of surprise, creativity, and change. Rather than an invading intervention, my interlocutors tend to see the possibilities of a future unknown glimmering, intermittently, in the very thing which is created to background this novelty: the world as it already is. It is not that the new innovation transforms the world into which it is introduced, but that the world already contains immanent potentialities for change which can be made manifest. It is in the re-working of relationships, not in the imposition of pre-determined plans or all-powerful interventions that they see potentials for (always risky) transformation. This perspective runs athwart the increasing pace of a hunt for novel objects or things to metabolize. Immanent novelty is not, eventually, digested into an expanded space of human capacity. It dances obdurately always beneath, always within, always already at the heart of what already is. This counter-narrative can also be useful for thinking other new biotechnologies. In a world where the constant search for the new renders an increasing sense of effeteness, to re-locate change potential in the immanent situations already around us is to reinvest the world with the terrible promise of the radically new. To think futurity through a new biotechnological object or technique may then mean focusing not, or not only, on the object itself but on what it draws forth and changes. Implementation, in this story, is not an after-effect of innovation but the main event.

Wild

The field, as the source for this counter-narrative of novelty I am proposing, is not often associated with the word "nature" among the scientists, but it is tightly linked with their notion of the "wild." I want to claim that, because the project works at the sexual interface between "wild-type" mosquitoes and genetically engineered ones, it is actually in the wild of the field that potential for change resides. This emerges most clearly for the population geneticists at the level of genes. Not all *Anopheles gambiae* mosquitoes are the same. Like people, they have diversity in their genetic codes. This diversity is known to them as genetic polymorphism. At the heart of the work of Vector Genetics Lab researchers, especially in this project, is the insight that the different genetics of mosquitoes, even within a species, is linked to the places where they and their ancestors live and have lived. Informational mosquitoes can be read for place, seen as accumulated histories of imbrications with different forms of human life. Places and bodies are, in their work, only conceptually separated in some instances. Most of the time, they are thought of together. Different sorts of places make different sorts of mosquitoes; the place from which a mosquito comes is readable in its genes. Much of the work of people in the lab is connecting place and genes to generate insights about changes over time.

To borrow Stefan Helmreich's phrasing, life forms and forms of life are interconnected in the genetics-oriented practices of the research (Helmreich 2011). The ways mosquitoes live is readable in the informatics of their bodies, and vice versa to some extent. This continues to be true when the scientists actually travel to prospective field sites, with added dimensions. What sorts of houses do people live in? What animals do they keep? What is the quality of the soil there? Where do they store water? What sorts of vegetation are nearby? For entomologists, all of these questions help them to understand the life-worlds of the mosquitoes there, and also their informational selves. The specificities of localities are considered important here. Because not all *Anopheles gambiae* are the same, their offspring with the genetically modified mosquito will not all be the same. There is a fractionated tension here between the research done by fieldworkers on the localisms of mosquito life forms and forms of life and the scalable aims of the project, which I discuss in Chapter Five. Important to note here is the significance of local specificities for the researchers. The ultimate aim of their work is to demonstrate that *the project could work anywhere* that has *Anopheles gambiae* mosquitoes. In order to do this, they believe that they need to show that the GEM works (meaning, it pushes the malaria refractory trait through populations) not only with laboratory mosquitoes, which all of the scientists involve understand to be highly idiosyncratic in their predictability, but with wild-type mosquitoes. The latter are much more the norm for mosquito life on this earth, but also much more abnormal insofar as they carry many alternative genetic sequences, not all of which are known. These sequences, known as polymorphisms, are discussed in Chapter Two. The Vector Genetics Lab scientists see mosquitoes as products of their entangled worldings with people, and the creation of new kinds of mosquitoes as having the potential to create, in turn, new kinds of worldings. Specifically, they seek not to change mosquitoes per se, but to change mosquitoes in order to create a world with different pathogenic networks of people, mosquitoes, and malaria parasites. This work poses a set of questions in some ways mirrored with dialogues about de-extinction efforts. There, we see critics asking what it means to "bring back" a species without bringing back the worlding in which it held on to the achievement of its existence in the past (Dooren and Rose 2017). Here, the researchers are working to create entirely new worldings by bringing in an organism posited as unlike anything that has come before.

What makes this organism *the most new*, for them, is that it is man-made. It is an embodied innovation. However, they do not conceive of it as wholly so. In the 1980s and 1990s, researchers in the nascent field of mosquito genetics envisioned creating synthetic mosquitoes de novo, starting not with an existing mosquito but with the basic building blocks of life. While synthetic biologists today continue to create forms of life starting from nothing, searching for the "minimum basic units" of life (Mutalik et al. 2013), this is not the way mosquito genetics has gone. Rather than creating life, this and all other projects in this area focus on repurposing it. This forms a contrast to the excitement about simulated or synthetic life that was in the air in those decades. Stefan Helmreich describes in his *Silicon Second Nature* the work of the Santa Fe Institute to simulate life which, Helmreich says, left "no original. When form is decoupled from life we are left with free-floating form. In the bargain, nature becomes everywhere and nowhere, both completely given and thoroughly constructed." This leaves us in a state "after nature" to borrow from Strathern; both post and in pursuit (Helmreich 2011). The distance between a synthetic mosquito and an editing one is important here. As Helmreich points out, a pure synthetic approach, whether virtual or biological, implies an alienation from nature, invoked but unlocatable. For repurposed, as opposed to constructed, life this is not the case. The researchers believe it is a better strategy to alter existing mosquitoes rather than create them whole cloth not only because no one can do the latter (and there is no prospect of being able to do so), but because mosquitoes as they already exist are seen to embody a capability and intelligence that exceeds those of the scientists themselves. If there is one thing they know of evolutionary biology, it is the infinite creativity and voraciousness of life. This becomes, as we will see, one of their arguments in favor of population modification rather than population replacement strategies. As Hans said to me once, in reference to the Oxitec mosquito (which aims to stop aedes aegypti mosquitoes being born) "I don't know how long it would take, but something else would definitely start feeding on human blood." Whatever would be vanished from a simulated nature is here thought to remain inside the mosquito. The scientists thus think of themselves as coopting as much as creating. And what they coopt is imagined as a powerful force, both in terms of life and of sex. Ferality, desire, creative adaptation are resources to be exploited from existing mosquitoes which cannot be made effectively. This remainder, however, is not conceived of as their naturalness, but as their wildness.

I want to dwell on the meaning and significance of the wild for this project. I argue that their uses of this concept assume some of the duties typically carried by a contrast between nature and culture, but with important differences. The wild may be "the process and the essence of nature" (Snyder 2020), but the wildness of mosquitoes defamiliarizes what that nature is. For one thing, it would be extremely hard for any of my interlocutors to say whether mosquitoes are "natural" or "cultural" beings. The disease-carrying mosquitoes of interest here are not domesticated, but neither are they in any sense denizens of a pure nature, however imaginatively untouched. I think often of how this was phrased by an Oxitec representative with whom I worked in Piracicaba, Brazil. She was asked to give a talk at a local university explaining the company's genetically modified mosquitoes, which were at that time just starting to be used in the city to prevent the circulation of dengue, chikungunya, yellow fever and, most pressingly in that Summer of 2016, the Zika virus. "Mosquitoes are just like cats" she told her audience. "If you kill all the cats, would you be messing with the balance of Nature? No."

As distressing as her prospect of massive cat death might be, Carla's analogy was apt. She was responding to a student who had asked what the impact might be on local ecologies if all of the aedes aegypti mosquitoes were to stop existing, as the company proposed. Aren't there things that eat mosquitoes? Birds or bats? What would happen to them? Carla was referencing research that seems to demonstrate that no, there are not animals which depend on eating aedes aegypti mosquitoes for food. The same appears to be the case for *Anopheles gambiae*, although the question is less relevant to population modification projects. *Anopheles gambiae* (the main African vector of malaria and the subject of the UCIMI work) and aedes aegypti (the main vector of dengue, chikungunya, yellow fever and the Zika virus and the subject of Oxitec's) are both anthropophilic. Without us, they are not. We have evolved together; both of our bodies shaped by a species-life of constant interactions. They do not live where we are not. They are a product of us, insofar as we are products of each other. We are their ecosystem, along with all the things that we build and feed and do. We are linked in relations of significant otherness, although we are not likely to call it companionship (D. Haraway 2003). Our interconnection is not valorized, nor do I argue it should be, but that does not lessen its intimacy. One point of interest in thinking with mosquito scientists is learning to speak of such co-constitutive relations outside of the language of amity, symbiosis, or love without lessening their intensities.

The distinction which recurs here between laboratory mosquitoes and wild type mosquitoes is indeed a very important one, but it does not reflect a difference in degree of interaction with humans, nor a degree to which an insect's body is shaped by such interactions. In a house, as much as in a laboratory, mosquitoes are made out of their dance with us. If we start to use an insecticide, their great-grandchildren are likely to inherit immunity to it as those genes spread through and across populations. If we use bed nets, they learn to feed in the daytime. In the laboratory, as we see in Chapter Three, mosquitoes acquire new bodies and behaviors as they adapt to that strange milieu. This adaptation is best seen as a change in their existing constitutive relations with humans, however, rather than the beginning of them.

This makes it much more difficult to think of the mosquitoes in the laboratory as entering more deeply into the control of humans. They may have less contact with people in the insectary, visited only by the perfunctory entomologist, than they do when their desires drive them to hound us in our beds. The increasing exclusion of humans from the realm of nature has made it possible to think of ourselves as intervening upon it from some external position (Williams 1980). This position is not tenable with mosquitoes. There is no outside, for them or for us, of either nature or culture. And yet there is wildness. I argue that the mosquito scientists do not think of mosquitoes in the field as somehow further from the realm of culture, or the influence of human beings. It is rather the nature than the degree of such co-involvement which is important to them. Mosquitoes of the field do not interact less with people than mosquitoes of the lab, but their interactions are not considered to be less *controlled*. Wildness can thus be thought of as an issue of control. For a project which is conceived as an exercise of biological control, the researchers think of themselves as establishing a kind of control over mosquitoes and their life worlds. But we may be misled if we assume too quickly that the meaning of this term is clear. As we have seen, the researchers do not aim or want to create organisms entirely subjugated to their wills. It is rather the feral desires of mosquitoes, which makes them good at exploiting and navigating their particular worlds, which is coopted and redirected in new ways using these genetic tools and techniques. Understanding how the scientists conceive of the control they pursue, then, can tell us more about the underlying theories and assumptions about entangled human and nonhuman worlds on which they draw and which they creatively repurpose for themselves.

Control

So, what is this notion of control they are working with? We can see how controlled versus uncontrolled relations with mosquitoes are not conceived of as an issue of less or more imbrication with the lives of people. Neither can we say that control is an issue of domination for them. As we will see in Chapter One, their strategy is posed as useful precisely insofar as it does not depend on direct domination or subjugation. In the broadest sense, the researchers would describe their project as a form of biological control. Described at greater length in Chapter One, this is a strategy for reducing the population of one organism by harnessing the natural desires of another. This technique stretches as far back as any author wants it to— often back to ancient Egyptian cats kept for rat control. The UCIMI is part of a "new generation" of this ancient and widespread practice that was formally articulated in its modern form in early twentieth century California. Genetic biocontrol, or GBC, moves this technique into the insides of organisms. GBC both continues the trajectory of thinking about control it inherits from classic biological control as well as subtly reformulates it. The new capacities for action offered by genetic techniques, I suggest, do not only enhance entomologists' power to carry out existing programs— they also open new possibilities and constraints for thinking about what it is they are doing.

Biological control works at a distance. People release some predator organism, and it eats is preferred prey. This accomplishes the goal of having less of the prey by delegating the reduction to the desires of predators, which are much more nimble sensitive aggressors than people could ever be. This is especially true of insects, which are too numerous and small to kill one-by-one. Biological control is a strategy, then, of framing the action to achieve a certain effect. It structures the rules and parameters within which organisms pursue predictable ends, harnessing their instincts and behaviors to achieve second-order effects people want. GBC likewise seeks not to dominate or subjugate insect preferences, desires, or behaviors but to channel them. With GBC, however, this channeling moves from an inter-species dynamic to an intra-species one. The mosquito becomes both predator and prey in the traditional biological control paradigm. But it is also neither. The UCIMI trial does not aim to have mosquito populations reduced by being eaten by another species. It rather aims to have them changed through reproduction. The primary relationship moves from eating to sex. With this move comes a focus on the transmission of genes, visible through work on the informational mosquito as an entity of data.

The *Anopheles gambiae* populations on an island experimental site are visualized as complex dynamic systems of interacting genes. Introducing not only a new gene, but also a new pattern by which it moves through populations, is a way of manipulating the working of that system. For both forms of biological control, this focus on the regulation of systems places them within an intellectual history linked to cybernetics. Coined by Norbert Weiner in 1948 (Wiener 1961), cybernetics posed animals, machines and ecologies alike as composed of circular feedback mechanisms whereby the results of action become the basis for future action. In *The Cybernetic Brain* Andrew Pickering describes the early inventions of British cyberneticians like thermostats or "turtles" which responded to light cues in the environment. We will see in Chapter Four the ways in which cybernetic thinking impacts the UCIMI's work with people, as well as mosquitoes, as they draw upon histories of thinking about human organizations and their management. In terms of mosquitoes, cybernetics influences how the researchers think about the kinds of control they aim to effect with mosquito populations. Their language of control is significantly indebted to cybernetic discourses about control as achieved by self-regulating mechanisms. From this tradition, they take the notion that control might be wielded not by an external human agency imposing a will upon a system, but by the manipulation of the self-regulating forces internal to that system's perpetuation.

Pickering is careful in his text to highlight the fraught potentials of this term. The term control often conjures the idea of "Big Brother watching and controlling one's every move—people reduced to automata" or Deleuze and Guattari's "royal sciences" seeking to understand the world in order to dominate it and put it at the disposal of human beings. "I need to stress" Pickering writes "that the cybernetic image of control was *not like that*". He emphasizes the unpredictable becomings in which those scientists were interested. Such inescapable unpredictability is incompatible with domination-style control. "The entire task of cybernetics was to figure out how to get along in a world that was not enframable, that could not be subjugated to human designs— how to build machines and construct systems that could adapt performatively to whatever happened to come their way" (Pickering 2010). Far from being deterministic, it is this openness to possibility that distinguished cybernetics thinking on control from some colloquial uses of the word that call to mind imagery of total mastery.

It is this cybernetic notion of control that researchers use when they speak of biological control— the abilities of complex systems to regulate and adapt. And it is these capacities to regulate and adapt which become the focus of attention and the space for intervention in gene drive techniques. Like the cyberneticians Pickering describes, they likewise are not using control to mean domination or direct imposition of human will by force. Like them again, they are more interested in what might be called design— the arrangement of situations at the molecular level by which natural action runs a certain kind of a course. Like the cyberneticians, they do not imagine themselves formulating plans imposed upon matter but rather visualize "a continuing interaction with materials, human and nonhuman, to explore what might be achieved— what one might call an *evolutionary* approach to design, that necessarily entailed a degree of *respect* for the other". For Pickering, what sets cybernetics apart from what he calls the modern sciences in terms of their conceptualization of control is the former's "ontology of unknowability and becoming."

Cybernetics introduced a way of thinking that could describe both machines and learning organisms as recursive systems, with inputs and outputs. Today, this mode of thought is no longer challenging. An organic approach to technology is widespread, and we can see even in the Vector Genetics Laboratory and the workspaces of their colleagues how living organisms are easily translated into the language of machinery. The immanent novelty I describe the VGL researchers uncovering in their fieldwork is within an ontology of unknowability and becoming that owes a great deal intellectually to cybernetics. It differs, however, insofar as that unknowability is not located only in the unpredictable emergent effects of the system itself. Nor can all of the recursive feedback elements to the system be mapped and understood. There is understood to be an element which retreats forever outside of such efforts— the wild. The finite is here not located in the infinite capacities of people to increasingly dominate the earth, but in the small, strange, and unpredictable ways the world is and will always become other to what we have been able to imagine. What is out of control and what is in control enter a new formation: that which is out is needed integrally to feed that which is in. This is theorized by the researchers through the notion of sex, as we will see.

The philosopher Yuk Hui made, to me, a surprising mention of biological control in a recent interview. Discussing methods of avoiding pesticide use, he said "There are also, for instance, specialized techniques of breeding certain insects that will eat harmful insects. This is technodiversity" (Dunker 2020). Hui's technodiversity is a mode of resisting a universalizing history of technology, moving through stages of development that happen to occur in various places. Instead, he calls for us to think about locality in terms of Foucauldian epistemes, or systems of knowledge. Not only do we have many places, but we have many systems of knowledge. Like the environmental niches which support biological diversity, diversity of thought is supported by areas populated by different assemblages of people, technologies, and ideas.

Why does Yuk Hui see biological control as an example of technodiversity? His call for local variations seems at odds with the universalizing goals of the project, which imagines a technology whose usefulness inheres in its global scalability. This universalizing impulse appears at times in tension with the work of the field, in which it is precisely the variations of localities, and the diversity of mosquitoes, which is of interest. This tension, however, works. The universal is only dimension of their work, however. It would not work without it— if the UCIMI were to announce that it had created a mosquito which could eliminate malaria *in one small island alone,* it would not be lauded as an interesting or important event. But mosquitoes are not self-replicating things. Anna Tsing writes of the modern dream of self-replicating objects, infinitely expandable, able to grow without changing in economies of scale. These kinds of self-replicating things are “models of the kind of nature that technical prowess can control: they are modern things. They are interchangeable with each other, because their variability is contained by their self-creation. Thus, they are also scalable” (Tsing 2017). Mosquitoes are sexually reproducing. Their offspring are not copies of them. Tracing the implications of these facts is, in large part, what populations geneticists are engaged in. How this matters is that it does not permit a universalizing narrative to overcome local resistance. Because the project works through sex, it cannot overcome, cannot do without, the constant production of difference at the sexual interface between wild mosquitoes in all their diversity. The image of control that emerges through their work, then, is more that of a surfer balancing herself with fast, ongoing movement of something much more powerful than herself than it is like a puppet master determining the movements of matter.

Yuk Hui’s was not the only unexpected mention of biological control that sat me up in my seat. When Donna Haraway came to Davis some years ago to give a talk about “making kin not babies” she made the case to us for a multispecies notion of reproductive justice that does not shy away from problems of numbers. We need to learn, she told us, how to make “enduring mutual, obligatory, non-optional enduring relatedness that carries consequences”(Paulson 2019) with others, and not just people. Our dominant framework for doing so, by having babies and tracing such obligations through biogenetic kinship, has made it harder to create the kinds of relationships we need outside of these circumscribed webs and, paradoxically, made us less responsible to our broader communities, which encompass more than human beings. It was at the tail end of this talk that she referenced a news story about the genetic biocontrol of mosquitoes, and casually cited this as an excellent example of what she was talking about. Why, and compared to what, does this practice offer a more hopeful politics of relations? For Haraway, such practices fit with an ethic of human recognition of interrelation with other-than-human worlds. It starts from an assumption that "no species, not even our own arrogant one pretending to be good individuals in so-called modern Western scripts, acts alone; assemblages of organic species and abiotic actors make history, the evolutionary kind and the other kinds too" (D. Haraway 2015). For Haraway, as for Yuk Hui, biological control is contrasted with other modes of control. Most clearly represented by the killing of insects with pesticides, this form of control is domination oriented; it is top-down, it imposes human wills directly upon the bodies of others. Biological control strategies, by contrast, involve a weaving together of human and insect reproductions, an act which cannot move without a close recognition of the other. Biological control strategies are often called "elegant" insofar as they work on their own. They are based upon intimate knowledge of other-than-human reproduction, or what Emily Wanderer might call "biologies of betrayal" (Wanderer 2015). They are responsive. But are they response-able?

Haraway uses insects again to explain this concept of response-ability to others. In *When Species Meet*, she borrows a fragment from the YA novel *A Girl Named Disaster* in which a laboratory assistant allows tsetse flies to feed on his flesh. He explains to the protagonist that this is in order to feel the suffering experienced by the guinea pigs he was exposing to them. For Haraway, this act of sharing pain does not negate the instrumental relationship the laboratory assistant is in with the guinea pigs: he is using them for his own ends. But he does not believe that his obligations for care or recognition are therefore negated. Haraway calls our attention here to the companionship we share with other species we do not like, which we often kill. Insects trouble notions of coexistence, often too easy to absorb in relation to loveable animals with whom we share some feeling of mutuality of being (Sahlins 2011).

This uncomfortable call for response-ability stands at odds with the dominant metaphor in human-insect relations: war. We can see this especially clearly in insecticidal campaigns against mosquitoes. Chapter One describes the explicitly military aesthetics and organization of midcentury American eradication efforts in Sardinia. This bloody war against the mosquito (Ferry 2016), our "deadliest predator" has shaped human history (Winegard 2019) and is being launched anew by various biotechnological advances ("Debug | Verily Life Sciences" n.d.). We are said to be "on the front lines" of a "war against mosquitoes" now and for all of our history (Sifferlin 2017). The war metaphor dominates discourse about mosquitoes and permeates biology at a deeper level. Conflict between species and between sexes is often posed as the main driver of evolution (Arnqvist and Rowe 2013). Biological control sits within this narrative frame, while at the same time offering a notion of control based less on winner-loser conflict and more on the cybernetically-inflected notion of regulation. This regulatory notion of control has differences from a conflict-oriented one, implying a goal of domination. But those differences are sometimes obscured by a lack of language to speak of these differences. Haraway's call for us to recognize response-ability to organisms we use instrumentally, dislike, or harm us, poses this problem. How do we conceptualize relations that are not of symbiosis or cooperation without falling into a framework of thinking war? I argue that when such relationships are posed in terms of war, we miss a significant portion of how practitioners are remaking control practices, practices that matter not only for this trial but for others like it which wed a cooperative framework with goal-directed engineering. How do we speak about that which is both symbiosis and conflict? In which the lives and lusts of an organism are fostered in the name of control, in which loving and devoted attention is required to make organisms less visible to us? Peter Sloterdijk makes a distinction between dangerous "allotechnics" manipulating nature and good "homeotechnics" cooperating with it. But these distinctions do not work very well for mosquitoes, which are not quite nature. Mosquitoes are as artificial as we are; they are as much products of built human ecologies and practices. So how to tell the difference between cooperation and manipulation? Is that still a reasonable goal? How does judgement remain possible?

Haraway's talk was related with her book with Adele Clark *Making Kin Not Population*. Their call for serious feminist attention to issues of human numbers and their impacts on the earth also intersects uncomfortably for some with histories of population control (Murphy 2017a). They note, referencing Michelle Murphy's work on the topic, how often WWII comes up in histories of population thinking and what Murphy calls the "economization of life." Discussed at greater length in Chapter One, "the framework of the economization of life was reworked post WWII, the historical moment when many former colonies became supposedly autonomous nation states. Frameworks of the economization of life offered new forms of neo-colonial governance promotable by more powerful nation states, typically conceptualized as 'development' (Clarke and Haraway 2018). The UCIMI trial is consonant with this trajectory in two overlapping ways: both by borrowing discourses of control from thinking about the control of human populations, and in the location of anti-malarial work within ongoing projects of development. Haraway and Clark offer as an alternative to such modes of population control an “other than biogenetic kin-making” or a “project of nurturing nonbiological kin-making.” To get away from overly biological or biogenetic notions of kin can open up other axes along which people can think about their obligations to and relationships with others— including not human others. But these questions of choice and control which made Clark and Haraway’s text so controversial for many feminists thinking about histories of human reproduction and choice also bring these troubles into the realm of not-only-human distributed reproduction. If reproduction is “not so much a ‘thing’ as an overdetermined and distributed process that divergently brings individual lives, kinship, laboratories, race, nations, biotechnology, time and affects into confluence” (Murphy 2020), then a project of kin-making like the UCIMI trial brings along with it questions feminists have long been asking about efforts to control or manipulate reproduction. How is structural power distributed? Who gets to make choices? And if choice, as Mol observes, is a “rhetorical magic wand used to transform subordinated objects into self-determining subjects” (Mol 2008) then what languages do we have to discuss control or freedom from it? Clark and Haraway make a call for a flatter more integrated analysis of human and nonhuman reproductive infrastructures; from this perspective, where does the mosquito sit within conversations about reproduction and control which originate with humans? We can see how notions of sexuality have moved fluidly between insects and people (Kosek 2010). If human and insect reproductions are linked, how much and in what way? What elements of life and reproduction appear as inevitable, natural, and universal for each, and how are these grounding assumptions born out of projects of control?

We can see how the UCIMI trial is predicated upon the basic assumptions that mosquitoes *will* reproduce. This assumption grounds not only this project, but other basic work in population genetics like the estimates of genetic isolation described in Chapter Five. If mosquitoes have been in a place, the thinking goes, they will have been breeding. And their genes will be there. We see in Chapter One how this inevitability, posed as basic desire, has driven biological control theorizations from their outset. These desires to either eat or have sex are considered so fundamental as to be extremely powerful. It is the dispersal of these basic and ineluctable facts through the bewildering multiplicity of insect bodies which makes domination-oriented methods of control impossible. Here, we see an assumption that the satisfaction of desires is a force much greater than human ingenuity. From that, we get the theorization that the best way for people to control outcomes is to guide these desires, rather than subjugate them. It is this basic desire that is posed both as a potential challenge to control, as when researchers debate anxiously about the possibilities of gene drive containment. It is also the driving force of the project which they propose can qualitatively change humans' relationships with insects and disease. Control is thus moved from a mode of disallowing to a mode of channeled flourishing. This channeled flourishing both spotlights human insect co-creation by making it the object of explicit work. It is also in service of a mode of insect control which is potentially useful insofar as it does not ask people to continually attend to their relations with mosquitoes.

Kaushik Sunder Rajan writes that "the life sciences represent a new face and phase of capitalism", accepting it as "the natural political and economic formation not just of our time, but of all times" (Rajan 2006). Sunder Rajan is interested in the ways in which biotechnological products circulate and gain meaning and value within capitalist markets. But this acceptance of the inevitability of capitalism as the natural formation of all time also shapes the theorizations of life and ecologies in more basic research. The image of the surfer balancing upon the powerful moving forces of nonhuman desires takes seriously such desires as the driving forces of the world. It rejects the possibilities of controlled planning in favor of the design of the parameters of markets. It is an approach of “using life to manage life” (J Lorimer 2020). For mosquitoes, this looks like the design of the system through which genes move. It assumes “selfishness” as a fact of all life and works with that assumption to direct selfish actions. We see in Chapter Four how a practice of control analogous to that applied to mosquitoes is also proffered in relations with human beings. We see there how experiences, pleasures, and feelings are paramount considerations in directing the decisions and actions of human beings. Selfish pursuit of desire is a basic fact of the world. These pursuits, further, take place in the context of others, forming systems. If the pursuing action cannot be changed, the thinking is that those systems can be shaped, like markets, to direct action. This is a mode of “composing with the world” (D. J. Haraway 2020), but it is a decidedly non-innocent one.

Sex

I have suggested that the informational mosquito is distinct from the mosquito as a bodied entity, and that these two forms are interrelated. We have seen how researchers move between each as they engage in the process of fieldwork. I have suggested that it is this process of movement which stitches together the ideations of the laboratory and what they conceive of as the existing realities of the world. What I find interesting about this process is the way in which it reformulates a familiar story about novelty: in this back-and-forth it is not the invented mosquito which is seen to be capable of enacting change on the world, but the world itself which is made as full of potential unpredictability already. Their question then becomes one of catalysis, rather than being. It is what the world can do with the mosquito that is at issue, and not only what the mosquito itself can do.

This catalysis is thinkable through the notion of sex. It is sex which, to the researchers, knits together the laboratory and the field, when the engineered mosquito mates with his wild-type contemporaries. Their offspring, and those offspring's offspring, are the central element of the future the researchers want to make possible. By not passing malaria on to people when they bite them, these future mosquitoes will interrupt the cycle of transmission between our species and theirs by which the parasite persists.

But what is sex? I have two contentions here: that, just as mosquitoes are rendered in particular kinds of ways through this work, so is sex and that this sex is not only reproduction. I have argued that the informational mosquito and the embodied mosquito are two distinct renderings. Each are produced by different questions and experiences. Of the informational mosquito, researchers ask about its genetic code. What alleles does it have? How are those similar to or different from the alleles of another mosquito from a different village, decade, or continent? Of the embodied mosquito, researchers ask about its habitats, breeding places, life stages, and morphology. They are also imposed upon by embodied mosquitoes in a way they are not with the informational mosquito: when they are in the field, it bites and harasses them.

One important image of reproduction is rendered alongside the informational mosquito, and through similar tools, concepts and formats. If each informational mosquito is a collection of data, or genetic sequences, reproduction is a mathematical recombination event. An offspring may get some alleles from its father, and some from its mother. It may also generate unique mutations, originating from neither. It is through this recombination process that researchers see mosquitoes reproduced as unique beings: the sheer number of genetic data means that recombination can happen in many, many different ways. The sheer quantity of unique individuals produced by sexual reproduction is seen as an engine of ever-increasing complexity in the world (Strathern 1992). Parents and their offspring can be related mathematically— for example, one postdoctoral fellow in the laboratory was engaged in a project of mathematically modelling parent-offspring groups to develop estimates of the average rate of mutation among laboratory mosquitoes. This rendering of reproduction also lends itself to a vision of the world: one can visualize a terrain across which genes flow, abstracting away the limits of insect bodies. Distributions of genes can be analyzed in relation to topographies, histories, or spaces of human habitation as a distributed substance, rather than as elements of living beings. Informationalized reproduction, alongside informational mosquitoes, makes possible a fluidity of analysis that population geneticists depend upon to generate insights and predictions. Not called upon to give answers about any particular mosquitoes, living or dead, they enact a separation between mosquitoes and their genes to produce insights about how those genes change over time within a population which is spread across the surface of the earth and morphs. The mechanism of change here is reproduction: it is the physics by which what I describe in Chapter Two as the morphing plane of genetic variation moves.

The reproduction yielded by informational mosquitoes appears as what researchers think of as pure or abstracted form of genealogy. It is an algorithmic pattern that can be adapted not only to the reproduction of new organisms, but to other things that change over time like learning. Yoosook Lee, a longtime member of the Vector Genetics Laboratory, completed her dissertation on the evolution of languages, asking why it is that languages do not converge over time into just one universal language. She saw her methods of mathematically modelling languages as not only adaptable to modelling reproduction in organisms, but as *the same thing*. This identity is possible because of her practices of radically simplifying material to represent basic dynamics of change.

Her analogy between language and biological reproduction is not a new idea. From the roots of modern genetics in philology (Trautmann 1987), notions of heredity have emerged out of the frameworks of lived relationships (Beer 2000). This process also goes both ways, with science of heredity and genetics borrowing from cultural resources for thinking kinship. Borrowed from kinship imagery and terms, biological genealogy circles back to figure as the naturalized ground upon which such lived relationships are layered (Strathern 1992). Historians of science point out the historical malleability of concepts of heredity, and their relationship with broader frameworks of kin relations and epistemic environments (Müller-Wille and Rheinberger 2012). There is not only traffic between kinship practices and conceptual tools for visualizing and thinking heredity; there is also a deep interconnection between notions of the relationships between concepts and the relationships between persons. Marilyn Strathern shows how certain patterns of relationality can shift between intellectual, biological and kinship spheres, alternately serving a grounding function in each. David Schneider pointed out the distinctive dichotomies in American kinship thinking between Nature and Law, or substance and code, showing how much ideas about natural relation shape what we think of as "real" kinship or relatedness, against cultural proscriptions about behavior and interaction (Schneider 1968). Kinship has been viewed as a meeting point of the domains of nature and culture: nature takes care of what bodies do when they reproduce, but people's decisions about how and whom with to do that are issues for the study of cultures and societies. The element that makes gene drives, along with other techniques of so-called lineage with intent, so interesting and transgressive to many commentators today is that it intervenes not on what Schneider might call the "Law" or the cultural half of the equation that is kinship which we are accustomed to thinking of as malleable. It rather acts on the mechanics of biological reproduction itself at what we think of as its most basic level: the recombination of genetic material. By making these processes work differently, it destabilizes the division between nature and culture within kinship itself. This destabilizing effect of reproductive technologies is not new and, as Marilyn Strathern points out in Reproducing the Future, neither is the feeling that a correspondence between social relationship and natural fact has been broken or opened up to disturbing new configurations. These implications come less saliently to the fore in discussions of animal, and especially insect, life which is not commonly assumed to include both natural and cultural components. As we have seen, however, mosquitoes are in nearly every way inseparable from human culture, living and evolving alongside us as they have. To make the "facts of life" follow different rules in a mosquito, then, is not seen to upset (at least not at this stage) deeply felt human values about kinship.

But mosquitoes are not simple automata, reproducing at random. Their sex, too, is a meeting point of the recombination of genetic materials and the stories of individual lives. There is more to mosquito sex than only reproduction. Before reproduction, mosquitoes need to copulate. Sometimes there are mosquitoes, like *Anopheles gambiae* and *Anopheles coluzzii*, which can reproduce, and can be persuaded to do so in the laboratory, but which do not do so in the wild. There are also various means by which copulation may not lead to reproduction. Many species of mosquito produce what are known as "mating plugs" or "nuptial plugs" whereby the male secretes a waxy substance at the end of his ejaculate which "plugs up" the vagina, preventing further inseminations. Mosquitoes also have various means of chemical communication which incite or inhibit copulation. Some species can satyrize each other when interspecies mating reduces the future fertility of one species more than the other. Female mosquitoes can refuse to mate with males by kicking them. Females also have preferences—

perhaps to males which have larger bodies or longer wings. There is some evidence that mosquitoes harmonize their wingbeats in flight to facilitate copulation (Gibson, Warren, and Russell 2010), but Greg is suspicious of this. To complicate everything, a standard assumption that female mosquitoes mate only once (storing the sperm in their bodies to produce multiple egg clutches) turns out to be incorrect. The Vector Genetics Lab researches not only the patterning of informational reproduction, but also the behaviors of sex. They have been conducting a number of experiments in the laboratory aimed at understanding questions about copulation and its consequences that take not the informational mosquito as a starting point, but the mosquito as embodied organism with specific life trajectories and behaviors. For example: they have been working for over six years on an experiment aimed at answering the question: if an *Anopheles coluzzii* female mates with an *Anopheles gambiae* male, is there some way in which she "knows" she "messed up" and will "correct" by re-mating with a conspecific? This kind of work is difficult, tricky, and messy. How to extract semen from a female body without contaminating it with her DNA? How to get them to mate in the first place? Chapter Three describes some of the many difficulties of getting mosquitoes to copulate in the laboratory. We can see there, as well as in the length of time the laboratory has worked on their re-mating experiment without successfully getting good data, that copulation is just as important a focus to their work as the mixing of genes and requires a distinct practice of research.

The Vector Genetics Lab wants to understand mating behaviors like multiple mating, satyrization, or hybridization (whereby mosquitoes of two different species produce offspring) partly to improve the models they have of genes moving across landscapes. The patterns of this movement are reproduction, and so where and when copulation happen and when and why that copulation leads to reproduction are important questions. Copulation is also viewed as a space of surprise and uncertainty in modelling efforts. There is unpredictability in genetic reproduction, as when new mutations arise. But the unpredictability of mosquito behaviors is assumed to be much greater and much more difficult to fully account for. This is especially true of wild mosquitoes, to which the researchers do not have exhaustive and constant access in the laboratory. The unpredictability of this wildness is due partly to the unknown genes inhering in the bodies of wild-type mosquitoes, which may lead to target site polymorphisms (discussed in Chapter Two). But it is also due to the unknown copulation behaviors of mosquitoes outside of the laboratory, which may impact the spread of the engineered gene. Wildness as an unbound space has long been contrasted with the orderliness of civilization culturally and aesthetically, a contrast which bears also on thinking about sex. In *Wild Things* Jack Halberstam traces how wildness has been associated with queerness, and how this association contributes to practices and politics of transgression (Halberstam 2020).

Copulation and other behaviors may become relevant to the VGL insofar as they impact reproduction, but an account of their workings requires a recognition that, as Jean Luc Nancy puts it, "making love does something other than make a baby, even when it does that." In his account of the excesses of love, which reach beyond the compunctions of reproduction, Nancy makes the case for the erotic drive as not the imposition of a lower animality when experienced by people, but a "simultaneously very new and very old figure of what has always exposed living beings to a surplus of life." The abyss and the violence of sex, he writes, bring sexual beings, human or not, to an opening past itself, a constantly renewed excess not captured by what may or may not be reproduced in the act. Like the splitting open of a seed pod, the dehiscence of sex offers "a polymorphic, general, and purposeless excess", "linked to the arousal of life that desires itself as a relation between living beings, their generations and their genres" (Nancy 2021).

Nancy's comments apply not only to human beings, but also to the "supplementary dimensions" of sex among animals, which cannot be reduced to arousal with the goal of reproduction. Debate emerged almost as soon as Darwin published his *The Descent of Man and Selection in Relation to Sex* about the profusion of beauty and variety in animal life, and the utility of viewing such excesses through a functional lens. In that text, Darwin argued from the example of extravagantly colored birds that female choice was a significant driver of evolution, outside and apart from pure fitness. A more beautiful bird may have an advantage in sexual selection, even if his beauty does not reflect an advantage in natural selection. Alfred Russel Wallace was one of the first to argue, against Darwin, that sexual selection did not exist apart from natural selection, and that the more beautiful bird is simply honestly signaling his superior fitness to female mates, an opinion which Wallace believed made him a more "pure" Darwinist than Darwin himself. Darwin, however, resolutely refused Wallace's synthesis of the two, insisting on a role for sex and desire themselves, not only as implementation mechanisms for natural selection of the fittest individuals (Prum 2012). Much of the debate around natural and sexual selection in biology today revolves around the possibility of female choice or, more specifically, female aesthetic preferences. Female choice remains mysterious to biologists, especially after the "promiscuity crisis" of the 1980s, when new genetics techniques showed biologists that females of many species, especially of birds, which they had believed were monogamous were, in fact, having sex with multiple males. The upending of this assumption is occurring in mosquito science as well, with significant effects as we can see from the investment of the VGL into the design of experiments to study multiple mating, its patterns and its consequences. Through these conversations and debates in biology more broadly and in mosquito sciences more narrowly we can see how sex, as distinct from reproduction, emerges as a problem of excess and unpredictability.

This has significant implications, for entomologists, for taxonomy. Mosquito speciation is a messy business. The stance that species are convenient heuristics, rather than "real" objects describing preexisting types has been pointed to as one key element of a shift from "typological" to evolutionary "population" thinking in the beginning of modern biology (Amundson 1998). This status of the species concept is much more visceral for entomologists than for specialists of large mammals, for example, for whom interspecies breeding or hybridization is not a very common phenomenon. There are over 3,000 species of mosquitoes in the world, many of which are morphologically identical to each other, many of which have contested or unclear names or categorizations (due to a bifurcation between British and American taxonomical standards which, according to Anton, are largely a product of a small number of acrimonious personal relationships), and many of which do or might mate with each other, potentially producing fertile offspring. For the vast majority of mosquito species nobody really knows because research funding is understandably concentrated overwhelmingly in those species which are primary vectors of human disease. But even within these species, a considerable ambiguity remains. For example, *Anopheles gambiae* and *Anopheles coluzzii* used to be the M and S forms (or *sensu stricto* and *sensu lato*) of the same species (Hanemaaijer et al. 2018). Now distinguished, they will sometimes mate in the lab although probably, maybe, researchers think, not very often in the wild.

Even once species distinctions are clarified in publication, confusions arise. How do researchers know what species something is? They primarily use genetic sequences to answer this question (because mosquito morphology is so difficult even for the species that are officially morphologically differentiable), usually with reference to the CDC MR4 repository of archetypical reference bodies. Researchers are sometimes suspicious, however, that these reference bodies have themselves even been classified correctly. Researchers also have moments of doubt when they are not sure if they are looking at a member of one species with some introgression (meaning some genes from another species with whom some of its ancestors have mated in the past) or a hybrid (a first or second generation result of such intermatings) or another species altogether. Or there could have been an issue with the sequencing protocols, or the machine, or the organization of data in their files.

In their practices of speciation, researchers see themselves as constantly striving to put some order to the overwhelming plasticity of mosquito life. Promiscuous and feral, the adaptability and mutability of mosquito life is what holds them so tenaciously on to survival in the face of eons of human work to annihilate them. This plasticity is seen to emerge from copulation as it results in reproduction, mixing, adding, and hybridizing mosquito types to form more of a continuous plane of variation than a clear grid of differentiable species types. We can see how gene drives depend upon copulation as the most basic prerequisite of their action in reproduction. Intervening upon reproduction is seen as more efficient, durable, and elegant because the work of finding other mosquitoes is delegated not to humans and their clumsy hands but to the deft and nimble work of mosquito desire. The fieldwork practices of addressing mosquito copulation, however, serves as a reminder that these desires are not only inevitable givens, and that they do not operate in predictable ways. Rather, sex itself emerges both as the animating force of this lineage with intent work and as a potential threat to it. For example: what if female mosquitoes somehow “know” they have mated with a GEM male and “correct” by re-mating with a wild-type?

Conclusion

Sex, in the fieldwork practices of the VGL, is more than reproduction alone. But the interface between sex and reproduction mirrors another important interface they engage in their work: that between laboratory and field. The informational recombination events that reproduction is translated into by data and, mathematical modelling of this data, are connected, workers in the field remind their colleagues in the laboratory, to real events of copulation. Events that may not happen when mosquitoes refuse. Or which may happen between strange or unexpected bedfellows. The plasticity of mosquitoes and other insects has provided a rich terrain for thinking not only about biological adaptation, but about intelligence, emergent and distributed effects, and topologies of intensities and potentials in motion (Parikka 2010). One of the intellectual affordances of thinking with mosquitoes is precisely their unpredictability: mosquito scientists engaged in all parts of this trial concede that the insects are not knowable as predetermined essences. More than most other organisms, mosquitoes are constantly in motion, changing in interaction with changing environments.

It is this plasticity which has made mosquitoes particularly hard to eliminate, or even eradicate. Easy to kill as individuals, they become nearly indestructible as distributed masses. They mutate, hybridize, introgress, and otherwise acquire new genes, and with them new bodies and behaviors, in response to all kinds of changes in their environments. Mosquito environments, of course, are people. Mosquitoes share this plasticity with the plasmodium parasite itself, also notoriously difficult to kill, despite current advances towards a vaccine (“WHO Recommends Groundbreaking Malaria Vaccine for Children at Risk” 2021). Despite thousands of years of ingenious human efforts to kill or avoid them, mosquitoes are extremely good at surviving, and extremely good at reproducing.

This dissertation points out that the UCIMI strategy, like other mosquito genetic engineering techniques, is predicated on the idea that such wily and durable plasticity cannot be directly destroyed by people. There is something about the too small, and too dumb, which will always, they say, be too big and too smart for us. Instead, they borrow a set of ideas from a lineage of entomological practice known as biological control, discussed in Chapter One. As we will see, they take from this lineage the idea that drives can be pitted against drives, that the desires of insects can be manipulated as tools. They shift, however, from a focus on predation to one of reproduction.

It is this reproductive focus, for a technology which works by reproduction, which puts the field, and sex, in a newly central position in anti-malarial work. The actual center of this project is not the engineered mosquito. It is what emerges between that mosquito and those of the wild. It is this hybrid future that researchers in the field are concerned with predicting and facilitating. This means that they have to know a lot about what wild mosquitoes are doing, what genes they have, and how they interact with their environments. As we will see, investigations into these questions lead the researchers seldomly to clear answers. More often, it produces a heightened consensus about the simple fact of mosquito multiplicity. We cannot know, they conclude, all of the genes that are circulating in a population, let alone the world. And we cannot know how they will change, on their own or in conjunction with gene drives and malaria refractory genes.

This emergent consensus re-locates what it means to try to build a new kind of a world. We will see in Chapter One how midcentury malaria eradicationists also dreamt of new global terrain, this one freed of mosquitoes. But theirs was a dream of meticulous planning and design, top-down, militaristic and highly controlled. It was based on the idea that strong enough weapons could destroy what could not be known. Today's strategies adopt a different tack: it is by becoming *with* the mosquito that a new terrain can emerge. This involves a subtly but significantly reframed notion of control, which is less about subjugation and force, and more about manipulation, coercion and co-optation. This is not a more idyllic imaginary of being with others. But it is different and, as more genetic techniques emerge in ecology and beyond in the next few decades, we are likely to see more and more projects which, as the United Nations Environmental Program puts it, move from a paradigm of "transforming nature" to one of "transforming our relationship with nature" ("Making Peace with Nature" 2021).

Chapter One: Biological Control

Introduction

In a broad sense, researchers with the UCIMI trial would describe themselves as practicing a form of Genetic Biocontrol, or GBC. This refers to a range of interventions into systems of living things using some manipulation of their genes. Often used for the reduction of invasive species, this technique is predicated on a set of organizing assumptions about populations, species, and their modes of interaction. In this chapter, I explore the history of this technique and way of thinking. Following a short introduction to some of the genetic engineering techniques used by the researchers at Irvine who designed the genetically engineered mosquito to be tested in the UCIMI trial, I offer a short history of biological control, which is drawn as the predecessor of GBC. This history, focused largely in twentieth century California, is contrasted with another way of thinking about unwanted life forms. A description of a campaign carried out on the island of Sardinia just after World War II offers, I suggest, not only a different technique for the reduction of insect pests but a distinct ethos of practice in relation to them. Bolstered in large part by the availability of DDT, midcentury entomology entertained for the first time the idea that human beings might extricate themselves from ongoing interrelation with chosen species and coexist only with life forms they desired. I suggest that these two contrasted lines of work offer not only different techniques for working with insects, but different theories about human control.

Our primary focus in this chapter is on this notion of control. Historical context about competing theorizations of this term help to frame an understanding of how it is used by the

Vector Genetics Laboratory, the UCIMI, and the field of genetic modification of mosquitoes more generally. I suggest that an exploration of the concept of control fleshes out the framework of thought within which this trial operates. I show how these notions developed and changed throughout the twentieth century, and the assumptions which ground this framework. Desire emerges as a central thread linking together biological control and contemporary genetic biocontrol; I show how a growing assumption that insect desires to feed and to reproduce were posed, in various ways, as inevitable. Rather than working against such desires, GBC practitioners, like advocates of biological control before them, suggest that a repurposing of such desires is not only a more elegant solution than the extinguishment of insect life directly, but is in fact the only durable way to combat unwanted insects. Especially as contrasted with the practices and imagery of mid-century eradicationists, the limits of intentional rationality come to the fore in discourses about this strategy. I show how this grounding assumption changed from the inception of biological control as a paradigm in the early twentieth century to the emergence of genetic biocontrol as it looks today. Whereas the founders of the field described inevitable desires as the vehicle for a control wielded by an appropriately balanced Nature, today's practitioners make little reference to the term. Instead, this naturalized desire is more often phrased in terms of "selfishness." We will see how gene drives themselves are said to work through such selfishness, posed as natural both to genes and to organisms. In being severed from what was once an important conceptual grounding in the idea of "Nature's balance", this desire which morphs into selfishness also changes quality. It is not thought to be geared towards some overarching and benign purpose, and the goal is no longer to either restore or create some kind of natural balance. Rather, as genetic techniques moved biological control from relations between species to relations within them, they were also conceptualized as capable of creating entirely new kinds of ecologies with no predictable telos.

Malaria eradication, or the goal of completely removing the human disease from the earth, was sidelined in the later decades of the twentieth century after projects like the one in Sardinia ended in failure. Today, it is back on the agenda. Researchers at the UCIMI, along with scientists and organizers involved in other projects (like, for example, that pursued by Target Malaria) believe that new genetic techniques like gene drives can make it possible to either eliminate or transform mosquito populations in order to interrupt the cycle of transmission which continues between us. Like their predecessors in the DDT-oriented malaria eradication paradigm, they envision a break with what is conceptualized as an age-old fight between people and mosquitoes. Malaria, they will remind us, is at least as old as the human species and so are its vectors. The relationship between people and mosquitoes which plague them and spread disease is often written in the tenor of a grand epic, reaching back as far as the mists of history. With the advent of genetic techniques like CRISPR and gene drives, conversations shelved in the 1970s have reemerged with significant changes. We may be at the end of this fight, in which high-tech ingenuity extricates humans from this unwanted intimacy. Unlike previous iterations, which were predicated on a war-oriented approach to severing this connection by obliterating the enemy, however, contemporary approaches take from the lineage of biological control the idea that this is to be achieved through the manipulation of relationships. I offer in this chapter a playful contrast of imagistic metaphors: while we see mid-century eradication organized like the American military of the 1940's, a more apt figurehead for today's strategies may be found on the logo of the Vector Genetics Lab. Their mugs emblazoned with "mosquito mafia" show a cartoon of a mosquito wearing a fedora, smoking a cigar, and brandishing several large guns. I use the contrast of these images to highlight the significance of their language of “selfishness” and to probe some of its implications for the larger frameworks on which they build, and which they alter through practice.

About CRISPR and Gene Drives

CRISPR is an acronym for clustered regularly interspaced short palindromic repeats. These non-consecutive repeated segments of DNA were first recorded in 1987 by Yoshizui Ishino and his colleagues at Osaka University in the genome of Escherichia coli, although they were not yet known by that name nor was their function clear. Dutch researchers in the early 1990s noted similar repeated segments in tuberculosis bacteria, and in 200 Francisco Mojica at the University of Alicante in Spain recorded findings of these interrupted repeats in 20 species of microbes. He and his collaborator Ruud Jansen coined the acronym CRISPR to distinguish this phenomenon from other examples of repeated sequences of genomic code. A breakthrough came from the field of yogurt science, when Radolphe Barrangou showed that streptococcus thermophilus, a bacteria used in the culturing of yogurt, became more resistant to bacteriophages the more times it was exposed to them; he showed that the bacteria incorporated additional non-repetitive CRISPR segments which he surmised allowed the bacteria to “recognize” and more efficiently “defend” against these bacterium-parasitizing viruses. The yogurt producer Danisco, later purchased by DuPont, standardized the production of this more bacteriophage-resistant culture. In 2005 researchers showed that the DNA sequences between the repeated segments were from viruses that had previously attacked the cell, and Mojica predicted that this could be used for “target recognition” in defense against viruses.   CRISPR thus came to be articulated as part of a complex immune response among microbes, who use it to "identify" or "recognize" their "enemies" by incorporating segments of their genetic code. Jennifer Doudna and Emmanuelle Charpentier used a simpler CRISPR system engineered from Streptococcus pyogenes to create an RNA molecules that could locate and cleave specific sequences of DNA. This system used the protein Cas9 which contains two molecules which jointly act as a "guide" RNA. This guide RNA brings the Cas9 to a specific spot in the genome, where it "cleaves" or cuts the DNA, which the cell then attempts to repair. For this they jointly won the Nobel Prize in Chemistry in 2020. The system they proposed could be flexibly re-programmed to target potentially any sequence of genetic code, providing a much easier and more precise way to edit genetic code. Around the same time, groups led by Feng Zhang at MIT and George Church at Harvard published on gene editing using CRISPR Cas9, launching a protracted battle for patents and intellectual property and credit.

CRISPR as a technique made easier and more accessible the propagation of what are sometimes called "selfish genetic elements", here known as gene drives. These genes are passed on to a greater proportion of offspring than would be expected under normal Mendelian inheritance. Researchers say that this happens when a sequence of DNA (an allele) copies itself to take the place of the other parents' allele, thus ensuring its presence in the genome of the offspring. Instead of being heterozygous for the allele, the offspring is homozygous for the gene drive allele- meaning that both copies are the same. In both scientific and popular language, the genes copied twice in this way are seen as "cheating" to "get ahead." This phenomenon has been recorded since the late 1880s (Austin, Trivers, and Burt 2009), and was posited as potentially useful for mosquito control in the 1960's (Craig, Hickey, and Vandehey 1960). Chris Curtis made the first mathematical model in the late 1960's showing how "desirable" genes, like ones which could hypothetically make mosquitoes non-infectious, could spread through a population to achieve fixation, or permanent abode in the genomes of its members (Curtis 1968). Tony cites Curtis' work as foundational to the UCIMI's project, although it took several decades of biological research for researchers to create a mosquito that could achieve the things described in his models. Tony spoke at a UCIMI meeting in 2019 of the "hubris" of the late 1980s and early 1990s when geneticists in his field thought this mosquito could and would be created "de novo" that is, from whole cloth. This is not an avenue pursued by any laboratory in mosquito genetic engineering today; like the UCIMI, all such mosquitoes are rather edited versions of existing mosquitoes, the complexity of which was, to Tony's mind, underestimated in the later decades of the twentieth century.

Several decades of basic biological research coalesced around what came to be known as gene drives in the early 2000's (Burt 2003). The availability of CRISPR as a tool for editing arrived on the scene to make genetic editing much easier and more precise, catalyzing the study of gene drives in mosquitoes relatively quickly. CRISPR can be used to make a "drive allele" that cuts a part of the chromosome of an engineered organism's sexual partner, replacing it with another copy of the drive allele. In this way, the drive allele is inherited by not half of an engineered organism's offspring, but all (or nearly all) of them. So, if an insect produced 100 eggs, instead of half of them having the allele, perhaps 99 of them will. If enough engineered insects are released to mate with un-engineered (known as "wild type") insects, this means that the next generation of insects is going to consist overwhelmingly of individuals with this drive allele. What percentage of the population will have this allele, and how long it will take to reach that percentage, depends on many variables and is an area of research addressed in Chapter 5.

Valentino Gantz, then a postdoc of Tony James' at Irvine, showed that a gene drive system produced using CRISPR could hypothetically spread malaria resistance through wild mosquito populations, and presented a prototype of how this might be constructed genetically (Gantz et al. 2015). Gantz showed that a gene drive system could be very efficient at spreading a gene which confers resistance of Plasmodium falciparum, a strain of the malaria parasite, among the malaria vector *Anopheles stephensi*, the primary vector of malaria in India. The goal of his research had always been *Anopheles gambiae*, the primary African vector of malaria, but preliminary research was done on *stephensi* for bio-safety reasons (should any escape, *stephensi* were considered unable to survive in California, while *gambiae* were considered able). The researchers created a transgenic version of the *Anopheles stephensi* mosquito whose DNA contains both the gene drive system and the drive allele (in this case, the gene conferring falciparum resistance). When the transgenic mosquito mates with a wild-type mosquito the drive allele copies itself onto the wild-type chromosome. The offspring thus has two versions of the drive allele, and none of the wild type allele. This strategy is known as "population modification." Gantz's work was taken up by researchers at Irvine and elsewhere as proof that CRISPR could be used to make effective gene drives that could transform a population. In this case, it suggested that a release of these engineered *Anopheles stephensi* into a population of ordinary *Anopheles stephensi* would yield, through mating, a new kind of a population after some amount of time: one in which all mosquitoes in the population have resistance to *Plasmodium falciparum*, the most dangerous strain of the malaria parasite.

Population modification is one of two ways of using gene drives on mosquitoes: the other is known as population suppression. In this strategy, a gene drive system is used to spread an allele that reduces the fertility of the mosquitoes (this can be done, potentially, in a number of ways). This strategy would thus yield not a different population, as in population modification, but a smaller or even nonexistent one. This is the strategy pursued by Target Malaria, a research consortium founded by the Gates Foundation which is also proposing to use gene drives to interrupt the circulation of malaria parasites between people and mosquitoes. Population modification works by changing mosquitoes, population suppression by getting rid of them. Both are aimed at reducing the number of people who are bitten by plasmodium-infected mosquitoes.

A Short History of Biological Control

The technique of using gene drives to alter, reduce or eliminate mosquito populations is considered by my interlocutors a form of "genetic biocontrol." Genetic biocontrol, or GBC, is posited as an updated version of an older tradition of "biological control." This refers to a method of indirect control some humans aim to exercise over other organisms. Rather than applying some force directly-- by killing or using insecticides, for example—biological control is a framework through which people aim to affect other organisms by manipulating relationships they are already in, usually sexual or predatory. Biocontrol, or biological control, is a framework with a long history; practitioners of genetic biocontrol present themselves as doing, in theory, a similar thing as these predecessors but with more advanced techniques. Drawing on a history of thinking and practice related to interventions into the "natural relationships" of unwanted forms of life, they posit that the advances in genetic engineering described above can move such work into the insides of the organisms they target. The gene drive practices described here propose to harness the sexual and reproductive behaviors of mosquitoes to change or eliminate them, considered more efficient than hunting down and killing mosquitoes by human hands. But the idea of harnessing "natural desires" of pests, especially insects, for human ends, is traced much further back in history.

Prehistories of biological control cite evidence of the use of domestic cats for rodent control as early as 4,000 years ago. Medieval Yemeni date growers transported predatory ants from nearby mountains to oases where they predated another species of ants that fed on date palms. Citrus growers in China placed nests of predacious ants, *Oncophylla smaradina*, in their trees to eat other pests that fed on the crop. Any evidence of people using some organism to predate a different organism they do not want can, potentially, be recruited to these pre-histories of biological control, which did not exist as a concept before the twentieth century, but which drew extensively on evolving thinking about organisms and their relationships which developed through the seventeenth and eighteenth centuries.

A growing interest in insect parasitoidism in the seventeenth century is cited as an important precursor. The Italian naturalist Ulisse Aldrovandi recorded his observations of the cocoons of *Apanteles glomeratus* attached to the larvae of the imported cabbage worms. In his 1662 *Metamorphosis and Historia Naturalis*, Johannes Goedart included illustrations and descriptions of insects parasitizing each other. Antoni van Leeuwenhoek described a similar parasitoidism of aphids, and Antonio Vallisnieri became the first to explicitly describe the relationship in his description of the association between the cabbage butterfly and the parasitic wasp in 1706. Vallisnieri's contemporary and correspondent Diacinto Cestoni referred to the two as "cabbage sheep" and "wolf mosquitoes", analogizing their relationship of predation. Throughout the eighteenth-century interest in parasitoid biologies grew, especially as associated with the silkworm industry and suggestions of how to combat its attendant pests. 1762 saw the purposeful importation of the mynah bird from India to Mauritius, where it reduced the numbers of locusts feeding on agricultural plants.

The publication of Thomas Malthus' *An Essay on the Principle of Population* in 1798 catalyzed a theorization of such relationships as issues of populations. This moved focus from individual organisms and their bodies and towards groups of organisms and the sizes, growths, and densities of those groups. Erasmus Darwin wrote about the possibilities of using syrphid flied and ichneumonid wasps to control cabbage feeding caterpillars, and Kirby and Spence (1815) demonstrated the use of predaceous coccinellids for aphid control. In 1826, Hartig proposed In Germany the gathering parasitized caterpillars in order to capture these ichneumonid wasps and release them as adults. Ratzeberg published in 1828 a volume on the parasitoids of insects in German forests, and Augustino Bassi showed in 1834 that the microorganism *Beauvaria bassiana* caused disease in silkworms. Kollar wrote in 1837 of the importance of insect-eating insects to "nature's economy." In 1840 France, Boisgiraud collected and released masses of carabid beetles to eat the larvae of the gypsy moth. In Italy in 1844 Villa demonstrated the use of carabid and staphylinid beetles to rid gardeners of pests (P DeBach and Rosen 1991).

The term "biological control" was first used by Harry Scott Smith, superintendent of the State Insectary in Sacramento, in his 1919 article in the Journal of Economic Entomology "On some phases of insect control by the biological method" which he defined as the use of "natural enemies" introduced or otherwise manipulated to control pest insects (H. S. Smith 1919). Biological control took off as a major area of research in the UC system for the next several decades, primarily at UC Berkeley and at its research center in Albany, California. Practitioners focused on searching the world for such "natural enemies" and researching their potential uses to control insect pests in agriculture. The Californian tradition of thinking in this way, however, predates Scott's coinage of the term by several decades.

The practice of identifying "natural enemies" and importing them took off in the mid-nineteenth century American West, as the gold rush and a rapid expansion of agriculture brought a flood of new people, plants and insects into the expanding Western frontier. Between 1840 and 1870 California agriculture grew rapidly, thanks in large part to the importation of seeds, seedlings and cuttings from other parts of the country and the world. With such importations, however, came new insects, some of which caused disease problems that significantly reduced agricultural profitability. The idea began to emerge among Californian horticulturalists and entomologists that the pathological effect these newcomers were having on farmers was due to their status as newcomers- what would later come to be called "invasive species." They theorized that, without existing "natural enemies", these pests were able to increase rapidly in number. This was based on the assumption that populations of any species are ordinarily kept in check by limits to their growth like running out of food or being eaten by predators. They suggested that the latter check was more impactful for insects than for larger animals like us, because of their small size, rapid reproduction, and the abundance of foodstuffs from their species perspective. The idea gained traction that "many of the insect species damaging American crops were 'exotic', that is, introduced from abroad. Unlike indigenous ones, exotic species lacked their usual complement of natural enemies from their country of origin" (Palladino 1990).

Robert Van Den Bosch notes in his discussion of the history of biological control that, although he dates the emergence of the practice to the 1890s and the term was not coined until 1919, several requisite concepts were gathering during the seventeenth, eighteenth, and early nineteenth centuries that would contribute to a world in which biological control made sense as theory and practice. Among these he identifies foremost the concept of population. From this modern conceptual follows the idea of the numbers of populations, their densities, their growths and limitations. And from this in turn, the idea of relations between population, and the notion of enmity as occurring at the species (rather than simply the individual) level. Cestoni's reference to "cabbage sheep" and "wolf mosquitoes" is often cited as exemplary of this gradual shift. These concepts emerging through the eighteenth and nineteenth centuries coagulated to offer a Foucauldian "surface of emergence" for the idea of such populations as forming systems amenable to abstraction and to manipulation (van den Bosch, Messenger, and Gutierrez 1982).

Foucault notes in *Security, Territory, Population* the transformations of knowledge and practice which population as "this new personage who makes a remarkable entrance" in the eighteenth century created. With the mercantilists of the seventeenth century, he tells us, we see the beginning of a modern concept of population as a natural phenomenon driven by the collective forces of individual desire. The concept of population made possible the observation of constancy in irregular events with the keeping and analysis of records on births, deaths, and disease. It contributed to a shift from general grammar to philology as language speakers came to be conceived of as geographically defined populations. Abstract phenomena like scarcity became visible as financiers became economists studying the behaviors of producing and consuming individuals, and mankind was joined by the biologically rooted concept of the human species composed of its diverse populations. The remarkable entrance of the population concept made possible the forms of thought that would later come to be called "biological control." The Malthusian heritage its proponents claimed for themselves in the mid to later twentieth century drew them in to this lineage of taking aggregates as objects of study with their own drives and dynamics visible through a series of mediating efforts, from recorded observations to mathematical modelling. The view from population also provided purchase for geographical definitions of organism through which the concept of "immigrant pests" which were increasingly becoming an area of concern in nineteenth century California could become legible. Populations are linked to places. As threads of thinking about such populations evolved in the nineteenth and twentieth centuries, insect populations were increasingly posed as *of* places. At the same time, places were described as constituted, in part, by "ecological communities" composed of populations of several species involved in various relations with each other. For biological control researchers, relations of predation were central to their study. They theorized that organisms evolved with and around such relations, which kept them in check. It was the removal of organisms from these relations which they said made "exotic" species potentially threatening. Places and their populations increasingly came to be seen as existing in a state of balance, in which organisms checked each other's growth, but for the interventions of humans which disturbed such balance. This point of view would later be articulated through the notion of "nature's balance."

For nineteenth century Californian farmers and entomologists surrounded by and caring for populations of agricultural plants imported from elsewhere, the relationship between populations as defined in some way by their geographic origins and the growth, death, disease or profitability of these populations in their current context was particularly visible. It was within this situation of massive human and biological immigration that "exotic" insects came to be a matter of concern as immigrants. The eighteenth-century theorizations of populations as driven by the pursuits of individual desires also blossomed into an understanding of such immigrant insect populations as potentially limitless in their capacities to grow without any external checks on this natural tendency. From here, the interactions of multi-specied "communities", defined as assemblages of species living in close and regular association with each other, came to the fore as a mechanism by which such naturally infinite tendencies were stymied. Entomologists at the time suggested that, with so much food and good weather on Californian farms and orchards, predation was the only real check on insect population growth. Without the predators co-evolved with the insects in question in their place of origin, their ability to rapidly reproduce would quickly lead to enormous populations, financially ruinous in their hunger for agricultural plants. The notion of "nature's balance" therefore took hold among people who conceived of themselves as living in a profoundly unbalanced ecosystem. The solution, they proposed, was not to return to some notion of "before" colonization and the importation of plants, animals, and people which had come with it, but to restore the relations of natural enmity that "check" the otherwise limitless desires of immigrant insects by importing insects or other organisms that would eat, compete with, or parasitize them and thus reduce their numbers. Humans could thus create a new "nature's balance" engineered to their own needs but recognizing the basic rules they saw at play in all ecosystems.

The first major success of this growing field of biological control was the campaign against the cottony-cushion scale. This fruit-tree feeding insect was first recorded in Menlo Park in 1868, but quickly extended across the Los Angeles and San Francisco Bay areas, harming the large and growing citrus industry of the state. In 1872, specimens of the insect were sent to Charles Valentine Riley, Chief Entomologist of the US Department of Agriculture, often referred to as the father of modern biological control. Riley thought that the insects closely resembled a similar pest he had seen in Australia, and he surmised that it may have originated in that country, from which much of California's citrus stock passed on the way from East Asia and the South Pacific. By 1888, Riley gathered support from the California State Board of Horticulture and the

US Department of Agriculture to send one of its resident entomologists, Albert Koebele, on a mission to Australia to locate this insect's "natural enemies" with a mind to their importation to California. Koebele located two enemies: *Cryptochetum iceryae* and *Rodolia cardinalis*, also known as the vedalia beetle. Both were experimented upon in San Francisco, in the insectary newly constructed by the California State Horticultural Commission to received and propagate imported natural enemies, and released in Los Angeles, where they dramatically reduced the cottony-cushion scale. "The marked success of the cottony-cushion scale project in California and its extension to many other parts of the world, coupled with its permanency, simplicity, and cheapness, led to enthusiastic support of similar ventures toward the solution of other agricultural pest problems. The method was envisioned as the utopian answer to age-old insect plague problems that have afflicted man throughout his history" (van den Bosch, Messenger, and Gutierrez 1982).

The success of the "vedalia project" galvanized support for biological control as an academic and agricultural project. California developed as a center for biological control research and practices, with academic centers at UC Berkeley and Riverside. The program of this "school" drew from the work of AJ Nicholson who, beginning in the early 1930s, argued for the existence of a "balance of nature" maintained by stable birth and death rates (A. J. Nicholson and Bailey 1935). For Nicholson, "a tightly and precisely woven web of interactions checks the otherwise unrestrained growth of insect populations, and …natural enemies- predators and parasites- played a central role in this web." (Palladino 1990) (256). Nicholson's work was mathematically rigorous and abstract. Nicholson drew on the terminology of control-systems, borrowed from engineering, to propose that insect population density is regulated by a negative-feedback control mechanism "much like a governor on a steam engine or a thermostat in a heating system" (A. Nicholson 1954). To Harry Smith and Paul DeBach, two key players in the rise of Californian biological control, this emphasis on the fundamental role of predators fitted with their views on the origins of pest problems. His theory also offered methodological tools for describing and predicting animal populations and their rates of change, and his notion of "balance" was felt to verify the idea that newcomer organisms disrupted the fine "balance" of a "natural" ecosystem. "Pioneer work on biological control was based on the view that the high population level of an immigrant pest was the result of its escape from a balance of nature existing in its native habitat, and could be lowered by the reconstitution of this balance in the new habitat" Thus, introducing "natural enemies" from other ecologies, where they acted to balance the population growth of the target pest, was seen as a re-establishment of natural balance. "In biological control, therefore, one is concerned not so much with what natural enemies actually do in their native habitats as with their intrinsic properties. Each natural enemy is a single component ecological link in the complex of the original ecosystem. Such properties have evolved in the context of natural ecosystems, but the ecosystems in which natural enemies are now discovered, and the ecosystems in which they are required to provide control, are not necessarily very similar to one another or to the original ecosystem" (Huffaker 1976).

Accordingly, biological control is viewed by many practitioners as "applied ecology", or the purposeful use of what is essentially a natural phenomenon. The researchers did not conceive of themselves as *inventing* something, so much as purposefully *discovering* it. As a term, biological control refers to both to the introduction of natural enemies *and* to the enmities which affect population densities without purposeful human intervention. Control is conceptualized as happening, whether or not it is wielded by humans. As Paul DeBach defined it in his comprehensive 1964 summary of the topic, biological control is "the action of parasites, predators, and pathogens in maintaining another organism's density at lower average than would occur in their absence" (Paul DeBach 1964). "The action" here is undertaken by the parasites, predators or pathogens; it is they who effect some impact on another species. Note the absence of humans in this definition. For DeBach, as for other leading figures of biological control during this time, people's observation, understanding, and even uses of these actions did not fundamentally change them. What organisms were always doing was controlling each other. These relations preexist and are not essentially shaped by human action. People simply create (or not) the conditions for them. Citing a lineage that roots itself in Charles Darwin, and reaches back to Thomas Malthus, biological control practitioners proposed that all populations naturally have a tendency towards out-of-control population growth that is only checked by external factors, most notably for them relations of "natural enmity." Like Nicholson's image of the steam engine or the thermostat, these natural controls are intensified or relent as population densities grow and wane, but their nature and their dynamics do not change. This givenness is considered a reflection of their status as "natural" phenomena. The action over time of these "control mechanisms" creates stable populations of different species in relation to each other-- a "balance of nature." Such balance is always effected by control, the nature of which is unchanged by being human-directed.

The success of the vedalia project was followed by work led by the horticultural commission on the codling moth (1904) using the parasite *Ephialtes caudatus* imported from Spain, as well as campaigns against the black scale and the California red scale. Biological control work also expanded outside of the United States: between 1900 and 1910 there were at least 11 programs for biological control (P DeBach and Rosen 1991). In 1923 California transferred Harry Scott Smith and his team to the University of California Citrus Experiment

Station in Riverside, where a new quarantine insectary facility was built. The move re-situated biological control research firmly within the University of California system, although it continued to cooperate intensely with the state, the agricultural industry, and international organizations. The field boomed into the 1940s but declined precipitously during WWII as the US Department of Agriculture program of biological control lost significant funding to the war effort. After the war, the increased availability and affordability of DDT and other chemical insecticides prevented a resurgence of funding or interest, although a disastrous experiment with DDT in the California citrus industry between 1945 and 1946 (the DDT killed all the vedalia beetles, ushering in a return of the cushiony cotton scale) ensured that UC researchers maintained some government and industry support. Around the same time, a bitter feud split the school, leading to the establishment of a separate laboratory in Albany, near Berkeley. The achievement of independence by former colonies from the British Empire further destabilized the balance of midcentury biological control research. During the first half of the twentieth century, the Imperial Bureau of Entomology facilitated much of the movement of people and insects that made possible the work of Californian biological control specialists, whose basic template of work was to search the world for natural enemies and import them using the bureaucratic infrastructure of colonialism. In 1948 the International Union of Biological Sciences held a conference in Stockholm, Sweden to discuss establishing an international organization for biological control that would restore the administrative linkages degraded by decolonization. Neither the US, the UK, nor the Soviet Union agreed to join.

Biological control did not disappear, however, either institutionally or theoretically. But the assumption of a "balance of nature" which predicated the practice of the Californian school was increasingly criticized as the twentieth century progressed. Canadian entomologists working on biological control, for example, traced their own intellectual foundations back not to Nicholson who proposed the concept of “balance of nature” as the Californians used it, but to W R Thompson, who was critical of the term. Thomson argued that Nicholson’s mathematical focus on birth and death rates elided the potentially incalculable number of factors which affect population density, factors so numerous as to make useless the notion of such a balance. Thompson rejected the grounding assumption of this framework, that “control” was a meaningful activity already maintaining such a balance, arguing that “the notion that observed effects can be ascribed to any definite controlling mechanism is therefore an illusion” (Thompson 1939). Whereas the Californian program of biological control had viewed its work as the re-establishment of balances of nature into what they saw as the unbalanced ecologies of California agriculture, Canadian entomologists like Donald Chant rejected the idea that agricultural ecologies were “natural” at all. According to Chant, “problematic crop losses were themselves a manifestation of the balance” if it existed at all. They moreover viewed as inappropriate what seemed to them an aspiration among the Californians to style themselves as physicists, relying too much on simplified abstract models which determined a narrow number of significant variables from the outset. This debate took place in the context of a general contention among ecologists about how natural populations are regulated. It also took place against the background of increasing social divisiveness around pest control following Rachel Carson’s *Silent Spring* in 1962. In the book, Carsen advocates for biological control as a more ecologically sound method of insect management, and many entomologists were folding a tradition of biological control into what came to be called “integrated control”, which advocated the use of both chemical and biological agents (Palladino 1990). Tensions remained, however, throughout the later twentieth century, between more chemically oriented entomologists (largely funded by corporations producing pesticides like DDT) and those oriented more towards biological control or the mixed methods of integrated control.

Significant criticism of biological control also emerged in the later twentieth century around the dangers of “nontarget effects.” Biologists and ecologists, increasingly attuned to the unpredictability of life forms in new settings, argued that introducing any new organism carried with it unacceptable risks. They point out that the distinction between an “invasive species” and an “introduced natural enemy” is not recognized by the life forms themselves, and offered examples in which such introductions have led to deleterious consequences as the natural desires of populations for infinite growth (Follett and Duan 2012). An “exotic” newcomer, whether intentionally introduced or not, is dangerous because its voraciousness for life might make it into a pest itself. One argument made by proponents of genetic biocontrol is that, unlike traditional biological control, a totally new species is not introduced into the ecological community. Instead, an existing member of that community is changed. If biological control offered a framework for thinking about networks of desires which inhere in species beings, an alteration is significantly different than an introduction because there is no new desire introduced into the system. Rather, existing beings, with existing desires and relationships of sex and predation, are altered such that these desires have new outcomes. Genetic biocontrol, unlike classic biological control, is more focused on sex than predation. Whereas the desires of “natural enemies” to feed could be repurposed in biological control for the reduction of pest populations, here the desires of genetically engineered versions of existing insects to copulate and reproduce can be repurposed for the transformation (or elimination) of that same species. The sexual network within species, rather than the predator networks between them, comes to the fore as the locus of intervention.

Biological control was a field of research and practice oriented around strategies for intervention, especially in Californian agricultural industries. Organizing the interventions offered by this field was a body of theory about insects, plants, and the relationships in which they were enmeshed. Through these relationships, it was understood that the desires of populations placed checks on the growth of other populations. This acted as a natural mechanism of control which maintained ecological balance considered to be natural. In this mode of thinking, it is flows of desire, and not organismal bodies themselves, which are the objects of scientists' attention. Both the theory and the practice of biological control faced significant criticism in the later twentieth century, and the field lost ground post WWII with the advent of new insecticides like DDT. The concepts, assumptions, and frameworks of thought developed through this work, however, continue to have widespread salience. Here I focus on the fact that practitioners conceptualized their work as manipulating phenomena they considered natural. This is important, because it framed how they located this approach within the "age old insect plague problems that have affected man throughout his history". It is the naturalness of insect desires, and of their functions within communities of populations, which practitioners claimed made their approach so durable and effective. Within this proposal is a conceptualization of human powers and its limits. The desires for sex and eating of insects is stronger than people. We cannot overcome it, but only use it.

These presumptions will be taken up in discourses around genetic biocontrol and projects like the UCIMI's. In the mid twentieth century, however, they were also challenged by a radically different vision of how the future might look between humans and insects they do not like. Whereas biological control work was predicated on an assumption that desires and relations were inevitabilities, DDT opened up a different avenue of thinking. Part of a wave of modernist utopic thinking about the future, a new agenda emerged in the field of mosquito control. This line of thought suggested that it may be possible to sever, not just manipulate, relations between people and insects. DDT was conceived as a new weapon so powerful that it ejected people from the "age-old" problem of pest insects, and the histories of conflict between man in his species being and insects which plague him that reach back to the misty beginnings of time. That could all be over. DDT as weapon seemed, to some, for some decades of the mid twentieth century, to offer a break with a human past that grappled with other life forms and to inaugurate a new era in which all that lived here was what we wanted.

Sardinia

DDT, or Dichlorodiphenyltrichloroethane, went on the market in America in 1945. Originally synthesized in the late nineteenth century, it came into widespread use as an insecticide during WWII. Shortly before its release to the public, Time Magazine published a breathless story on the new compound which "promises to wipe out the mosquito and malaria, to liquidate the household fly, cockroach, and bedbug, to control some of the most damaging insects that prey on the world's crops. Lieu. Colonel A.L. Ahnfeldt, of the U.S. Surgeon General's office, exclaimed last week: 'DDT will be to preventative medicine what Lister's discovery of antiseptics was to surgery" (*Time Magazine* 1944). Between 1945 and its cancellation in the mid-1970s, about 1,350,000,000 pounds of DDT were released into the US, with usage peaking in 1959 at about 80 million pounds per year ("DDT - A Brief History and Status" 2020). DDT was particularly attractive because it lingered on surfaces: a wall sprayed with it could continue to kill mosquitoes on contact for some months.

DDT also became the central core of mid-century mosquito control work, especially in relation to malaria. The period after WWII and before 1969 was dominated by an eradication agenda largely led by the WHO and the Rockefeller foundation. The midcentury eradication agenda was global in theoretical (if not actual) scope and set for itself a shockingly ambitious goal. Amidst a spirit of optimism, the eradication agenda aimed to completely wipe out species of malaria-carrying mosquitoes. Eradication is, by definition, a grand goal. It is often opposed to elimination campaigns, which aim to get rid of a species of mosquito only in one locality. The ebullient midcentury agenda, by contrast, set their sights on the earth as a whole (Litsios 1996; Packard 2021).

This project was bolstered by several successes. Timothy Mitchell describes in his chapter "Can the Mosquito Speak?" what was considered to be a successful eradication campaign against the *Anopheles gambiae* mosquito. This potent vector, common elsewhere in Africa but previously unknown in Egypt, invaded the country in 1942 alongside British, German and Italian forces fighting WWII. The mosquito was able to rapidly expand its territory of habitation thanks to new irrigation methods, patterns and vehicles of human movement, and changes to agriculture. Along with the new mosquito came *Plasmodium falciparum*- the most dangerous species of the malaria parasite. An epidemic of the disease interacted with widespread malnutrition caused by a wartime shortage of fertilizer to cause massive amounts of death in some regions, especially among impoverished sugarcane plantation workers. Fred Soper, then the preeminent authority on mosquito eradication, advised on a military-style campaign to eradicate the new mosquito using the insecticidal chemical Paris Green. His success contributed to the growing idea that global eradication of the species was possible using "hunt and destroy campaigns and the killing power of the pesticide" (48). The *Anopheles gambiae* in Egypt was, however, a newcomer- an “immigrant pest.” Soper, flush off of his successes in Egypt and, just previously, Brazil, knew that one of the main objections to the feasibility of global eradication was that such newcomers were more easily wiped out than more well-established populations. Only a few years later, he would turn to the Italian island of Sardinia as a test case to prove these objections wrong.

Between 1946 and 1951, he oversaw a project led by the Rockefeller Foundation that sprayed 10,000 tons of DDT mixture onto the island of Sardinia-- just over one ton per square mile. The goal of the project was to fully eradicate the indigenous *Anopheles* malaria vector or, more importantly, to show that it *could* be eradicated. Following previous successes eradicating invasive *Anopheles*, the Foundation wanted to demonstrate that the new powers of DDT were capable of making extinct whole species of life. *Anopheles labranchae* which, together with the endemic malaria it transmitted, had shaped the island’s history since at least the time of the Roman empire (Sallares 2002), was posed as a model capable of demonstrating the potential universal applicability of a new project of eradication- defined as the total destruction of an indigenous species.

For this reason, 32,000 mosquito control workers, organized in military fashion, sprayed DDT not just in the interior walls of living spaces, as was being done elsewhere in Italy at the time (Snowden 2008; Pombi and Modiano 2018). Instead, they endeavored to climb every mountain, reach the center of the deepest thickets, and leave no stone of ancient ruins un-sprayed (Tognotti 2009). The immense and ambitious labors of this task were recorded in a video report produced by the Shell Petroleum Company which emphasized the transformative powers of human will, ingenuity, and militarism. “Sardinia, with its barriers of sea and the problems of its difficult terrain poses the question not yet answered whether it is possible to eradicate abundant indigenous mosquitoes from an area of some thousands of square miles…If an island can be cleared and quarantined, is it practicable to apply similar techniques to rid a continental area? Can it be done?" (Unit 1949)

The goal of the campaign was ostensibly the economic opening and development of the island, but for its organizers it represented a much larger goal: demonstration of the potentially universalizable and scalar concept of eradication. Almost from the beginning, the target of *Anopheles* eradication was questioned by organizers on the ground. Soon after taking over as the Superintendent of the campaign, John Austin Kerr wrote to his boss George K Stroke that he is "of the opinion that the aim of the project there should be changed from the eradication of *Anopheles* to the eradication of malaria." [June 6 1946 letter] Kerr later resigned, and it was said by colleagues of Dr. Kerr that he "never really believed in the feasibility of the eradication of *labrachiae* and never really made a serious effort to try whether it can be accomplished." [September 1, 1947] Kerr believed too literally in the *campaign* against malaria; his superiors needed someone who would work for the *experiment* of *Anopheles* eradication. They did not want to create just an island free of malaria, but one free of *Anopheles* mosquitoes. Their object was not to only to control a disease, but to eradicate a vector. Malaria rates were already falling in Sardinia before the eradication campaign, and Rockefeller Foundation officials recognized that the disease could have been eliminated with only moderate indoor spraying as was being conducted elsewhere in Italy at the time. Kerr's superiors insisted on the continuation of spraying in the face of likely failure for the sake of the speculative future of eradication- not the existing possibility of elimination.

Despite the herculean labors of spraying, by the conclusion of the project Kerr was proven correct and although *labranchae* populations were considerably suppressed they were never eradicated; malaria, however, was eliminated from Sardinia. In the wake of this failure, the meaning of eradication changed in an important way. At the start, eradication meant the possibility of the disappearance of a species from the earth. By its end, Rockefeller Foundation organizers had taken the success of malaria elimination to retroactively redefine the concept to focus on the eradication of the disease, rather than its vector. It was this meaning of eradication which was carried forward in the Global Malaria Eradication Agenda, an ambitious program which shaped global health work on malaria for several decades.

Eradication versus Biocontrol

Eradication campaigns of the mid-twentieth century were military in nature, people conceived as infantry in a war waged against the mosquito. This was total war: the enemy had to be annihilated absolutely and completely as a species for victory to be declared. It is a form of warfare Foucault describes well, in its linkage with the emergence of the population concept:

> "Wars are no longer waged in the name of a sovereign who must be defended; they are waged on behalf of the existence of everyone; entire populations are mobilized for the purpose of wholesale slaughter in the name of life necessity: massacres have become vital. It is as managers of life and survival, of bodies and the race, that so many regimes have been able to wage so many wars, causing so many men to be killed. And through a turn that closes the circle, as the technology of wars has caused them to tend to all-out destruction, the decision that initiates them and the one that terminates them are in fact increasingly informed by the naked question of survival. The atomic situation is now at the end point of this process: the power to expose a whole population to death is the

underside of the power to guarantee an individual's continued existence. The principle underlying the tactics of battle- that one has to be capable of killing in order to go on living- has become the principle that defines the strategy of states. But the existence in question is no longer the juridical existence of sovereignty; at stake is the biological existence of a population. If genocide is indeed the dream of modern powers, this is not because of a recent return to the ancient right to kill; it is because power is situated and exercised at the level of life, the species, the race, and the large-scale phenomena of population." (137)

Like biological control, eradication campaigns dependent on chemicals like DDT were predicated on the concept of population and its attendant tools, methods and frameworks. For entomologists, however, this hardly made them the same. As chemical insect control increasingly came to dominate entomological research, proponents of biological control and the importation of natural enemies felt themselves increasingly sidelined. A divide between themselves and entomologists working with or for the corporations which produced these chemicals hardened as popular sentiment against the widespread use of chemicals (especially DDT) increased in the 1960s and 1970s. Foucault cites both the military and the economy as institutions and model images for the regulation of populations, or biopower. But, in the divide between biological control-oriented and chemical insecticide-oriented entomologists of the twentieth century, important distinctions emerge between the two. While the insecticide branch leaned heavily on military styles of organization and frameworks of thought to yield a "total war" conducted directly against mosquitoes using chemicals as powerful weapons (often likened to nuclear weapons), the biological control party favored much more economic metaphors. They favored the manipulation of natural enemies as a more "natural" approach. Unlike their bellicose contemporaries, they were not proposing to add any object, substance or being that was man-made and thus considered by them to be unnatural. Rather, natural enemies were themselves living organisms- products of nature. And their predatory behavior towards pest insects was also natural- the result of evolution. This they contrasted with synthetic chemicals- man-made, not occurring in nature and, increasingly, seen as dangerous and damaging to the "natural environment." Biological control practitioners saw their work as part of the nature chemicals threatened, suggesting with the phrase "applied ecology" its close proximity with disinterested academic understanding of the natural world. In some ways, biological control practices were viewed as making ecosystem's more natural, as when introduced natural enemies restored "nature's balance" to Californian agricultural ecosystems perceived to be fundamentally unbalanced because of human intervention. While the warfare model of mosquito eradication campaigns sought to decimate populations through brute force, clearing and defending territories and remaking them according to human wills and needs, the biological control model (which, at this time, was not primarily focused on vector-borne diseases of humans, but of agriculture), was seen as a more subtle practice of guiding natural population's towards balanced relationships (which were usually presumed to be consonant with agricultural and economic aims of people). In the distinctions the two drew between themselves in the later twentieth century, tactics were elevated to ethe. Biological control advocates viewed themselves as different from "chemical guys" not only in their approach to the elimination of pest insects, but in their ideas about how nature worked. Foremost among these was the assumption that insect life, driven by indefatigable desires, would always outwit human minds. That the only weapon against such life was life itself, in the form of the desires of other organisms. Rational intentionality was seen as no match for the natural phenomena of populations; this is where they differed from the grand dreamers of midcentury eradication efforts. In the contrast between the two we can see a snapshot of opposing images not only of mosquitoes and their capabilities, but also human beings.

Genetic Biocontrol

In the early twenty-first century, the biological control frameworks and practices described here were transformed into “classic biocontrol” by the introduction of what came to be called “genetic biocontrol.” While the “classic” version involved introducing the “natural enemy” of an organism and hoping that it becomes established and permanently reduces the population of the target pest, genetic biocontrol involves the alteration of an organism’s genome to reduce the numbers of a pest. Importantly, there is no other organism in this equation which serves as the natural enemy to the organism one wishes to reduce or eradicate. The organism itself serves as its own natural enemy. This can be accomplished in a number of ways: through irradiation, parasitic microbes which distort sex ratios, hormonal sex reversals to generate sterile or sexually incompatible genotypes, or genetic engineering like that accomplished using CRISPR. Not all genetic biocontrol programs involve genetic engineering- for example, the widespread use of the Wolbachia parasite to reduce mosquito numbers by distorting their sexual development is not, properly speaking, genetic engineering. Genetic biocontrol precedes the kinds of genetic engineering techniques used by the UCIMI and their contemporaries, including techniques for genetic alteration like irradiation and chemosterilization which have been around since the 1960’s (Pal, R 1974).

While genetic biocontrol is different from classic biocontrol insofar as it does not involve the importation of a natural enemy, practitioners have adopted the name, and situated themselves within the lineage of this thinking and practice, because they see their own work, like those of their predecessors, as dealing with the exploitation of natural relationships for the control and management of unwanted populations. Rather than reducing population density by killing and eating, as external natural enemies of the past, the internal natural enemies of the genetically altered biocontrol agents usually work by intervening in the reproduction of the target organism. The target organism splits into two: what is called the wild-type strain, and the laboratory or engineered strain. It is through sexual congress between the two that the genes "introgress" into the wild-type population: they exist in the genomes of the offsprings of such pairings. This can take the form of population suppression strategies (which work to stop offspring from being conceived, born, or surviving), or population modification strategies (in which offspring do survive, but carry genetic changes like the malaria refractory gene which alter how the population interacts with other species of organisms, including people).

War v Mafia

The Vector Genetics Laboratory some years ago produced a number of mugs and t-shirts with a sketch of their informal logo. It shows us a mosquito smoking a cigar, wielding two large guns, and wearing a fedora. Emblazoned across bottom are the word "Mosquito Mafia" in aggressive looking lettering. The logo speaks to the organization's overall purpose of violence against mosquitoes, but its imagery might also serve as a playful entrance into thinking about the specific forms this violence takes. We have seen how biological control emerged as a concept in the early twentieth century which advocated the importation of “natural enemies” to kill and eat pest insects, usually “immigrant pests” (later called “invasive species”) with the aim of restoring a “balance of nature.” This strategy made sense in a world already structured by the population concept, in which insects could be considered as aggregates, numbered, and abstracted into graphological representations showing the changes of those numbers over time. We have also seen an example of midcentury eradication strategy, a practice of hunting down and destroying mosquitoes and their breeding sites using martially organized people and synthetic chemicals. This practice, too, was made possible by a population-organized world. I have suggested that an important part of the distinction biological control proponents drew between themselves and those enrolled in this war was that they saw their own practice as more natural. Whereas eradicationists directly killed insects (or larvae or pupae) by poisoning them, biological control has often been considered more “elegant” insofar as causation of death is indirect. Rather than people killing organisms, organisms can be made to kill each other. This process harnesses what has been recognized since the eighteenth century as the driving force of populations: desire. The desires of predators can be put to work as an external check on the desires of pest species which, unregulated, would lead to population growth that damaged agricultural products. In genetic biocontrol, the natural enemy is inside the house as it were: a genetically altered version of the unwanted insect acts on its kin within the population to reduce its numbers or modify its nature. Like classic biological control, it is about the manipulations of what are conceived of as natural desires- to eat and to reproduce- to achieve human ends. Michel Foucault cites both the economy and the military as institutions of the regulation of biopower; while both eradicationists and biological control strategists worked within the same histories of thought he describes, the former are much more military in aesthetics and practice, and the latter much more economic. Theirs is not a practice of violence, explicitly, but of regulation of "nature's economy."

Michelle Murphy, in her thoughtful critique of the population concept *The Economization of Life* traces the intimate linkages between population and the economy. Twinned concepts, they emerged in tandem and are often theorized together. Murphy opens with the image of *drosophilia* flies kept in bottles by the American biologist Raymond Pearl. Pearl recorded the numbers of flies in his bottles as they reproduced and later died and expressed his recorded tallies in graphical form to produce what is now often called an S curve: an expression of the natural rate of growth and decline for any population. Murphy is concerned in this text to point out the ways in which economy has served as a metaphorical glass bottle for our thinking of populations: she shows us "economy as atmosphere", so natural as to act as the nearly invisible glass walls limiting our thinking of the world, "a contained existence that ends in extermination" (Murphy 2017b).

For Murphy, there is a violence encompassing the notion of population itself; a presumption of scarcity, and an intimation that we are too many. Behind these notions is the long arc of racist, eugenicist and colonialist projects to control populations of people. Those same projects, of course, shared tools, theories, and frameworks with entomology. The two disciplines have never been far apart for long. For both eradicationists, waging total warfare on the mosquito and for the biological control proponents manipulating its relationships, natural facts about insects were grounded in the same truths held stable for people. Both sets of practices emerged out of and linked with broader ideas about nature and populations which were also applied to human beings. Mosquito biologist Marston Bates, for example, was asked by his boss at the Rockefeller Foundation George K Strode (of the aforementioned Sardinia campaign) to use his expertise with mosquito populations to make a study of "the human population problem" which was later published as *The Prevalence of People*. "We were living in a time when some people were beginning to question the philosophy and value of public health work as now carried out. He and other public health men were increasingly subject to jibes about public health being a 'bad thing'— sometimes jokingly, sometimes half-seriously, sometimes in deadly earnest. Public health, by forestalling death through disease, might be creating a new problem as it solved an old one, might simply be providing more tinder for death by starvation in the overcrowded countries of the world, might be changing the disease problem into a population problem. Was there any truth in what the alarmists said? And, in any case, could we deal with public health as a separate sort of enterprise, a thing-in-itself, apart from the economic and social problems of the peoples concerned?" (Bates 1955)

Ideas of populations and their control have flowed between people and insects for nearly as long as those concepts have been around. In the case of mosquitoes, ideas about how to decimate populations, to clear territory, and to engage in total war which leaves no survivors were imported, relatively directly, from plans for human beings developed in the mid twentieth century. Observations of insect population growth and decline like Pearl's have been used to make statements and predictions about human populations, their problems, pathologies, and futures. And the balance of nature sought by classic biological control borrowed much (and lent much) to generalizations about what makes a healthy economy. The economy of the mafioso is not part of a healthy economy. Natural, yes, insofar as it is driven by the natural desires which undergird both economic and ecological notions of population. But the criminal economy, by operating outside of the law, perverts the market forces applicable to above-board actors. Its growth, fueled by a closer and less regulated association with human desires, is figured as feral, intense, and potentially unlimited. The figure of the mafioso, as it functions as a cultural symbol, is one of dangerous charm. He does not abide by the rules of engagement required for war: he is a cheat. The language of cheating is prevalent in popular-audience summaries of gene drives. The process by which a "selfish" genetic element "rigs the competition" is said to "cheat" the "fair competition" of normal Mendelian inheritance ("Gene Drives Explained: How to Solve Problems with CRISPR" n.d.). By "selfishly" forcing itself into both of an offspring's alleles, the gene drive is described as "cheating evolution" (Champer, Buchman, and Genetics 2016).

This "cheating" is a natural phenomenon to the scientists engineering gene drives. The "selfish genetic elements" recorded in the 1950s were observed, not made. They are posed as "naturally occurring." Scientists working with the genetic engineering of gene drives see themselves as harnessing this natural selfishness for human ends. This links their practice with classic biocontrol strategies. When entomologists imported the vedalia beetle, for example, to reduce the populations of the cottony cushion scale they saw themselves as harnessing a desire that was naturally occurring- the desire of the vedalia beetle to feed on the scale insects. This natural desire created, at an abstract level, a population dynamic. The numbers of vedalia beetles and of cottony cushion scale insects could be visualized in graphs, which produced for entomologists a relationship between the two species' populations (as numbers). This relationship was seen as emergent from the more concrete relationship of killing and eating happening in citrus groves. The desires of each insect (and, in aggregate, each population) were figured as selfish. Population growth, fed by selfish desires, was only kept in check by the selfish desires of others (specifically predators).

In genetic biocontrol, the attribution of selfish desire moves from the organism to the gene. Working within a gene-centric framework this way of thinking takes the gene itself as the unit of analysis and the organism as the medium through which it achieves its “desires.” In this parlance, all genes “want” to be inherited; cheaters pursue this desire outside the rules of engagement. Desire as emotion has no salience for either genes or insects to the entomologists mobilizing this concept. Neither insects nor genes are thought to think and feel in recognizably human ways. But the anthropomorphism is thought to indicate a deeper truth about nature: all beings seek out their own advantage. Like a market economy, the balance of nature sought by classic biocontrol was a system of checks instituted on organisms’ desires by the desires of other organisms. The balance of nature concept appears little, if at all, in literature on genetic biocontrol, however. Here, the goal is not the establishment of a “balanced” agricultural ecosystem. Balance falls away as a significant concept, partly because the idea that nature tends towards balanced stasis if unmolested by human intervention came under increasing scrutiny in the later twentieth century, and partly because the frame of action is no longer the dynamics between species but the dynamics within one.

Instead, the organism itself takes the place of the ecology in this formulation. And it is not an ecology that seeks towards balance. In either population suppression strategies or population replacement strategies, the goal is total takeover. In some respects, this framework is similar to the “war” mentality of the eradicationists: the vector species is to be wiped out (or transformed) completely. The eradication agenda was largely abandoned in the later twentieth century in the face of frequent failures. While some countries did eliminate malaria vectors, the example of Sardinia suggested that chemical control was fundamentally incapable of eradicating, and keeping eradicated, an indigenous or well-established mosquito species over time even on one island. Sub-Saharan Africa, where the majority of malaria cases were recorded then and continue to be recorded now, was not even part of this global agenda. Moreover, eradication attempts which failed (often through inconsistent funding) sometimes create more disease and death as, for example, in Zanzibar where a WHO led eradication campaign ran out of funding before *Anopheles gambiae* was eliminated but after the local population lost their acquired immunity to the disease. Without this immunity, thousands more people died of malaria than likely would have had the intervention never happened (Graboyes 2015). Eradication, and the exuberance which accompanied its pursuit in the post-WWII era, was largely consigned as a costly failure until 2007, when it made a dramatic comeback with an announcement by the Bill and Melinda Gates Foundation. Started in the late 1990s, the Gates Foundation has become a major player in global public health, and malaria has been its star disease. Its funding on the issue so dwarfs the WHO Global Malaria Program that the former often apply to the latter for funds. Gates announced a pivot from a "global malaria control paradigm" to a "malaria elimination and eradication agenda" (Chandler and Beisel 2017). Global domination was back on the agenda, but with a new and different ethos. Gene drives have been a key part of Gates' strategy of pursuing malaria vector eradication and, by extension, the total elimination of the disease.

This new eradication agenda, however, speaks less of brute force than the "war" paradigm of midcentury. Instead, especially in the use of gene drives and genetic engineering, it is about a manipulation which takes place within the species, rather than against it from outside. The strategy of using transgenic organisms repositions insects from vectors of disease to barriers against it. This repurposing is understood to be a manipulation of dynamics that are fundamentally natural- those of competition and enmity. But now competition is between genes, and enmity significantly more ambiguous within the species. The new eradication links itself with a history of biological control but repurposes some of its core images and metaphors.

This strategy is linked with a broader global One World, One Health agenda which Melissa Salm describes as “an approach to global health that emphasizes the interconnectedness of human, animal, and environmental wellbeing, and it includes a correlative call to examine zoonoses (diseases that spread from animals to humans) vis-a-vis multidisciplinary collaborative efforts that target pathogenic activity at “the human-animal environment interface” (Salm 2020) (ii). Javier Lezaun and Natalie Porter point out in “Containment and Competition: transgenic animals in the one health agenda” that this signals a “different model for the management of human-animal relations.” Rather than trying to contain interspecies contact (by killing mosquitoes, for example), the One Health agenda “assumes that pathogen circulation across species is inevitable; its fundamental premise is that interspecies boundaries are too porous and permeable to circumscribe human health policy to the wellbeing of humans alone.” Unlike the “war” mentality of midcentury eradication, pathological contact between humans and mosquitoes is not pursued topographically, by killing the mosquitoes in areas of human habitation. Rather, genetic strategies aim to “establish an isolation perimeter *within* the body of the host or vector.” As such, “the power to keep infection away will rest on the ferality of these artificial life forms, on their uncontrollable ability- neither fully engineered, nor completely natural- to behave in a ‘wildly opportunistic fashion” (Lezaun and Porter 2015) (101). Capacities for reproduction (and thus population growth) are seen as natural phenomena that can be modulated- ferality itself is framed as natural (both in the desires of the insects to reproduce and the “selfishness” of genes), but it is seen as able to be put to work towards human ends. This shift in ethos is consonant with language in other regulatory spaces—like, for example, calls by the UNEP to move from aiming at “transforming nature” and towards “transforming people’s relationship with nature.”

The figure of the “mosquito mafia” thus acts neither as a general nor towards an idealized balance of nature. Rather, he is a libidinal and opportunistic actor, pursuing his own ends by whatever means possible. For genetic biocontrol practitioners today, the key is in designing a field of action, both within the body of the mosquito and outside of it, in which those means guide his selfishness towards predictable ends. We thus see the development of a new framework for thinking control. Midcentury eradication efforts, modelled on legal warfare, envisioned the decimation of mosquito populations from territories. Biological control strategies proposed instead the harnessing of species’ pre-existing relationships of natural enmity to establish a balance of nature. Genetic biocontrol dispenses with the balance of nature concept, and no longer believes that people can hunt out and destroy every individual of a mosquito population using their eyes and hands and chemicals. Instead, organisms like mosquitoes have natural capacities that can be modulated. Somewhat like the transition from societies of discipline to societies of control described by Gilles Deleuze, in which free-floating, perpetual, and flexible forms of control operate in open systems without end. He locates us in the middle of a shift which accelerated after WWII which is exemplified by the corporation, that “spirit” or “gas” which harnesses the forces of competition and “opposes individuals against one another and runs through each, dividing each within.” From the pair of the mass and the individual described by Foucault we get what Deleuze calls the “dividual”- persons are dissolved into forces, materials, and elements whose dynamics are the objects of this new form of control. We might replace the word “man” in his text to say that “The disciplinary [mosquito] was a discontinuous producer of energy, but the [mosquito] of control is undulatory, in orbit, in a continuous network.” This new logic, which takes on the image of the serpent, shapes not only the systems within which we live “but also…our manner of living and our relations with others” (Deleuze 1992).

The figure of the mosquito mafioso is a player in these continuously self-deforming casts. He is an internal enemy, distorting populations by reproducing with them. He is flexible, adaptive, and feral. This ferality is not to be waged war with but modulated and exploited. The mosquito and the gene are both analyzed through assumptions of avaricious selfishness which take the form of natural laws: this is how things work. They cannot be changed. They can only be guided or manipulated. This is a world under human control, but not under human domination the way Fred Soper or other heroes of the eradication era would have imagined. There is a spreading out of the human into the mosquito, and a reduced relevance to the concept of "natural" that was previously significant to classic biological control practitioners. Instead, there is a continuous world of people and mosquitoes striving for life, reproduction, and expansion, and cheating the rules is the smart thing to do. I suggest that contemporary gene drive techniques both index and contribute to this emergent logic of control. As we will see in Chapter Four, the natural laws through which the mosquito mafioso operate are applied also to what are called "human systems." Almost too obvious to point out, they are agnostic about the boundaries of the human or his exceptionalism.

Conclusion

Control very often means, colloquially, an exercise of force or domination. It implies the subjectification of something to one's will, the overcoming or defacement of another's agency. Its meanings and its tones proliferate across contexts, but here we have traced one use of that word as it emerged with the practice of biological control, and as it changed alongside ecological models, entomological practices, and biological paradigms. It is this notion of control which researchers at the UCIMI are drawing upon when they link themselves with this history, identifying their project as a form of genetic biocontrol. We have seen how, from its foundations, the control of biological control implied a different kind of a relationship than one of domination and submission.

From the emergence of modern biology, it revolved around discourses which posed organism's behaviors and desires as inevitable. For insects, predation and procreation were suggested to be outside of the possible realm of human control. Working from this assumption, biological control as it emerged in early twentieth century California proposed that those natural desires could be co-opted. Farmers and entomologists working in an agricultural landscape destabilized by colonization, imported "natural enemies" to counteract the invasive species introduced by an influx of colonizers into ecosystems whose "natural" relationships had been severed. A shift in attention was effected, from the organisms themselves to the relationships between them. This perspectival shift carried into the frameworks of contemporary population geneticists like those working at the VGL. They see new techniques of genetic engineering as enabling an intensified version of this practice. By putting insect desires to work on themselves, they hope, they can design a field of action such that human goals are met (the eradication of malaria) without the application of direct human force.

This evolving notion of control matters not only because it structures explicit thinking about the possibilities for human shaping of insect environments. It is also predicated on basic assumptions which apply to more than insect life. In the shift from biological control to genetic biocontrol, we have seen, paradigms of thinking subtly shifted. For example, relationships like those of natural enmity (or, we could add, procreative desire) were no longer framed as tools deployed by a hypostasized Nature pursuing balance. Decades of mathematical modelling and theoretical ecology had suggested that a tendency towards balance is not inherent in such systems, and so these desires entered center frame not as a means to a systemic end but as a simple fact. Organisms, like genes, like people, are selfish. We can see how, in the language of gene drives in particular, “cheating” or “selfishness” are naturalized and re-framed as basic tendencies of life. The idea of the UCIMI project is to co-opt that selfishness, at the genetic and the organismal level, to pursue not balance but engineering of complex interspecies systems.

Chapter Two: Wild Resistance

That will allow that lightning flash decision to appear once more, heterogeneous with the time of history, but ungraspable outside it, which separates the murmur of dark insects from the language of reason and the promises of time.

Madness and Civilization

On the seventeenth of July, 2017, a little after two in the afternoon, in a conference room on the fourth floor of the UC Davis Veterinary Medicine Center, members of the UC Davis Vector Genetics Lab were gathered for their weekly Monday meeting. Men and women, mostly in their early 30's, primarily postdoctoral researchers here working on population genetics fiddled uselessly with the screen pulled down over large windows at the back of the room which looked out onto a glare of rooftops and a hazy view of distant mountains parched a uniform beige. The material of these blinds is a loose woven mesh, not opaque enough to block out the late afternoon sunlight which very often falls directly onto the large screen used for PowerPoint presentations. "Whoever designed this building didn't have a lab" grumbled Yoosook, a senior researcher. The mood is downcast, although I hadn't been working with this group long enough to understand exactly what was going on. Hanno Schmidt, a postdoc in the lab, had sent his colleague Mark Hanemaaijer seven genes to be sequenced, and it looked like there was a lot of polymorphisms. This was not good news. "This is going to be a problem" agreed Greg Lanzaro, the Principal Investigator of the lab.

I had met Greg and become acquainted with the work of the Vector Genetics Lab just two months before, when I visited his office to interview him about trends in the field of mosquito genetic engineering. Towards the end of our conversation, he mentioned to me a new "Africa project" to be announced that next week. A consortium of UC Universities, led by Tony James' lab at Irvine, would be aiming to do an open trial of a new kind of genetically engineered mosquito (GEM) on an island somewhere off the coast of Africa. This mosquito, he said, was different from the one I had been learning about up to that point, a "sterile" version of the aedes aegypti mosquito developed and marketed by the company Oxitec to reduce the circulation of dengue, chikungunya and, most pressing at that time, the Zika virus by causing the deaths of its progeny. This new *Anopheles gambiae* mosquito the people at Irvine had developed had something called a gene drive. If the trial were successful, he told me, this could be the first demonstration of a genetic technique that could totally revolutionize the management of any number of organisms. Invasive species, agricultural pests, endangered wildlife, you name it. It may also, should the mosquito make its way into continental Africa, eliminate malaria, a disease that continues to kill nearly half a million people every year, most of them children. Greg said he was particularly interested in the Union of the Comoros, a set of three islands off the coast of East Africa. It's beautiful, he said. White sands, blue waters. A real tropical paradise. And we don't have a social science person yet.

A gene drive, I had learned by this day in July, is a way of spreading a gene through a population of sexually reproducing organisms very quickly. This is accomplished by causing the gene to be inherited by all, rather than half, of an individual's offspring. Researchers refer to this as "super-Mendelian inheritance" in which "selfish" genetic elements increase in frequency. This is so even if they do *not* confer what is called a "fitness advantage." Something that weakens, reduces the fertility, or kills a mosquito confers a fitness "disadvantage", while a genetic trait that allows it to evade death (like insecticide resistance) offers an "advantage." One would expect a gene which confers some fitness advantage to increase in frequency in a population.

Researchers in the broader world of vector genetics speak of gene drives "stacking the deck" in favor of a particular gene without making that gene particularly advantageous for the mosquito in which it is embodied. The survival of the fittest, apparently, can be gamed. This ability for the gene to propagate itself without offering its host an advantage in competition or survival makes it "selfish", "winning out" over other alleles.

The idea of exploiting such "selfishness" to control the organisms around us is not all that new- it was proposed over thirty years ago for the control of disease bearing mosquitoes. But the horizon of possibilities had expanded rapidly and dramatically with the discovery and advancements of CRISPR Cas9, a gene editing tool developed from the immunological strategies of bacteria and archaea. In most descriptions, CRISPR allows scientists to "snip" or "edit" precisely defined sequences of DNA. Scientists in Tony's lab in Irvine had developed a version of *Anopheles stephensi* (the main Indian vector of malaria) and then *Anopheles gambiae* (the main African vector) with a genes making them refractory to the malaria parasite and equipped with gene drive mechanisms that used CRISPR Cas9 to cut specific sequences in the genomes of developing offspring, stimulating the cell's natural repair mechanisms to copy this refractory-inducing gene onto both chromosomes, giving not half but all of an editing individual's offspring this quality. This preferential inheritance would thus, hypothetically, spread rapidly throughout populations of *Anopheles gambiae*, creating a world in which these mosquitoes are not absent but modified. A bite from one would carry no risk of malaria, a disease so ancient and so costly it has significantly shaped the human genome. The massive human cost of malaria made it an obvious choice to "go first" in the court of public opinion. Once gene drives are used in mosquitoes, some thought, it would be much easier to use them for any number of applications, from agriculture to defense.

The problem, at this worried meeting in July, was those specific sequences. For those present, the problem was clear: for the gene drive to "push through" a population, it needs to "cleave" at a "target site", or a specific genetic sequence, in each individual mosquito. This target site needs to be "conserved", or the same in its DNA sequences across individuals. If it is not, the drive's "push" may be "resisted" by drive-resistant alleles (DRAs) harbored by some mosquitoes with alternate DNA sequences. An alternate DNA sequence is known as a polymorphism. Not all polymorphisms are located in the mosquito genome in such a way as to resist the gene drive. When they are, however, they are termed as Drive Resistant Alleles, or DRA's. The concern that Hanno was addressing for the first of several times over the next few years was that many people increasingly doubted whether there *are* any target sites in the *Anopheles gambiae* genome conserved among all individual mosquitoes. His initial findings, after looking for seven possible target sites suggested by the Irvine lab in the sequenced genomic data of wild populations stored at the Vector Genetics Lab, appeared to show that most of these sites had variants. These variations, or polymorphisms, refer to differences within a population. Any population with more than one discontinuous type is polymorphic- for example, eye color and blood type are polymorphic traits in people.

In the seventeenth century, polymorphism referred to any drastic change of form, for example like the metamorphoses of insects, or the dramatic changes in skeletons unearthed by paleontologists. Over time, however, this idea of mutating "forms" has expanded to refer also to the patterns of genetic code, and when Hanno reports finding significant polymorphisms at potential target sites it means that these target sites do not always bear the same sequences of nucleotides. *Anopheles* mosquitoes have about double the number of polymorphisms that humans do, meaning that even within a population each mosquito is much less similar to another than I am to a fellow human. Even compared to other insects, *Anopheles* are astonishingly polymorphic, a trait which has aided them in their millenia-long evasions of human tactics to avoid, reduce, or kill them. Because there is so much standing genetic variation, any tool people use to kill mosquitoes is likely to be resisted by some members of a population- a quality which will rapidly spread if it increases fitness. It is this rapid spread (minus the fitness) which gene drives exploit. Even more troubling is the unpredictability of these differences- there is no way to know before you collect and sequence mosquitoes from a population how many polymorphisms there may be in their genomes, nor where they may be. Both differ significantly across localities. That year, in 2017, a "flurry" of academic papers was published suggesting that this plastic and unpredictable quality of mosquito populations would make the whole idea of gene drives entirely unworkable. As Greg would phrase it later in a blog post for the journal Nature, "critics argue that mosquito genomes in nature carry so much sequence variation… that a significant proportion of any wild mosquito population will carry drive resistant alleles (DRAs)" (Lanzaro 2020). Left unsaid, but heavy in the worried tones of this meeting was the open question: could this jeopardize everything?

Hanno's voice and accent are remarkably similar to those of the German filmmaker Werner Herzog. As he relayed the bad news to his colleagues I would think, as I would[1] many times later, of Herzog against the Peruvian jungle drawling about a land that God, if he exists, has created in anger. The documentary *Burden of Dreams* follows Herzog's Sisyphean efforts to create the film *Fitzcarrald*o in defiance of the resistance he sees posed by the environment itself (like the film's eponymous hero). Both men situate themselves as fighting valiantly against chaotic forces of entropy named Nature to achieve their grandiose visions. I imagined Hanno shifting imperceptibly into this speech about the overwhelming obscenity, the overwhelming fornication of a land which curses whatever he attempts to do there. While his language was far less imagistic than Herzog's, the same chaotic entropic forces were invoked, and the same tension between what Michel Foucault names in *Madness and Civilization* the "language of reason and the promises of time" and the "somber murmur of insects" (Foucault 2003). His tone, like Herzog's, retains its own peculiar cadence regardless of the topic.

> We are cursed with what we are doing here. It is a land that God, if he exists, has created in anger. It is the only land where creation is unfinished yet. Taking a close look at what is around us there is some sort of harmony. It is the harmony of overwhelming and collective murder. And we, in comparison, to the articulate vileness and baseness and obscenity of all this jungle. We in comparison to all that enormous articulation we only sound and look like badly finished sentences out of a stupid suburban novel. A cheap novel. And we have to become humble in front of this overwhelming misery and overwhelming fornication. Overwhelming growth. And overwhelming lack of water. Even the stars up here in the sky look like a mess. There is no harmony in the universe. We have to get acquainted to the fact that there is no harmony as we have conceived it. But when I say this, I say this all full of admiration for the jungle. It is not that I hate it, I love it. I love it very much. But I love it against my better judgement (Blank 1982).

Rebecca Carballar was less tranquil. When Hanno sent her these sequences, she would say later, "I was freaking out!" A molecular biologist at UC Irvine, she had played a key role in designing this drive system, which so far had "pushed" successfully through laboratory colonies in her insectary. Hanno and Rebecca's collaboration was one point along the interface between laboratory and field which this project continually creates, massages and dissolves. Rebecca, at UC Irvine, works in the lab; her attention is on the genetic sequences, and the design of the "Target Product Profile." This "TPP" is an ideal mosquito, still at the time under construction, which could hold in its genes the potentiality for a malaria-free Africa and the first successful trial of a gene-drive bearing organism outside the biosecurity level three walls of the insectary. Hanno, in his association with the Vector Genetics Lab, played the position of the field. Davis' obligation to this consortium from the start was the "fieldwork portion" of this undertaking. They are responsible not for the creation of the ideal mosquito but for the description and management of an environment in which its capabilities might potentiate. Within this domain fell many aspects of the field, and it would gather several more as work progressed: regulation, national politics, impenetrable forests, colonial and geographic histories, local laboratory equipment, unused water cisterns, unusual dogs and "community engagement" all must be tackled in turn by indefatigable Greg. But here, in this exchange between Hanno and Rebecca, the division between lab and field was drawn through the body of the mosquito, specifically the *Anopheles gambiae* of the lab, and the *Anopheles gambiae* of the field (that is, here, the "real world"). These latter they referred to as the "wild-types." With the troubling polymorphisms, *wildness* emerged as a complex problem for the VGL, one uniquely resistant to the efforts of prediction that might knit together the ideal mosquito and a living locality in which it might work. As Greg sighed, when Hanno finished speaking, "Over and over again you design things in the lab, and when you transition to the field things just don't work."

At the precipice of this field/lab divergence is the resistance from the field posed by these "wild-types". How could researchers predict how the gene drive would "behave" as they say in the field if the wild type mosquitoes are too different from the laboratory mosquitoes? How can experimentation on one be applied to predictions relating to the other? Within the conversations surrounding this work on polymorphisms field and lab came into focus in contradistinction around these questions. Polymorphisms became a topic of concern for the VGL because they are not present in the mosquitoes available to scientists for careful, exhaustive manipulation and examination in the lab. They remained unaccounted for- suggested, but not described. Because they are so rare, they are not foreseeable; there is as yet no good way to predict their distribution or proposals that such a thing might be possible. Without creating a Borgesian map coextensive with the territory, it appeared increasingly unlikely that all polymorphisms could be "captured" by sampling efforts. They are simply "out there", as likely in the near infinity of short-lived and rapidly multiplying mosquitoes as life among the innumerable stars. Except that a Drive Resistant Allele, should it exist, will likely blossom in frequency if it offers an escape from whatever fitness disadvantage a gene drive might demand.

And yet, *Anopheles gambiae* is far from "wild" in the sense of being unmarked by contact with humans. On the contrary, their love for the human, what researchers call their intense anthropophilia, is the distinctive mark of this mosquito which, by most estimates, has killed more people than war. Alone in a forest somewhere, without us forever, they would not be what they are genetically, behaviorally, or morphologically. In this sense, *Anopheles gambiae* is technically domesticated, although that word catches in the mouths of even the scientists who know them best. "That doesn't seem right, does it?" said one postdoc some months later at the end of another's presentation which made mention of the long history of *Anopheles gambiae* which is proposed to have begun with the increased human density that accompanied the emergence of agriculture and the subsequent dependence of a previously forest-dwelling insect on human blood. "Is that the word?" Perhaps "cosmopolitan" is more appropriate, a colleague suggested. "It's a hard mosquito to find now in the forests of Africa" Greg added, flashing a glimpse of a small tattoo of it inked into the flesh between his left thumb and forefinger as he rubbed his forehead and closed the meeting.

The threat of resistance from wild-type mosquitoes enters center frame in the crisis generated by these polymorphisms. For all the interest and fear and celebration of the GEM mosquito at the center of this trial, I turn away from it here towards what becomes in this relationship its antithesis: the wild-type mosquito and the "open field" it inhabits. How is wildness made through an attempt to assess or fold it into the project as a fertile ground through which the drive may push? And what is the nature of the resistance it presents? In other words, what "wild" (or wilds) are these researchers making? I argue here that wild-type mosquitoes are posed as potentially resistant both to the proposed drive system, *and* as uniquely resistant to knowledge and prediction. As researchers explored the question of whether polymorphisms undermined the gene drive strategy for malaria, they generated other interesting questions in its wake: what is it possible to know about wild mosquitoes in the field? What limits exist to this knowledge practically and hypothetically? And how might it be possible to act on and in a space of uncertainty? I argue here that this double resistance, to the drive, and to knowledge or prediction, played a central role in defining "wildness" as a quality of mosquitoes within this work. I suggest that the notion of wildness which precipitated out of these practices has implications for broader conceptualizations of the dynamic relational contrast between laboratory and field in a broader atmosphere of grand promises about gene drives, genetic biocontrol, and what some practitioners call the "sculpting" of evolution (Davies and Esvelt 2018). Tracing how these two forms of resistance intertwine within the development and eventual resolution of this crisis of polymorphisms can offer a perspective on evolving notions of wildness as they take form around this biocontrol project. Explicating this work can help to lay the groundwork for a broader discussion of biocontrol, or the management of unwanted forms of life by the manipulation of relationships. In the broadest sense, biocontrol is what practitioners at the VGL think of themselves as doing; as a project on the cutting edge of new genetic methods of biocontrol, however, their work is also actively changing imaginaries of what such control means and may look like in the future. Through working to conceptualize control, I argue, researchers also must present a workable definition of its opposite. Here we see some examples of how researchers worked to describe measure or account for that which is, within this framework, out of control. I argue that a close look at these practices shows us that quite different experimental and cognitive strategies are called upon or invented to address this. It is this description of wildness as chaotic resistance I want to focus on here. From an exploration of wild resistance, we can provide a path into reflection on what notions of both "bios" and "control" are becoming in their hands.

This phrase, "in their hands", is borrowed from Natasha Myers, who uses it in her ethnography of protein crystallographers to guide readers' attention to the kinesthetic and affective work of these scientists. By directing our attention to the material performances of knowledge, Myers shows us that mechanistic or conventional accounts of molecular life do not exhaust this area of research. The gestures, stories, and bodily contortions of researchers render animate and wily proteins for which these researchers have a *feel*, as well as an education (Myers 2015). Myers' point that molecular life is much livelier, and much stranger, than textbooks suggest offers an opening here to consider how "the field" comes together in the mosquito lab both within and beyond the pages of scientific publications.

Conversations in which lab members discussed how big of a problem polymorphisms were likely to be often hinged on Drive Resistant Alleles, or DRA's. How common are they? How are they distributed within genomes, populations, continents? I'd like to consider here the imagery researchers conjured in their work. DRA's are of interest because they may prevent the modified genes from spreading. Within the language of gene drives is the image of "pushing" or "driving" something through a population. Scientific literature on gene drives also use ample language of "force" to convey this concept (Champer, Buchman, and Genetics 2016). As many social scientists, researchers and bioethicists have pointed out, social metaphors in science shape both public discourse and research agendas in conscious and unconscious ways. The prominent metaphors of "cutting" or "editing" with precision that have accompanied the recent explosion of research and popular attention around CRISPR Cas9 provide one example of the ways in which such language subtly but powerfully shapes the emergent meanings of new biotechnologies (Perrault, Halpern, and Ikemoto 2015). The language of gene drives likewise conjures an image of populations as planes or substances through which genes penetrate or "incorporate" with some amount of force. Such images conform well to existing views of population in use by population geneticists like those at the VGL, for whom populations are often visualized as topological planes of variation. While semi-bounded by reproductive isolation, populations for these scientists are fields of variation as much as they are distinct objects of research; the imagery is less that of an encircled group than a morphing plane. As opposed to more typologically minded scientific workers, for these population geneticists it is individuals and their variations which are truly real, and abstract generalizations of type which are but convenient heuristics for investigation. In the contrast proposed by Ernst Mayr in *Animal Species and Evolution* these researchers fall firmly into the "populationist" as opposed to the "typologist" camp:

> The assumptions of population thinking are diametrically opposed to those of the typologist. The populationist stresses the uniqueness of everything in the organic world. What is true for the human species, that no two individuals are alike, is equally true for all other species of animals and plants… All organisms and organic phenomena are composed of unique features and can be described collectively only in statistical terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions; only the individuals of which the populations are composed have reality. The ultimate conclusions of the population thinker and of the typologist are precisely the opposite. For the typologist, the type (eidos) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different (Mayr 1963).

It is within this image of a topological field of variation visualized as a plane that it makes sense to visualize a gene spreading, penetrating, or driving. For these populationist thinkers, this plane is moreover not composed of any uniform substance which they might call *Anopheles gambiae*. Instead, this species term is held explicitly as an abstraction. This is true for most biologists today, who conceive of populations as individuals sharing space and interbreeding, but it has a different resonance for people working with mosquitoes, for which species boundaries are especially porous and confusing. This abstraction is necessary at several levels of their work, and in the sphere of the laboratory and its cultivated laboratory mosquito strains it often holds together quite well. But when researchers turn their attention to *the field,* however, they remind themselves of the artificiality of this object. “Real” populations of *Anopheles gambiae* mosquitoes, as the experienced field entomologists on this team note, are hugely and unpredictably variable in their behavior and, significantly for Hanno's project, their genetics. This variation is what researchers mean when they talk about standing genetic variation, and *Anopheles gambiae* has a lot of it. Researchers do have access to ideal type *Anopheles gambiae* mosquitoes in the form of genetic sequencing data housed by the Center for Disease Control's Malaria Research and Reference Reagent Resource Center (MR4). But all the researchers involved reminded me that this archetypical mosquito was but one among many millions of variant mosquitoes with their slight genetic differences. Figuring out exactly how many variants are 'out there' and how different they are from the *Anopheles gambiae* of the laboratory was thus a primary goal for scientists at the Vector Genetics Lab in their duty to the "fieldwork" portion of the trial. To predict how the malaria refractory gene will "push" through a population outside of the laboratory, researchers needed a way to generate good estimates for the genetic compositions of these populations. Specifically, here, they needed to predict how many polymorphisms (which may be potential DRA's) might be present, and what their frequencies are. If there are too many, the drive's "force" may fail to "push" this trait to the whole of the population.

One might think of a drop of coffee falling on a sheet of paper. The coffee extends itself gradually along the capillaries of the paper, but should there be preexisting drops of wax on that paper its movement will be impeded. "Payload genes", or desirable genetic traits which researchers wish to drive through a population (here the malaria refractory gene) may likewise be "resisted" by the presence of a DRA which disallows the binding necessary for its propagation. One may equally say that the drive is resisted by organisms carrying these DRA's, but within the imagery of the population as topological plane it is genes in their distributions which form the substrate of this surface across which a gene drive spreads; the limits of individual bodies become less relevant to visualizing the work gene drives do on populations from this perspective. It is the distribution of genes throughout the substance or plane of a genetic population researchers work to assess and visualize. The unpredictability of an individual mosquito's genetic sequences transforms into the unpredictable genetic topography of a field.

After she freaked out, Rebecca discussed with Hanno possible approaches for investigating whether polymorphisms posed a lethal, or merely inconvenient, threat to the project. First, she and her team returned to the lab at Irvine and designed some different alleles. To test whether these alleles would drive through wild populations, Hanno designed an in vivo experiment. He planned to turn to data the VGL already had on mosquitoes, as well as to genetic data on thousands of sequenced *Anopheles* mosquitoes hosted by the AG1000 databank hosted by MalariaGen. This data was in the form of frozen bodies and charts of data showing the results of various kinds of genetic testing previously conducted on samples collected from a variety of field sites around the world (but mostly in Africa). In his own words, Hanno wanted to try to find the "most problematic" mosquitoes he could. The more DRA's, the more problematic. He then planned to rear live colonies of these problematic mosquitoes in the VGL insectary. He hypothesized that, by setting up live colonies of these highly problematic mosquitoes he could test how the drive would (or would not) "push" through a maximally "resistant" plane of variation. He could create what we may call a controlled "hyper-wild" in the laboratory by locating and amplifying this resistance. If the refractory gene successfully pushed through this hyper-wild, it seemed reasonable to assume it would successfully push through the ordinary wild as well.

Resistance could thus be made manifest *before* it was encountered in the field, and in an atmosphere more conducive to the collection of detailed data. "Wildness" identified with “problematic” and measured by looking at the numbers and frequencies of genetic sequences which would impede the spread of the drive. In his process of setting up this experiment, Hanno found a way to “capture” an emerging notion of wildness in the altered reality of a laboratory insectary by magnifying it. In identifying and counting DRA’s, resistance was posed as something that might be brought from the unpredictable “out there” of the field into the altered reality of the lab. Wildness could be purified and amplified to yield an observable hyper-wild the capacities of which could be challenged directly. Resistance could be simultaneously brought “out into the open” and enclosed within the experimental apparatus. In this setting, the “behaviors” of the drive and the DRA’s could be transcribed into abstracted data amenable to extrapolation back onto the field in future prediction efforts.

Mosquito wildness could thus (hypothetically) be invited into the space of the lab to perform its resistance under optimal conditions for observation. But Hanno is a particular and interested interrogator: it is not any “Nature” or “wildness” which is being tested here, but a very specific hypothesis about the “resistance” to a gene drive which has been glossed as “wild” and associated with an image of Nature as that which exceeds or resists the intervention or prediction of scientists in the lab. The qualities of frozen or data-fied mosquito samples which are magnified and attuned to within this experimental setting, then, yield their own unique articulation of “the field” as a force which resists. Qualities which pose a *problem* gather attention and investigation. How does the staging of this experimental reality then shape what comes to be heard here, what is rendered visible, and what is its significance when this resistance is brought “out in the open” by enclosing it in the insectary? What wildnesses make it into the lab, and what image of the vast “out there” of insect reproduction do they yield?

Hanno's experiment was designed to interrogate these wild forces by creating an experimental apparatus in which they could be both visible and active- in which they could "answer back" to the challenges he and Rebecca might pose them. This attention turned upon resistant wild-type mosquitoes positions them as highly *interesting* for the lab, and Hanno's experiment was designed to stage a setting through which whatever "wildness" they contained might shine as brightly as possible. But this wildness is not any wildness. Unlike, say, exotic pet collectors or conservation ecologists, the term for Hanno and the rest of the VGL makes no motion to harken back to some state of affairs before or outside of human involvement. *Anopheles gambiae*, as Greg pointed out, is a hard mosquito to find now in the forests its ancestors are hypothesized to have roamed (buzzed?). Everything about *Anopheles gambiae* today- from its behavior to its genetics- is dependent on human beings, on whom it preferentially feeds. Wildness is an important concept for mosquito researchers, particularly in this project. But this is no de-peopled wild; nor does it carry any of the romantic connotations it sometimes yields in other contexts. Here, wildness refers to mosquitoes which live with people in what researchers consider to be an *uncontrolled* way. To borrow Travis' term, we might consider the wildness invoked in these conversations about *Anopheles gambiae* as relating to a kind of uncontrolled cosmopolitanism in the shared lives of mosquitoes and people. There is no less, and perhaps more, intimacy between the person and the mosquito who bites them in their sleep than between the population geneticist and the swarms inhabiting a humid insectary down the hall. But the millions of interactions which take place every day between these mosquitoes and humans, and their genetic consequences for mosquitoes, are not available to the scientist of mosquito genetics. Wildness here is associated not with a separation between humans and mosquitoes, but by an inaccessibility of scientific observation and a lack of a certain form of human control. In the lab, researchers can sequence the genotypes of mosquitoes, or assay for particular genetic sequences, and thus see how a laboratory colony might hypothetically change in its genetic composition over time. Out in the field, however, it is unclear what genes are “floating around out there” [T]. Wildness is thus associated with unpredictability and with resistance. It was this latter Hanno set out to maximize in his experiment to gain some control over the former.

Wildness was here observable and measurable as resistance. The question- how different are lab colonies and field populations in terms of the performance of the drive- made these wild types interesting and focused attention on this wildness by interrogating its capacities for resistance. Bringing wild-types into a laboratory colony for this proposed experiment could, hypothetically, allow some predictions about what may happen if the gene drive-bearing mosquitoes were released into populations with highly diverse and largely uncharted genetics. But within this experiment Hanno faced a problem knitted into the core of this strategy: how does one study the unpredictable? Because many of the potential problematic alleles he was interested in were extremely low frequency, there is no guarantee that even the most “problematic” frozen samples will display resistances that may be out there in the wild. In designing this experiment with Rebecca, Hanno was hoping that resistance could be captured and maximized in the lab. As he started screening samples to find these problematic mosquitoes, however, this hope began to fade dramatically.

In a lab meeting in July, he reported to his colleagues that he had found “exceptionally high” levels of genetic variability across all the samples. Moreover, a large proportion of the variant alleles he was uncovering appeared extremely infrequently in the data. This suggested to him that there is a large number of variant alleles among mosquitoes in the world, and also that most of these variants appear in an extremely small proportion of the population. Hanno increasingly doubted that any live colony he set up, no matter how problematic, could evince the full range of resistances offered in the field. The unpredictability of resistance appeared to make any attempts to amplify it in the lab extremely limited, if not hopeless. Wildness appeared increasingly likely to exceed any efforts to interrogate it in the lab. How could someone possible enclose *so many* variants in a lab space for observation? Even if this were possible, how could he think that all of them had been captured? Extrapolating from the data he was screening (itself massive, yet a miniscule slice of actual *Anopheles* diversity in the world), it looked like "there's always going to be a resistant allele out there". Moreover, because resistance to the drive may yield some fitness advantage to a mosquito which carries it, this hypothetical inevitable resistance allele "will increase in frequency." Hanno abandoned the experimental plan. He no longer believed that all resistances could be gathered, tabulated, and amplified. He took this turn of events, however, to reformulate his question completely.

The question: which DRA's are present, and how frequent are they- could, he decided, not be answered a priori. The results of Hanno's survey efforts collapsed some hope for abstracting and predicting wild resistance directly. DRA's, he concluded, cannot be "mapped out." Surprisingly, however, neither he nor the PI Greg seemed disheartened by this news. That flurry of papers in 2017 which seemed to cast doubt on the viability of gene drive strategies for mosquitoes had posed the question: are there any alleles which can serve as target sites for the drive system which *do not have* variants among some mosquitoes? The preliminary results of Hanno's investigation suggested that the answer is no. There are no potential target sites which are conserved among all mosquitoes everywhere in the world.

This phrasing, everywhere in the world, is significant. When he was deciding how to arrange his data for the publication he prepared on this research (Schmidt et al. 2020), Hanno faced a question: should I group my data by geographic region, or include all the DRA's together, regardless of where the mosquito it came from was collected? The first option would yield some statements about wild resistance in general. The latter would yield contributions towards descriptions of more particular resistances, or more plural "wilds." Because DRA's and their frequency both vary across geographic locales, one might end up with geographic places populated by planes of variation which have more resistance, or resistances more problematic for this gene drive. An option for bifurcation emerged around which the field needed to be articulated. Is "the field" the whole world outside the lab? Or is it a geographical region, like East Africa, or is it a particular country, or a province or a village? What do other researchers in the field of mosquito genetics interested in the viability of gene drives need or want to know? And which kind of statement- about wild or wilds- would serve the UCIMI project best in promoting their work?

This question mattered for the framing of polymorphisms. There were indications that DRA's are structured by population- their frequency and distribution are not the same across different locations. As Greg pointed out clarifying the issue: "what a good target even *means* depends on population." That is, it was unclear whether there could or should be one "target product profile" or genetically modified mosquito best able to push the gene drive through a population. Is this a project about one solution, or many? This lay in tension with the project's mission, which was to develop and test in an isolated island setting a mosquito capable of "working" (that is, spreading the trait of refractoriness) to all *Anopheles gambiae* populations on the continent of Africa and, by extension, the world. This vision requires a scalar logic: to test the mosquito on an African island is to model how it would behave in a larger continental setting. The mosquito, and its dynamics with natural populations, must remain hypothetically stable across changes in scale. However, indications of population structured DRA's suggested that these dynamics are unique to local sites with their particular mosquito populations and the particular frequencies and distributions of alleles therein. Can one speak of wild populations in generalizable terms, or only of particular wilds with their own signature patterns of resistance? The answer to this would bear on whether researchers in this nascent field considered it feasible to imagine one mosquito adequately designed to their task, or many. As an onlooker, I was also interested in other downstream effects of this choice. How might a decision to open a conversation on either *the* field or *multiple* fields influence how wildness comes to be framed in broader conversations about biotechnology and biocontrol? Would researchers, and perhaps by extension the public discussing this topic, come to speak of wildness and the field it inhabits in the singular or the plural? What might that do to how Nature (or Natures) is/are invoked as a contrast, a substrate, or an opponent to such control?

Hanno made the decision to present his data *without* grouping it by geographical source location. This decision was motivated, he said, by a desire to "be conservative and present worst-case scenarios here." Presenting his survey *not* structured by population provided his readers with a worst-case scenario because all DRA's detected in any of the samples used would appear in his generalizable statement about the frequency of DRA's in *Anopheles gambiae as a species*. It also generated a "wild" which is not locally specific. This allowed Hanno's paper to provide a comprehensive answer to the fear that was implicit in those early meetings: do polymorphisms and particularly the drive resistant alleles among them imperil the viability of the concept of gene drive as a potentially globally scalable concept? Un-grouped data presented a worst-case scenario because it provides an interested reader with *all* the potential DRA's captured by collection and sequencing, wherever those collections occurred. Together, they make a larger list of "problems" than would be encountered in any one region. This conservatism was framed as good science insofar as it maximized the problematic in question, a move similar in nature to that pursued by Hanno's plan of the laboratory enclosed "hyper-wild." As Vincianne Despret and Isabelle Stengers write, it "maximiz[ed] the recalcitrance of those [Hanno] wanted to interrogate" (Stengers 2010). Maximizing the recalcitrance of the DRA's was posed as an honest approach, not trying to minimize the threat pointed out by the previous flurry of papers. This presentation of the data maximized resistance and, as something of a side consequence, offered the field as a global reference.

As I soon learned, however, maximizing this resistance in the data had another effect which was very important to the lab. Researchers increasingly stated as fact "there is always going to be a resistant allele out there that'll rise in frequency." The presence of these alleles turned from a crisis to an assumption. Following the statement above, another researcher discussing Hanno's work continued, "you will not find a target site that doesn't have a variant somewhere in the world." This researcher, Travis, was relaying what had by then come to be taken for granted about "natural populations": variants are *always out there*. As sample sizes increase, so do rare and potentially resistant alleles. Thus, nature as it came to be viewed from the lab through this prism of the wild types implies a type of heterogeneity which is a priori unpredictable. In this context, that very unpredictability came to be the distinguishing characteristic of the wild. Because many variants are very rare, and thus unlikely to be represented in any conceivable sampling efforts, they become both centrally important *and* stubbornly resistant to exact description. Rather, they must be worked around in the absence of concrete identification; it is this mode of engagement through which researchers articulated what is "natural" or "wild" about populations in the field to themselves and each other.

In his research, Hanno shifted the question from: are there too many polymorphisms at target sites for a gene to effectively push through a wild population to: how many genes have a good target somewhere? Most, he discovered. Around 90% of protein coding genes had a good target site conserved somewhere. However, this number drops precipitously if genes with very low frequency DRA's are excluded. Essentially: variations are many, unpredictable, and rare. Any potential site has a target somewhere. If it isn't in the data, it's probably out there in the field. No one can collect enough mosquitoes to record all DRA's in all mosquitoes everywhere. It is this rarity that formed the crux of his pivot towards the advantages of the Irvine project. This project is based around population modification: you don't get rid of the mosquitoes, but you change them so that they don't transmit malaria. There is another major group pursuing gene drive applications for mosquito borne disease control. Target Malaria, their much better funded "nemesis" is following the other genetically modified mosquito (GEM) strategy: population suppression. In other words, try to have less mosquitoes. Traditionally, people have pursued this goal by killing mosquitoes; Target Malaria's gene drive strategy, however, aims to prevent them being born.

If drive resistant alleles are always out there, always unpredictable, and occur in very low frequencies in nearly all protein coding genes in the *Anopheles gambiae genome*, then one cannot rely upon a strategy which requires *no* DRA's, or under one percent frequency. Hanno was able to describe a global field containing very many low frequency DRA's by grouping his data without reference to collection location. He knew, in assembling his data this way, that his own laboratory had access to more data (more sequenced mosquitoes) from fewer sites than Target Malaria, whose datasets were broader but shallower. Pooling all mosquitoes into one global field using this narrow but deep set, then, yielded a more "problematic" image of the global wild than a wider ranging but less intensive survey because of the structured nature of these polymorphisms (because some places have lots, and some few). Providing good evidence to describe wildness in this way highlighted a contrast between the UCIMI and Target Malaria relating to the question of fitness costs. The inevitable polymorphisms, should they confer a fitness advantage on mosquitoes which resist the gene drive, will quickly be selected for an increase in frequency. This, I was very patiently told, is an inevitable fact about evolution. This inevitability led very obviously for these researchers to the question: how much of a fitness cost does the gene drive extract? They also sometimes phrased it in the converse by asking: how much of a fitness advantage does a drive resistant allele offer?

Researchers also explained to me that it is as yet impossible to genetically engineer a mosquito which is not at a slight disadvantage vis a vis its wild-born counterparts. They are (to different extents) weaker and slightly worse at courtship, copulation and reproduction. Thus, just having a genetic modification is of evolutionary disadvantage to some degree. To bear a gene drive which causes you to die is pretty much the highest fitness cost one can have, however, and the gap between the fitness disadvantage of mosquitoes designed to survive, and those designed to die, thus became the central point of Hanno's paper. DRA's, he concluded

> "within the standing variation that exists in natural populations of the mosquito species studied will not pose a problem to the successful deployment of CRISPR Cas9 based gene drive for population modification strategies. Gene drive used as part of population suppression strategies are more likely be unsustainable because of the low frequency DRAs and the fact that they impose much stronger selection favoring them" (Schmidt et al. 2020).

Hanno assembled his findings into a logical chain with a clear narrative: if DRA's exist for every potential target site, are very rare, and cannot be fully predicted or accounted for, any potential GEM project, whether modification or suppression, will encounter them. From this, he led his readers to the question: what happens when a GEM encounters them? And he proposed that the answers look very different for population modification and population suppression strategies. For population modification, if the drive fails to spread to some very small number of individuals this will slow the spread of a modified through the population but will not stop it. The rarity of most DRA's means that these impediments will not block the drive from incorporating throughout a population. To return to the image of coffee spilled on paper, the drops of wax are very small and very few, and the paper will eventually still saturate. His story looks quite different for population suppression. If the modification carried on the drive leads to the death of the offspring of a carrier- wild type mating, then having resistance is very highly favorable to the mosquito. This means it will be "strongly selected for". Genes that are selected for will increase in frequency. To return to the language of "selfish" genetic elements, the selfishness of the gene drive will not be sufficient to overcome the selfishness of genes which allow mosquitoes to survive which would otherwise not because of the population suppression GEM. Within this narrative, it is the strength of selection against the effector gene which is of primary importance. For population modification strategies, that strength is quite weak. For population suppression strategies, Hanno and especially the PI Greg wanted to emphasize that it is very high. This means that "GEMs which create a large fitness cost, such as in population suppression strategies, will face significant challenges finding suitable target sites" (Lanzaro 2020).

As Greg concluded in a Monday lab meeting over a year after the initial worrying results, "this paper isn't a tool. It's a statement about whether standing genetic variation is going to be a problem for gene drive. The answer is that it is for suppression, but not for modification. This is good for us." It is important for the UCIMI to distinguish itself from Target Malaria because both are very often chasing the same sources of money. As Greg continued: "it's not that we're especially interested in seeing Target Malaria have problems, but we are in the process of negotiating additional funds from granting agencies because we are way short of the money to do what we need to do. Invariably what they ask is how is this different and better than what Target Malaria is doing so it's nice to have some answers. We're clearly different and in and of itself that's worth the price of admission because there needs to be more than one group with one plan. And it's not that we fight with them our relationship with them is pretty amicable, I think. The reason I'm asking these questions is so we can communicate how we're different and why in some ways this might be a better approach. Or at least an approach that's different."

The research described here into the question of standing genetic variation and the viability of gene drive strategies for mosquitoes extended over nearly three years. During this time, a potential crisis was transformed into an asset. Potential DRA polymorphisms were re-framed from a destabilizing possibility to an assumed inevitability. Along the way, researchers also mobilized this data to make a case for the population modification strategy of the research consortium of which they were a part over and against the population suppression strategy of a competing organization. This outcome, a research paper which presented what its authors saw as strong evidence in favor of population modification over suppression, also entailed a reformulated orientation towards "the field" and the wild type mosquitoes which inhabit it. The field came to refer within this research to *all spaces* outside of the laboratory, and its significant divergence from the laboratory was over the wildness of the *Anopheles* mosquitoes which inhabit it. I showed here how wildness was associated with resistance both to gene drives and to predictive description. The main researcher on this project, Hanno Schmidt, originally thought to maximize wildness for research purposes for study in the laboratory by identifying the most "problematic" or DRA rich samples he could and rearing them in the lab. The idea at this stage was to create an enclose what I termed a "hyper wild" which could magnify the qualities with which researchers were concerned. I showed how this proposal was replaced by a growing agreement that it was impossible to represent, let alone magnify, all relevant polymorphisms in the laboratory after a review of the data showed high numbers of them at very low frequencies. With this came a fact that the wildness of the field could not be mapped out a priori but will always remain, to some extent, unpredictably resistant. This shifted researchers away from questions about whether there exist DRA's at potential target sites towards discussions about negotiations and outcomes with wildness fixed at a global level and referring to *Anopheles gambiae* as a species. This preserved the scalar model of the island test experiment: one field, one wildness, one GEM mosquito which can be applicable anywhere.

Here I have turned our attention to the half of this mosquito-field dyad which is less frequently foregrounded: not the creation of a mosquito which might work, but the building of a field in which its capacities might potentiate. This labor, of assessing "real world" spaces outside of the lab and the genetics of mosquitoes that live there, is often discussed by practitioners in terms of the production of adequate and useful descriptions. Field sites are assumed to pre-exist the intervention: they are what is "already there", the baseline data through which change can be registered. As I hope to show here, however, the choices researchers make in defining their field and its wildness generate particular and significant accounts of natural environments. It is not only the new mosquito but also the “preexisting’ field which have to come into articulation in compatible ways. As a complement to attention on this new mosquito, I proposed a shift in view which draws its (literal) background into focus. Research on this topic of polymorphisms was part of a broader effort to assemble descriptions of real-world places (fields or the field) and their mosquitoes. Researchers do not speak of such descriptive work as an act of creating. But, as many anthropologists of novel scientific capacities have demonstrated, ideas of the new are made visible in relation with that which is posed as preexisting, a relationship which often entails a reformulation of what, exactly, was there in the first place. Here we see researchers working to articulate to themselves and their colleagues the qualities which make mosquitoes wild. In the process, emerging descriptions of “the field” and the wildnesses it contains were oriented around the concerns of this gene drive project. Nature was explored through, and sometimes as, resistance both to genetic engineering and to prediction. This account of wildness is particular: unlike other invocations of Nature or The Wild it makes no reference to a world apart from or free of humans and their built environments. The wildnesses of mosquitoes in the field, rather, is a quality of adaptiveness, unpredictability, and resilience. *Anopheles gambiae* mosquitoes do not live without from human beings. Their love of us, their anthropophilia, is fundamental to their species being. Developing descriptions of their genetics and behavior outside the lab, then, involves discussions about how to negotiate and abstract *uncontrolled* mosquito-human relations and their consequences. To return to the term proposed by a VGL lab member, we might say that wildness in this setting refers broadly to a kind of *uncontrolled cosmopolitanism*. The nature, distribution and consequences of resistances, here posed as distinctive of wild mosquitoes and their fields, was, by the end of this work, posed as not only unknown but ultimately unknowable. This unknowable, unpredictable resistant nature was mobilized as an asset within the scientific and institutional ecology of mosquito gene drive research. It also reinforced certain aspects of what researchers see as common notions of wildness or nature while focusing less on others.

Because of their epidemiological significance, *Anopheles gambiae* mosquitoes are among the most highly studied insects in the world. Even so, scientists who work with these mosquitoes are constantly encountering surprising and often baffling phenomena among them. This does, somewhat paradoxically, not surprise them. In the field, one expects to be surprised. *Anopheles gambiae* usually live about a week or two, and females produce hundreds of eggs in each oviposition. The sheer number of *Anopheles gambiae* in the world is astronomical and incalculable. Estimates are much more obscure for nearly every other species of mosquitoes, and unknown for the vast majority of species of insects. From the perspective of a scientists interested in bugs, then, one might easily feel one's research as quite a small point in this vast expanse of billions of brief tiny lives and their untold multitudinous variations. The story of the VGL and their investigation of standing genetic variation among *Anopheles gambiae* mosquitoes in the wild gives us one image of researchers articulating to themselves what they see as the cryptic immensity of insect variation. We can also see an emergent notion of the wild as that which necessarily exceeds human capacities for surveillance. This is a distinctly insectile wild, never out of earshot of the chaos of the swarm.

Of course, to conclude that something is unknowable, unreachable or senseless is itself a sort of knowing of it. The epigraph to this chapter, taken from Foucault's *History of Madness*, evoked for me an image of contrast between this expansive uncertainty I saw researchers describing and their own highly elaborate and precise methods for registering, measuring, testing and combatting this uncertainty. To move up just a few more sentences, however, this passage on the very different topic of madness makes a point that could well serve as a reminder to me as an observer of their work. Foucault writes:

> To write the history of madness will therefore mean making a structural study of the historical ensemble – notions, institutions, judicial and police measures, scientific concepts – which hold captive a madness whose wild state can never be reconstituted; but in the absence of that inaccessible primitive purity, the structural study must go back to that decision that both bound and separated reason and madness; it must tend to discover the perpetual exchange, the obscure common root, the originary confrontation that gives meaning to the unity and the opposition of sense and senselessness. That will allow that lightning flash decision to appear once more, heterogeneous with the time of history, but ungraspable outside it, which separates the murmur of dark insects from the language of reason and the promises of time.

Foucault speaks of a madness which is "held captive", but whose "wild state can never be reconstituted". Madness itself, in this passage, cannot be fully registered or described in its "inaccessible primitive purity". Rather, it emerges in contradistinctive relation with that which both studies and opposes it. Foucault's location of an event, even if heterogeneous to the time of history, in which the separation of the murmur of dark insects and the language of reason gave life to both serves as a helpful reminder in this writing as well that the incalculable immensity of mosquito variation itself does not, in some pure sense, predate its position as a problem for mosquito researchers. Their descriptions contain this notion which they constitute as inaccessible, but the very excessiveness of a wildness that reaches beyond such efforts becomes sensible (as insensible) within this dyadic relation with researchers' own practice. We might take Foucault's point that there is no primitive purity of madness before the study of it to reconsider the assembly and analysis of baseline data as doing something quite other than “merely” describing a preexisting state of affairs. Genetic biocontrol efforts like this one increasingly offer us images of a new and different kind of environment, one precisely “sculpted” to answer human needs and problems. But, to draw again on the image of the lightning flash, this promise of time also conjures beside it that which it both studies and opposes. Here I have drawn attention to the labor which is involved in constituting that which is taken to be “outside” in some way the realm and possibilities of knowledge. In showing how researchers work to register, measure and describe that which they say exceeds such efforts, I hope to gesture towards some of the ways in which natural environments are posed as materials amenable (or not) to such sculpting.

Chapter Three: How do you Cage the Sunset

Introducing the Cage

This is a drawing of a sunset. Or, I might say: this is a drawing of a sunset-generating apparatus for mosquitoes. It is the first sketch of the design for a cage designed by an insectary manager who wants to stimulate swarming, and thus copulation, in *Anopheles gambiae* mosquitoes. The sketch shows us a rectangular cage, bisected into two unequal parts and deconstructed into two dimensions. Above the upper limits of the rectangle, a simple outline of a lightbulb is labeled "daylight." The outline of the rectangle tells us it is the outer limits of a "bug dorm cage." Along either side of the upper, larger, portion of the rectangle long and narrow rectangles give off short pencil strokes indicating light directed toward the cage: these are dimmable lights to mimic the mosquitoes' crepuscular period. A cutaway shows us that the front of this cage is to be a transparent screen through which an observer can watch the cage's inhabitants. Towards the bottom of the rectangle, a thick dark line cuts off the upper, lighter, portion from a bottom quarter indicated as dark by cross-hatched lines. This is the dark resting area. From the bottom, rising up to just below the meeting point between dark and light, a stick with a circle on its end represents the swarm marker. Floating hazily above it is a charcoal smudge which indicates hopefully the *Anopheles gambiae* swarming around it. This is a design for an object which can *do* a sunset for mosquitoes; but what does that mean?

Communicating to Others

I felt that if I could find the right words to describe these ever-changing phenomena, if I could communicate to others the character of an event which was never twice the same, then I should have penetrated- or so I felt- to the innermost secrets of my profession: bizarre and peculiar as might be the experiences to which I should be subject in my career as an anthropologist, I could be sure of putting them, and their implications, at the disposal of the common reader.

So begins Claude Levi-Strauss his journey within a journey, describing the sunset viewed from the prow of a ship departing Marseilles just after six in the evening sometime in 1934 with all his dizzying vigor, intense specificity and timelessness, imaginative leaps and clear eyes of observation. Levi-Strauss devotes nearly ten pages at the opening of *Tristes Tropiques* to this minute description, an artful testament to his virtuosity in the face of what he poses here as the central task of the anthropologist: *communicating to others the character of an event*.

We readers are swept through the "ensemble of procedures" by which night staged its "insurrection" over the ocean that evening, waltzed through this overture with "a beginning, development, and an end". Each color is painstakingly precise- Alpine yellow peeks over shrimp-pink and salmon-pink and flax-yellow and straw-yellow, each cloud in its evolving formation recounted in the grand march of an epic. His specificity is so intense it is almost painful- each wispy filament is recorded in its act of disappearing. And yet the sun, in addition to being itself, is also an architect, a painter, a "metal sheet at once hard and lacey", a "coin thrown down by a miser in the heavens", the "yolk of an egg". The clouds are not only clouds but “sculpted imitations of clouds” with “the polished and rounded look of wood that has been carved and gilded.” They are “vapors arrested in the act of boiling.” They are “ramparts: battlements heavily breasted and yet ethereal”; a “complicated edifice” involving crystal, “flimsy scaffoldings”, “bloated pyramids.” They form a vast unreachable summit. At the same time, they are as scrutinized as through a microscope; they are “like a skeleton”, but “plump”. The light gathers like a “clenched fist”, from the sleeve of which it becomes “a few stiff and glittering fingers”, or it comes forward momentarily like an “incandescent octopus” from “vaporous grottoes.” It is the “sudden blaze of footlights in the theater”, the bow of a violin, spun glass, “liquids of different colored densities poured into a glass”, a “tracery of a stick of charcoal on granulated paper”, “wiped away firmly and unhurriedly with a piece of cloth”, doomed to collapse at its end like “some marvelously colored Japanese toy.”

Levi-Strauss does not think for a moment that an adequate description of this event could remain in the language of gaseous formations, or light rays and their diffusions, alone. That would be as wrong-headed as scholars, who claim that “dawn and twilight are the same phenomenon” or the unfortunate Greeks who failed to recognize in the resources of their language the profound differences between the two: “an excellent illustration of our tendency to put theory first and take no account of the practical aspects of the matter.” No. To communicate the character of the sunset is not to explain the physics of the earth’s rotations. It isn’t even to minutely describe everything he sees. He rather communicated to his readers a *feel* for that event, to evoke something of its character for them. In so doing, the sunset explodes into a panoply of other images, objects, and sensations.

This practice of honing his descriptive skills was undertaken in preparation for his ethnographic voyages, and for the writing of ethnography that would follow them. Levi Strauss believed that he could communicate an experience of the sunset that would be recognizable to his readers if he could conjure a certain kind of feeling experienced by all people at this time of day. "The fisherman", "the peasant", and "the savage" alike, he tells us, all stop what they are doing "when the sky is first lit up by the setting sun", struck by "a hallucination, one might say- of the indecipherable forces, the vapours, the fulgurations whose obscure conflicts he has glimpsed vaguely, within the depths of himself, from time to time during the day." It is a common humanity which holds together these various experiences with the physics of the sun, and a common humanity which underwrites the possibility of ethnographic communication. His writing may not function as a representation of the sunset, but it evokes something common to us all, and it is this commonality, more than the specific sunset of that evening outside of Marseilles, which he pursues in this exercise (Levi-Strauss 1955).

In this chapter, I ask what happens to such a practice when this guarantor of human experience is excised. What is it, then, to "communicate to others the character of an event"? I offer here the story of mosquito researchers attempting a practice which, on one level, is not dissimilar to Levi Strauss': they want to evoke the sunset. They do so, like the anthropologist, by exploding it into various other objects aimed at evocation and response. Like Levi Strauss, this work is precise and virtuosic. But mosquitoes are not like "the fisherman" "the peasant" "the savage" or us. As we will learn, they are "the closest thing we can get to an alien." "Communication", for example, no longer feels adequate to name the relations drawn by and through the varying material configurations which they named as sunset. Instead, we witness an artful dance, in which both mosquitoes and persons are stimulated to act by each other and by shifting assemblages of light and shadow, heat, scent, atmosphere and mood. When the cage functions, we will see, both are held by a mutual captivation. This paper comes to terms with the particular form of artfulness this dance takes, attending to this installation's status as an abstract, rather than representational, piece.

The Death of a Colony

Abram Estrada had only very recently been hired at his first job out of college when all the mosquitoes died. He had interned at the Vector Genetics Laboratory at UC Davis during his undergraduate degree in entomology and joined the team full time after graduation to manage the insectary when Alli, who had previously cared for the rotating colonies of mosquitoes housed in the small insectary on the fourth floor of the Veterinary Medicine building, left to take a new job. Abram was, and remains, fascinated by bugs. During the five-week field station program he did one summer in college he collected more insects than anybody else, more insects than anybody could remember anybody else collecting. He is creative, precise, and untiring. He comes in on the weekends, he puzzles over mosquitoes at night. And so, eager as he was to do well at this job, he was crestfallen when a new colony of mosquitoes transported at staggering expense from two islands off the coast of West Africa died on him en masse.

These mosquitoes' parents had been born on the islands of Sao Tome and Principe, where the VGL was in ongoing negotiations with the national government to establish a unique and unprecedented experiment. The lab, as part of a consortium led by the UC Irvine Malaria Initiative, wanted to transform these two islands into experimental models. Researchers in the UC system had developed versions of the *Anopheles gambiae* mosquito (the primary vector of malaria in Africa) which cannot pass the parasite on to human hosts, and they had equipped these mosquitoes with what is called a gene drive- a genetic element which causes this trait to be inherited by not half (as would be expected in normal Mendelian inheritance) but all of a mosquito's offspring. This "super Mendelian inheritance", it was hoped, could be the key to eliminating the disease completely from the continent of Africa and, hypothetically, the world.

But before that can be possible, the group needs to prove that these genetic modifications *work*. They need to demonstrate that the gene "drives" through mosquito populations in the real world, and not only in the lab. And to demonstrate this they need not only some small chunk of the real world (the specificities of which are discussed in chapter X), but some real-world mosquitoes to test it on. Eventually, researchers hope to address both at once by releasing the new GEM's (genetically modified mosquitoes) onto these islands and into the mosquito populations that inhabit them, but a crucial intermediary step involves bringing these real-world mosquitoes into the lab.

This is important because mosquitoes of the laboratory are quite a different beast from mosquitoes of the rest of the world, referred to by the researchers as "wild-types." It takes about seven or eight generations in a cage, another insectary manger had told me a year or two previously, for mosquitoes brought from the "wild" to become laboratory mosquitoes. They learn new habits. Their genetic profiles change. They adapt to the strange world of the insectary. It was these laboratory mosquitoes upon which genetic modifications had been made, and through which the drive had been shown to "push" in insectaries in Irvine. Researchers were confident that their plan would translate from insectary to field, but they also know that wild types are different in significant ways from laboratory mosquitoes. Accounting for these differences, then, was essential to predicting how the gene drive would "behave" in the world outside the laboratory. To describe these discrepancies between laboratory and wild-type mosquitoes, to account for potential differences in the behavior of the genetic modifications, and to conduct some “dopey dumb stuff for regulators” about safety, they needed to bring the wild mosquitoes into the lab.

One of the most significant differences between laboratory and wild type mosquitoes, and the one which emerged as a central concern in the work described here, is copulation. In the wild, *Anopheles gambiae* copulation is a fascinatingly mysterious process. They mate only at sunset. The males form a swarm over some kind of a landmark (a fork in the road, a turnstile, or some other object- scientists still do not know how they choose or interpret these landmarks), and they begin to “dance.” Females hear this dancing swarm and enter it singly. They mate in flight, potentially harmonizing their wing beats to do so, although this remains a topic of debate ((Borrell 2009; Gibson, Warren, and Russell 2010)). The same insectary manager who told me about the evolution of wild-type to laboratory mosquitoes told me that her laboratory mosquitoes just “sort of” swarm, especially if you “thump on the cage a little bit.” But mostly “they just kind of wander around.” They don’t need the large dense swarms enacted by mosquitoes in the wild in order to copulate and reproduce because they have adapted to the milieu of the lab (and its cages). They do a different kind of dance, which does not require the sunset.

For Abram, facing the catastrophic mass death of his first very important colony, this was the difference that made all the difference. In early January 2020, at the usual Monday lab meeting, he reported the beginning to the die-off. These mosquitoes had been hatched from eggs brought back by VGL researchers from Sao Tome and Principe a month earlier, but they were expiring at a rapid clip. The problem was that they would not swarm. If they do not swarm, they do not copulate, and they will not blood feed if they do not copulate, and if they do not blood feed they start to die. In a last-ditch effort, Abram tried a notoriously tricky and labor-intensive technique to save the colony: forced copulation. First described in the literature in 1957 (Mcdaniel and Horsfall 1957), it involves anesthetizing females with ether. Abram would then aspirate a male, using his mouth and breath to suck up a single individual which he then stabbed through the abdomen with a very fine pin. “And then”, Abram explained to me later, “you cut off his back legs. Mosquitoes have this thing where they can kind of push themselves away if they don’t want to have sex, so this prevents that. And then you cut off his head. They perform better at copulation if you cut their heads off. It’s like they’re little machines, just made to do this in life so you just kind of remove anything else that gets in the way of that, any reservations or whatever. Then you move him down to the female and the tricky thing is getting them to attach; he needs to clasp her with his pincers for it to work. It’s pretty tricky.” For readers at home, I cannot stress enough how much smaller *Anopheles gambiae* is than the mosquitoes most of us have in our heads when we think of that word. Their average wingspan is only around 3-4 millimeters; the work is incredibly precise and notoriously difficult. Abram practiced this technique for weeks, trying to salvage what he could of the actively dying mosquitoes. It did not work. Abram was disappointed, but his older and more experienced colleagues reassured him that this is a very fine and difficult process which takes years of practice. Even if it had worked, forced copulation is no reasonable way to maintain a colony. Eventually, all the mosquitoes were dead, and Abram turned his attention to the future: someday soon (he thought, as Wuhan entered lockdown) there would be new mosquitoes making the same journey from Sao Tome and Principe to Davis, and it was crucially important that he keep them alive and breeding to provide for his colleagues’ experiments. How would he convince them to copulate, and therefore live? Forcing them on each other was no durable solution to this problem. Nor was it possible to use mosquitoes acclimated to laboratory copulation. To preserve this wildness, and to study these mosquitoes without making them into laboratory mosquitoes, Abram could not *impose* conditions upon them. He turned his attention to a more imaginative creation.

In a laboratory meeting around this time, he presented the problem to his colleagues: they mosquitoes would not mate, and he could not force them to. Shaghayegh, a postdoc migrating from working with beetles asked, quite reasonably: “is it because they are from the wild?” “I wish I could say, honestly” responded Greg, the PI of the laboratory who has over thirty years of experience in working with mosquitoes. “I really don’t know. I’ve brought back these species before and reared them just fine, but sometimes it just doesn’t work and…ugh.” The startling pace of the colony’s death was leading him to doubt claims made by the local mosquito control workers in Sao Tome and Principe, who said that they had successfully colonized these mosquitoes in labs of their own on Sao Tome. Perhaps, he suspected, they were “just adding from ongoing collections, not rearing a colony. But we did get them to feed there. They reared there. I do know they were feeding on guinea pig blood.” Shaghayegh, thinking about the beetles of her PhD, asked: “were they the exact same conditions?” Anton, another highly experienced entomologist who had been on this recent trip to the islands, chimed in: “it was just in a room. It’s warmer there.” Greg looked dubious: “I don’t know if we could replicate it.” Hans, who manages the logistics of most experiments in the lab, countered: “But we have temperature and humidity controls here. Can you record the temperature and humidity counts in that room?” Shagheyegh added that that is what she did with her beetles, replicating the conditions exactly. Not without pride she added, “it was a great success.”

In this initial conversation we can see a hypothesis that would be elaborated upon extensively in the following months: the deaths of these mosquitoes are an issue of *place*. Even though these individuals were hatched in the California lab, they were not mosquitoes *of* that habitat. They were still wild types, demanding at the cost of their own survival the specific sensorium of their ancestral home. Something in the laboratory- here we see researchers hypothesizing about temperature and humidity- was not right for them. This was not an issue that could be solved using laboratory mosquitoes, or even by making these mosquitoes into laboratory mosquitoes: these samples were important precisely *because* they were not laboratory mosquitoes. Researchers wanted to be able to research wild types in the environment of the lab, but they wanted them to stay, as much as possible, "the same" as mosquitoes of this specific place. Otherwise, the experiments they planned would not act, as they needed, as a sort of bridge between the laboratory and the wild. Emerging in this first conversation was the idea that doing so was going to require the establishment of a milieu within a milieu- the building of Sao Tome and Principe, in some way and to some extent, in Davis California. This problem of rearing wild-types in the lab is not new. Underrepresented in the grand narratives around gene drives and their potential, it is in fact a central crux of making possible this dream, which rests on the assumption that the wild and the lab can be knitted together by reproduction, eventually re-creating global populations in its image by "designing evolution." The ability to keep mosquitoes out-of-place is often assumed, but it emerges as a frustrating and potentially unsolvable problem in this and other GEM experiments. As Greg lamented with frustrated exasperation at another laboratory meeting, "If we could recreate the conditions of the lab all over Africa there would be no malaria!" Short of remaking all of Africa in the image of a California lab, researchers need to cope with the specificities of places and the kinds of habits, genetics, and sex that they produce. The story of this box, then, is also a story about researchers trying to think place through this box. What is important about a specific place to a mosquito? How might they abstract those significant elements and re-create them in the laboratory?

Caging the Sunset

A couple of weeks later, Abram brought a sketch with him to the annual UCIMI conference at UC Irvine. This sketch shows the design for an object which Abram intended to build in the lab. It is a modified cage, outfitted with materials like lights, a swarming point, and a make-believe "horizon" which Abram hoped would generate the elements perceptible and important to mosquitoes under the Sao Tome sunset. The cage does not represent the sunset for either mosquitoes or people, but for the former he intended to evoke it. Having presented his initial idea of creating a cage which generates not the sunset, but the perceptual stimuli sunsets afford which matter to mosquitoes, Abram saw his task as testing and modifying this prototype. Were the elements he had selected (light, barometric pressure, humidity, et cetera) to distill from his understanding of the sensoria of the sunset the same ones that did things to mosquitoes? Which parts of the scene are vital, and which expendable? Are there other sensorial qualities that mosquitoes attend to which he hadn't noticed?

That evening, after he presented the design of this cage, over wine and cheese in the same spacious conference room, Abram told me "This is what's so cool about mosquitoes. They're the closest thing you can get to an alien." If Levi Strauss drew upon ramparts, battlements, architects and painters, misers, egg yolks, polished wood, crystals, shirt sleeves, pyramids, scaffolding, clenched fists, octopi, violins, theater lights, glass, microscopes, charcoal sticks, Japanese toys, and plump skeletons to evoke for a human reader the feeling of a sunset, what could Abram draw upon to evoke for *Anopheles gambiae* the character of this event?

Mosquitoes are aliens; we live shared sunsets, but theirs are not ours. Even the most experienced entomologists do not know what the sunset is for a mosquito, what aspects of it they smell, hear, feel, or see. Nor can the general facts of the shifting positions of specific points on the earth in "an indivisible movement between the zone of incidence of the sun's rays and the zone in which the light vanishes" as Levi Strauss phrases it tell us what the sunset *is* for the mosquito, any more than they capture the experiences of people. It is inviable, for Abram, to take no account of the practical aspects of the matter. He did not know what makes up the island sunset for the mosquito, what elements matter or how they matter for the emergence of the swarm and the act of copulation. But, if he were going to succeed in averting a second disastrous colony failure in the future, he would need to find out. This, he figured, did not entail an exhaustive inventory of the mosquito's perceptual capacities (an extensive field of study addressed much more directly by other laboratories for years without reaching the ends of what a mosquito is capable of), but it did mean the building of something which *worked*.

Abram used the word "simulate" or "replicate" to describe the relationship he wished to achieve between the sunset of Sao Tome and the sunset cage (and its surrounding room) he wished to create. Expanding on this, he told his colleagues that his goal is to have the cage "operating as nature." Between the plywood, Lowes office lighting, duct tape and fabric of the cage in the University infrastructure and sun setting over the pigs, dogs, and stilted wooden houses of Sao Tome, Abram wanted to establish some workable linkages of similarity. He wanted his design to *operate as* what he saw as the natural space of Sao Tome. As we have seen, this operating as does not entail a relationship of resemblance. The strip lighting looks nothing like the filtered lights of dusk through trees; the office ceiling light nothing like the sun. And yet, no less precision was required of this relationship for this lack of resemblance. The difficulty and the art of the task ahead of him was laid out by the massive colony death he experienced initially: these wild-type mosquitoes are not tolerant of sloppy reproductions. Though what he makes may look nothing like that which it simulates, Abram's creation must simulate it very precisely.

Abram has a number of tools at his disposal in pursuit of this precision. Measurement devices were foremost among them. For example, he used Lux meters to measure the light in the rice paddies from which he collected the *Anopheles freeborni*, and then to replicate those light levels in his cage. Measurements of humidity, barometric pressure, and time (zone) of Sao Tome were located using the internet and likewise replicated in his setup. As we will see in a following section, even odor was subjected to this precise work of re-creation. These measurement devices allowed Abram to pursue a project of creating a milieu which operated as another without resembling it at all.

The test of success would be the behavior of swarming and proof (through dissection or living offspring) of insemination. Thus "sunset" in the terms of his practice became coextensive with "swarm" in terms of his final goal. He would know he had constructed a sunset which "functions" not when he recognized it, but when he observed his mosquitoes swarm. "Sunset" thus bifurcated: the idea of the setting sun in Sao Tome remained relevant, and Abram would return imaginatively to this sensorium to make guesses about which elements of its mosquitoes may find significant. But the version of "sunset" he set out to create in the laboratory was to be an abstraction of this sensorium, distilling only its mosquito-significant elements. We might distinguish the imaginatively invoked sunset of Sao Tome as $sunset_{1,}$ and the abstract simulation he aimed for in the laboratory as $sunset_{2.}$ There is a relation between $sunset_1$ and $sunset_{2,}$; the latter achieves what Abram guesses to be the significant qualities of the former. Thus, like an abstract painting, Abram's $sunset_2$ evokes something of the impression of $sunset_1$ while dispensing with a thorough accounting of it.

But what aspects of $sunset_1$ are significant to a mosquito? How is Abram to abstract when he does not believe himself privy to the operations by which the world becomes sensible and important to them? Because he does not know all of the processes by which elements of the world become important to a mosquito, the question of what to abstract from this sensorium, he felt, became a question of observation. Whatever a milieu may be for a mosquito experientially Abram posed as epistemologically inaccessible. Instead, he thought he could answer other kinds of questions like: what do the mosquitoes react to? What do they like? What "turns them on"? What "stresses them out"? Abram believed he could know mosquitoes' worlds only through this process. All that a mosquito is aware of in perception, what we might term, riffing on Whitehead, "mosquito natures", could only be represented in reverse. In any case, a description of such worlds was related to, but distinctly beside the point of, this exercise. The sunset has "happened" when the mosquitoes swarm. $Sunset_2$ at least in practice, became fully coextensive with "swarm."

In this sense, the sunset for which Abram worked was an abstraction: from its dense and potentially limitless sensorial attributes were distilled only the qualities which mattered for "swarm." This abstraction, however, does not sit above the realm of the concrete, functioning as an immaterial category through which the material configurations in Abram's insectary were evaluated or compared. Rather, because the abstract sunset did not follow the abstraction practices of people (in which case it might look like, for example, a painting whose colors evoke the mood and experience of the sunset, or a literary description like Levi Strauss'), but the abstractions of mosquitoes, there is no space between its form and its effects. There is no mosquito-experience-of-sunset against which Abram's efforts can be measured outside of what it makes mosquitoes do here and now. Sunset$_2$ does not stand above the cage as an abstraction; it is reduced or distilled from the cage as an emergent capacity of the various qualities Abram strives to produce (light, light changes, and other sensory stimuli) Abram has introduced using an idea of the original sunset for inspiration. Once they can be seen to work, however, the idea of the "real" sunset as a source of conceptual inspiration loses relevance. In a very literal sense, it no longer matters once its mosquito-inciting properties have been distilled. It is those properties, and not the sunset as a whole or in itself, which is of interest to Abram.

This practice of abstraction, by which objects, sensoria or, as we will see, mosquitoes themselves, are distilled to their relevant qualities allows for what will come to feel at times a dizzying dance of transformations. If the motion of the setting sun can be distilled to measurable changes in light intensity and angles over time, it can be, according to this method of practice, transformed into office ceiling lights and strip lighting. If the shifting motions of the sun and the earth can be distilled to the horizon line, this can be transformed into a pattern of white plastic and black cloth. And if, as we will see in the following section, *Anopheles gambiae* itself can be distilled to a choice set of relevant sexual behaviors and characteristics, it can be transformed into *Anopheles freeborni*. It is notable that all of these transformations are contingent: nobody thinks the black cloth they are staple gunning onto a frame "is" the horizon, nor that fluorescent lights are the sun, or that *Anopheles freeborni* is *Anopheles gambiae*. The ability of each to serve as the other only works in specific material, epistemic, and sensorial milieus in which it is these qualities themselves, and not their bearers are believed to be the object of experimentation.

Dance Practice by Proxy

One of Abram's most immediate priorities was finding some mosquitoes to pay attention *to*. By this time, all of the mosquitoes from Sao Tome and Principe were dead. With the pandemic dragging on, he knew it would be a while before any more would come to Davis. Abram anticipated a process in which he would construct prototypes of the box, see how well they worked, and re-make it again. To do this, he needed a population of test subjects on which to observe its effects. But he had no access to access to wild *Anopheles gambiae* (which do not live in California), and he did not want to order some from the CDC MR4 collections. Those CDC mosquitoes, in addition to being thoroughly lab mosquitoes and thus less relevant for the reasons outlined above, would probably die in large numbers over the process of cage design and, as Greg had tetchily reminded everyone in a lab meeting, "the CDC is getting cranky about sending us so many samples that we promptly kill!" So, Abram went looking for proxies to "play with". He wanted a mosquito that was accessible, similar morphologically, and mated in swarms. Together with his PI Greg and in conversations with others, he landed on *Anopheles freeborni,* coincidentally named after the first chancellor of UC Davis, an entomologist. Greg agreed it was a good choice: "Yeah, I think behavior-wise it's pretty similar to *Anopheles gambiae*. It would be good for us to fool around with."

*Anopheles freeborn*i is a mosquito known to inhabit the rice paddies of the Central Valley. It was selected as an appropriate proxy for *Anopheles gambaie* in the testing of the design of the box because "they have a similar mating biology" and, as anophelines, are "relatively closely related" to *Anopheles gambiae*. "Most anophelines exhibit very similar mating behavior", Abram explained. Just as significant was their availability: Greg, the PI of the lab, contacted Sacramento Yolo vector control to ask them where there were good collection sites, and was redirected to Sutter Yuba vector control, where they said the numbers were larger. Fortuitously, Erik, a former member of the lab who had only recently left, lived nearby and agreed to keep in touch with Abram to let him know when numbers were higher in the late summer of 2020. Another advantage of using *freeborni* was their wildness. Unlike *Anopheles* samples they could have ordered from the CDC, these *freeborni* would go through a transition from "field" to "lab" similar to the one undergone by past and future Sao Tome *gambiae*. As Travis pointed out discussing Abram's idea of collecting from the rice fields near Yuba City, "the practice of taking from the field… you're going to have to adjust on the fly for collected stuff so it would be good practice." The lab/field transition was useful because it made the *freeborni* more similar to *gambiae* (and thus better proxies) insofar as they might display some of the same confusing "finicky" tendencies not shown by laboratory mosquitoes, but also because the process would be good practice for Abram, staging by proxy the field/lab transition which had failed last time to help him learn how to manage it in the future. Both he and the proxies would transition together, each learning.

Freeborni were also a good choice because, as Greg explained, "it has been colonized, but it's not a really easy mosquito to colonize." *Freeborni* were good proxies because they were colonizable (it had been done), but they were "finicky" "picky" or "discerning" in the manner of the failed *Anopheles gambiae* colony. A mosquito that was too easy, for example one of those that would "mate in a shotglass", would not have been good proxies. Colonizing them would be feasible, but hard, making them similar to *Anopheles gambiae* which have been colonized, but are known to be "difficult." They were also accessible (when Abram did go to Yuba City with his undergraduate mentee Elizabeth, it only took them a day of collection work), but wild (the process of moving from the non-lab world to the lab would simulate the same difficult unpredictable process that would happen with *Anopheles gambiae* allowing Abram to practice adjusting "on the fly"). Finally, the lab members decided they were similar enough to *gambiae* in terms of physiology (morphology, life stages, sexual biology) and behavior (they swarm at dusk and exhibit similar mating patterns like dancing). Once established, the similarities between *freeborni* and *gambiae* would allow Abram to extrapolate observations about the former's responses to the mating box to predict the latter's. Once he had a test audience to work with, Abram thought, he could begin a back-and-forth process of refining his design. His plan was to rear *freeborni* using the rearing protocol recorded in the PhD dissertation of a person named Gary, put adult mosquitoes into his cage, and observe if they swarmed (along with other behaviors like dying, feeding on sugar and blood, and inseminating one another). By analogizing *Anopheles gambiae* and *Anopheles freeborni* we can see the traits through which place comes to matter in the insectary. It emerges as behavioral patterns (like swarming), morphological characteristics, and personality traits (like being finicky). If those traits are enough the same, Abram can aim to address Sao Tome in absentia by "playing with" mosquitoes bearing the same qualities which make the Sao Tome mosquitoes both difficult and useful. Abram is working upon locality through the inside of the mosquito- by tracing which parts of the mosquito are judged relevant to the selection of a proxy partner, we can see Abram and his colleagues deciding which aspects matter in the bridging between wild-type and laboratory.

As we have seen, however, the substitutability of one mosquito for another does not hold regardless of context. Just as the cage and the sunset are substitutable insofar as they stimulate the same things from *Anopheles gambiae*, *Anopheles gambiae* is substitutable for *Anopheles freeborni* only insofar as each are stimulated by similar qualities of material environments. In the following section, we will see how attention turns towards these environments themselves. The complex density of an environment, taken as a multifactorial ambience or atmosphere, complicates the practice of abstraction through which Abram pursued substitutions.

The Room Does Things Mosquitoes Don't Like

Before he could initiate this process of tinkering with the box, however, Abram faced a host of unexpected problems. He had been thinking all about the cage, but his attention was soon almost entirely subsumed by an intermediary between place and milieu which had seemed insignificant at first. He had forgotten about *rooms*.

In recounting this saga of rooms, Abram began at the beginning. When the mosquitoes from Sao Tome and Principe had died, Abram had noticed that they weren't blood feeding. When they were dead, he dissected their spermathecae (organs used for the storage and maintenance of live sperm in the female body for later use). They were empty, confirming his suspicion that they had not mated. This "got me wondering about mating and feeding preferences. I knew about swarming, so I thought we might need a bigger cage." These mosquitoes had been reared in the insectary space that Alli and Kendra, the previous managers of living mosquitoes, had used for all their colonies. It is a small, humid, walk-in closet attached to the laboratory space (a large, clean room outfitted with microscopes and various pieces of equipment on broad gray countertops), which is accessed through a door from the office spaces. Lab members expose their identity cards to a gray rectangle on the door which flashes green and red to gain admittance; I had spent some time here previously either watching dissections in the lab space or standing awkwardly close to lab members in the insectary space as they showed me the setup in these cramped quarters. The mosquito cages stacked on the metal shelving in this area are, on average, roughly the size of two shoeboxes. The humidity is stifling after a relatively short period of time.

Abram knew that the lab also had access to a BSL3 (biosecurity level three) insectary space further down the hall in the same building which was larger than this insectary space. If the mosquitoes weren't swarming because they didn't have enough space, moving to the larger area seemed like an obvious place to start crafting a solution. It seemed reasonable, moreover, to have a mating cage which could service GEM's from UC Irvine when they arrived for biosafety experimentation (which would have to be in the biosecure unit). Moving to the BSL3 area, however, posed a number of unexpected problems related to the infrastructure of its biosecurity status. The problem with BSL3, as they call that space, is that the biosecurity regulations require it to constantly circulate the air. This precaution, appropriate for work dealing with various airborne pathogens, is anathema to the rearing of mosquitoes which requires very warm temperatures and high humidity. Abram later told his colleagues "I know Alli and Kendra were trying to use BSL3. They used humidifiers and shower curtains and I tried that, hoping for just dumb luck, but we put in sensors and saw the humidity is highly unstable there, and refilling humidifiers is a dumb task. So, I developed a different plant to change the room's alarms and set points." He shows a photo of himself and ten other members of the lab crowded into the small space. They tried putting humidifiers into the room. Then they tried hooking the humidifiers up to the hot water pipes to avoid the cross-purposes of simultaneously running heaters and humidifiers. They considered the purchase of "heating humidifiers" which are unfortunately extremely expensive. "Are they more sensitive to humidity or to temperature?" another researcher wondered, as they pondered the trade-off. "We just need to put them in there and see", Greg said. Travis developed a sensor system using a space heater and an old humidifier juryrigged to take in environmental data to regulate the room's atmosphere. Their attempts to add water to the air, however, resulted in lots of condensation. First, puddles appeared on the ground. Then the paint started to bubble. Then the horizon collapsed. The cardboard and tape demarcating the light and dark areas of the cage came unstuck, drooping forward like some melancholy statement on surrealism. The mosquitoes all immediately hid behind this fallen horizon, couldn't figure out how to fly back out, and died.

The BSL3 space also has motion sensor lights the team could not disable, so they put aluminum foil over them. They also "tried covering the vents with plastic bags to see if anyone would yell at us." A blue light blinking in the room reminded them that it is outfitted with an alarm system. What would make it go off? What would happen, really, if it did? Hans had a "wink and nod sort of thing" exchange with a facilities person who suggested that the alarms may not be a very big deal. Greg concurred. "All that happens if they do go off is someone calls you and you say 'oh, well, okay.'" Hans summarized the scene pretty aptly, saying "it looks very DIY up in there." Melina was more concerned: "you are using lots of things that are meant for other purposes. There is a reason they have those rules! It doesn't seem safe!" "The air system is designed to be safe if you're working with pathogens" Greg responded. "Normal houses don't have that. The room does things the mosquitoes don't like." When Hans and Abram told Greg how often the air was being replaced in BSL3- 7 to 8 times and hour- he looked exhausted. "It's impossible to humidify that much air flow. You just do the best you can to clean it." What they want, Abram said, is a Darwin chamber. Greg asked, is the one in Tupper ours? Can we move it? Hans said "it's, like, a room. A walk-in room." At this Greg exploded in frustration: "Tupper's not in San Diego! We're going to be doing some dopey dumb experiments for the regulators like making a fish eat them or having them bite a person. People are just crazy- what they like to do is to keep people out of this research. Every lab on campus is making a transgenic something with the windows open! This isn't Europe for God's sake!" Unfazed, Abram responded that the problem with the Darwin chamber is that there's no lighting in there. Could we rig it? Travis said that there's a port in there, you could run an LED strip.

Hans' clarification in response to Greg's question about moving the Darwin chamber opened up an important, and, in that moment, unclear distinction between an object and a room. Abram's original plans had been on the scale of the sunset cage; he was interested in how that object might "simulate" a sunset for a mosquito by creating for them an environment which felt like one to them- complete with its own horizon line, time zone, and nightfall. The issue of the BSL3 space, as Greg very eloquently put it, was that *the room does things* mosquitoes don't like. I have noted in my description of earlier stages of planning the distinction which emerged implicitly between place and milieu, with place indicating the geographical specificities of Sao Tome, and milieu the more affective experiential components of a sunset. While interrelated, these two concepts were held apart in cage design work, largely because researchers did not see all elements of place as necessary to the simulation of milieu. What we see emerging with the issue of rooms, particularly rooms that do unlikable things, is a mediating level between place and milieu- a cage, if you will, enclosing the cage which makes the sunset, one with its own atmospheric conditions. Rooms thus shift from sites where things happen (through the manipulations of the cage) to active doers of things themselves, often outside of the control any of the people involved. One might thus visualize the nested environments around the mosquito which may swarm by Abram's horizon line: the cage is within the room, which is within California. Abram's task became significantly more complex as he realized that the creation of a placed milieu would have to emerge within an interplay of nested environments, none of which were the environment he desired to evoke. What had begun as a two-partied practice of analogy began to expand in new dimensions. We see additionally a notion of security built into the infrastructure of this space. This notion of security, based on research dealing with airborne pathogens, is focused on controlling and containing the atmosphere of the room; maintaining a distinction and a separation between the room's air and the rest of the building's which, unfortunately and unintentionally, deeply maligned mosquito inhabitants.

Abram struggled with the complexity of this room- its biosecurity status, its ventilation system, the materials of its flooring and paneling, and its abilities to hold heat and moisture- because, within so many shifting elements it became more difficult to hold stable an environment in which mosquitoes could survive and, more importantly, be abstracted to their significant biological and behavioral elements. In the following section, we see him address his own practices of abstraction more directly. If the sunset (imagined as it occurs in Sao Tome and Principe) can be abstracted into $sunset_2$, Abram needs to distill the former into its relevant qualities, and then reproduce them in the latter. As we will see, however, this requires a working idea of what is, and is not, relevant to mosquitoes.

Living Their Best Lives

We might begin with yet another image. In a university building in the Netherlands in 1994, a healthy twenty-seven-year-old white Dutch man sat naked and motionless on a stool. He rested his palms on his knees. His feet he tilted slightly upward from the floor. A fine mesh cage surrounded his body on all sides, perforated by one tiny, almost invisible hole. It had been over nine hours since he last bathed. Just outside the mesh someone watched him. The temperature, humidity, and illumination of the room they occupied were precisely calibrated.

Through this hole the observer released a single female *Anopheles gambiae* mosquito, between five and ten days old, which had been starved the night before. For three minutes the man and the female were alone in the mesh. When one of them bit him, the observer entered, stroked his colleagues' skin to locate the bite she had made, and released another. They repeated this 200 times. In this way, R De Jong and B G J Knols demonstrated what they called a "preference" among *Anopheles gambiae* for human feet. The mosquitoes, they wrote definitively, were very attracted (De Jong and Knols 1995).

Twenty-seven years later, Abram would bring the paper they published on this experience to bear on his own labors to, as he phrased it, help his mosquitoes to "live their best lives." Abram needed the mosquitoes to copulate to stay alive, but he also needed them to stay alive to copulate. And so, as the coronavirus pandemic and its attendant travel restrictions wore on, he devoted more time to the question of what mosquitoes "really like" and how to give it to them. He thus mobilized an extensive literature on what are commonly referred to as mosquito "preference" and "attraction." Abram envisioned his work as the pursuit of material analogy- he needed to make a mating cage which operated as the sunset of Sao Tome. Connecting these two geographic poles, however, the substance of the analogizing connection, was the mosquitoes themselves. What makes a cage operate as a sunset cannot be determined by reference to human perception or reaction. Abram would know that he had succeeded in simulating the sunset when he stimulated the mosquitoes to swarm. Methodologically, the test for "sunset" was "swarm." Abram would never look for what *he* saw as a sunset; he would only know he had created them when he saw the swarm which, in this work, became a definitional aspect of "sunset." Through his research and experimentation on preference and attraction, Abram thus delved deeper inside this question of stimulation. As we will see, preference and attraction are concepts which shift mercurially between the interiors of mosquitoes and the material infrastructures and sensoria of their surroundings. It is this mobility- both the usefulness and the troubles it offers- that I explore in this section. I suggest that both offer a perspective on strategies of interspecies communication of events which are shared but radically dissimilar.

Here I want to describe how Abram mobilized these concepts to work with the two most important substances in the life of any mosquito: sugar and blood. Only female mosquitoes drink blood. Males and "virgin" females sustain themselves on sugar (or its variations). Following this work of, as he called it, "playing around" with these substances will return to the queer relationships of simulation and replication brought to bear on the Sao Tome sunset, in which things do not look like what they replicate. In the work of mosquito attraction, the passage between unlike doubled images takes a similar pathway through the mosquito itself. Unlike the practices of analogizing proxies, however, testing attraction and preference require a discomforting reference to mosquito experience which is both invoked and denied. Before we enter this strange world of blood and semen and odors, however, more needs to be said about these terms, preference and attraction, which I am here naming as concepts. Why are they so productive, yet also troubling? A look at how they emerge in the paper published by Knols and Jong and used by Abram exemplifies patterns standard in this literature.

The touching of one colleague by another after exposure to a starved female was carefully recorded. Each of the two hundred bites received by the naked man were carefully transcribed onto an outline sketch of a generic human male, and then onto an outline sketch distorted to reflect the uneven distributions of heat and sweat glands on our bodies (both known to attract mosquitoes). Even with this distortion, the statistical analyses the men conducted told them (they claimed in their paper) that more *gambiae* bites were found on the feet than would have occurred by the intermingling of chance, heat, and body sweat alone. Later, they repeated the same experiment, but washed the naked man's feet with medical soap every hour; the results, pictured on the bottom right corner of the image, demonstrated (again with the aid of statistical analyses) a significant reduction in bites on the feet.

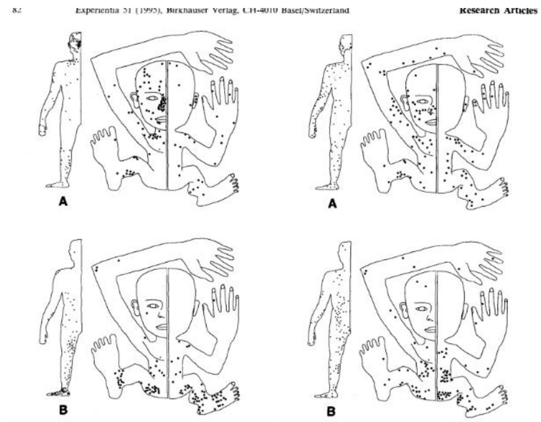

Figure 2. Distribution of biting sites of *A An. atroparvus* and *B An. gambiae* on a human host. One hundred bites per species are superimposed on a half silhouette (left) and front/back of the body (right).

Figure 3. Distribution of biting sites of *A An. atroparvus* when breath was removed and *B An. gambiae* when feet and ankles were washed. One hundred bites per species are superimposed on a half silhouette (left) and front/back of the body (right).

"Preference" thus emerges through the variable densities of mosquito biting, made visible by their transcription onto the distorted outline and academically defensible by the subjection of these bite patterns to statistical analyses against the null hypothesis of even bite distribution relative to body temperature and humidity. It emerged, in short, on the page. Preference was a statement about mosquitoes ("they prefer feet"), but it was only visibly true as evidence after a series of intervening stages. It is an essentially statistical concept based on the creation and manipulation of numbers out of the gentle palpitations of one man's skin by the other.

Attraction has a very similar meaning in this body of literature: that which mosquitoes prefer attracts them more. In contrast to "preference", however, "attraction" carries with it no implied comparison. It is both easier to see (sometimes described as inferred from simply watching cages for probing and landing responses) and harder to see (insofar as mosquitoes appear, most of the time, to be doing nothing clearly meaningful). Both terms are amenable to rapid, frequent shifts between active and passive: *gambiae* are attracted to feet, they prefer them. Foot odors attract the mosquitoes, biting preferentially occurs on the feet. This ability to rapidly shift from stories about mosquitoes' inner drives (their experienced preferences and attractions) to the properties of things is common in literature on mosquito feeding, olfaction, host-seeking behaviors and trap design. As we will see, this capacity makes possible certain kinds of hypothesis testing and rapport building with mosquitoes and those who work directly with them.

So how does one, as Abram aimed to, find out what mosquitoes "*really* like"? Following Jong and Knols, one way is to count. This was a method pursued by Abram when he asked: what types of sugars do *gambiae* (or their *freeborni* proxies) prefer? By giving them their most preferred sugars, Abram hoped to "kind of make it easier for them", coaxing travel-stressed and finicky individuals to eat "so everything is preferred, leaving as little to chance as possible."

Abram felt confident that sugar is attractive to mosquitoes- he had seen them land and probe on it very often. He had also read papers in which mosquitoes were fed orange juice, acaia honey, or fresh fruit rather than refined sugar infused water alone. The compounds in these fruits, it was speculated, may provide antioxidants which slow down the aging process in the mosquitoes. But, to find out if they *really* prefer sugar water or orange juice, Abram wanted to get rid of the confounding variable of color (it could be that they are attracted to juice because of its color, rather than its smell, taste, or nutritional properties. So, he dyed the liquids to see if color affected sugar choice. To gather numbers from this work, he intended to dissect the guts out of mosquitoes after this last meal. Greg, however, suggested it would be far more efficient to simply smash their bodies whole against white filter paper, and to count from the lurid artworks thus produced numbers of colors which would reflect "selection" and thus "preference." Here we see bodies transcribed into the process of preference creation as they are smashed into colored splotches, a transcription which, counterintuitively, bypasses questions of their interior experience altogether. The mosquitoes can literally become tally-marks translating their preferences.

In the smashing of the mosquito body into the white filter paper, we might find an image useful for thinking Abram's abstraction work in other areas of practice as well. The capacities of "preference" and "attraction" to move between being qualities of objects and states of desire introduce ambiguity, but also make work functional because they can, put to work in certain ways, introduce mobility or even blurring between material object, subjective state, and behavioral response. Preference *is* the smash, the meeting to the point of indivisibility between the mosquito and the recording media. As a concept, it emerges between the two. As an abstract concept, it draws our attention to the salient aspects of each (our eyes are drawn away from the legs, the hair, the texture of the paper, and towards the colors), introducing these qualities as objects of research themselves. The mosquito body is transformed into preference. The recording makes the mosquitoes' preference real; the mosquito body, violently disassembled, is transformed into the concept. "Preference" is thus neither strictly a quality of either a material object (like sugar) or an internal state of a mosquito. It is an abstraction distilled from their indissoluble meeting point. We might similarly view the sunset cage as a work of abstraction. The "sunset" it simulates is neither a behavior of the mosquitoes (swarming) nor a material infrastructure alone. Rather, it is an emerging event which attains its status as an abstraction by the carving out from sensorial complexity and behavioral variation only some fine threads. Properly refined and correctly traced, Abram and his colleagues believed these trace elements, these isolated qualities of mosquitoes and of our world, can make mosquitoes do things. But how does one know when an effect has been achieved? Mosquitoes are wildly uncharismatic animals. They have no faces, they express few recognizable responses and, most of the time, they appear to be doing pretty much nothing. If the sunset is an abstraction achieved in the smash between material environments and mosquito desires, learning how to see this smash happen turns out to be, sometimes, surprisingly difficult. The practice of counting addressed this issue for Jong and Knols and for Abram in his experiments for sugar. But in avenues where enumeration doesn't work, how can Abram know, as he phrased it: "what am I looking at?"

We might start with the example of cheese. "Wild mosquitoes" Abram related exasperatedly to his colleagues, "*loathe* artificial membrane feeding. With the ones Erik and Irka [former lab members] had from the field, they hated it. Ours hated it. It won't be a surprise if, when we bring more in, they hate it too." Abram had experimented with a number of substitutions to the parafilm membranes normally used to encase the blood fed to laboratory mosquitoes. Blood sausage casings seemed like an intuitive choice to mimic the texture and mouthfeel of human flesh. He also tried lambskin condoms which hung, distended with hot blood and proudly erect, from the metal slats atop large cages.

In addition to these more appealing textures, Abram thought he might also apply what he had learned from Jong and Knols about *Anopheles gambiae* and human feet to the problem. From their work, he knew that these mosquitoes prefer human feet; by extension, he felt confident that feet attract them, and that they are attracted to feet. Could he make them more attracted to blood in the laboratory by exploiting this attractive quality of feet? Abram and Elizabeth, his undergraduate mentee, pursued this line of inquiry by again drawing upon the published experiences of other insectary workers, who had tried scraping the dead skin off of their own feet and culturing it, wearing socks for seven days straight ("not so hard for *some* people in this lab!" commented Greg), or culturing their own dirty socks to produce that je-ne-sais-quoi foot quality. Anton, the experienced entomologist, offered Abram advice he had learned from Hollander entomologists, who had found that nylon absorbed and retained human foot odors better than any other material. "You don't have to wear the ones all the way up to the waist", he reassured his younger colleague. "The ones up to the thigh are good enough." While odor was presumed to be the factor attracting *gambiae* to human feet, the exact elements or mechanisms of this attraction remained obscure, and Abram and Elizabeth found reasonable others' conjectures that the secret must lie in the distinctive microbial communities which colonize us there. Whereas Jong and Knols had suggested that their research may be useful to those seeking to design more efficient traps for *gambiae* mosquitoes,

Elizabeth was the one to suggest that this knowledge may also be applied to laboratory blood feeding. Moreover, Elizabeth and Abram recognized that individual foot microbiota may vary among people and so, in their search for a standardizable, replicable, and predictable foot odor they turned to what seemed like a safer choice. Limburger cheese does smell a lot like feet, and scientists have confirmed that it is cultured with a mix of bacteria very similar to that found on human feet. Abram and Elizabeth tried to see if they could use this cheese as a foot substitute-would it be attractive in the same way as feet? Could they isolate "attractiveness" from feet, cheese, and mosquito behaviors to produce something that would induce mosquitoes consistently to feed?

Working with the cheese became Elizabeth's primary project. She tried rubbing cheese on parafilm membranes hot, cold, and room temperature. She tried leaving it out of the refrigerator as well, thinking that this may increase its attractive foot-like capacities ("it smelled more pungent to *me*”). Elizabeth then watched the *freeborni* proxy mosquitoes in the presence of variously cheese-treated membranes. She felt disappointed and confused, however. These mosquitoes feed at night, so when Elizabeth opened the door and the automatic light turned on, she suspected that the mosquitoes may have dispersed from whatever attracted behaviors they might have been doing with the cheese in the dark. Moreover, even when she could watch them continuously, Elizabeth wasn’t at all sure how to understand or record what she was seeing. “What counts as attraction?” she asked despondently at a lab meeting. “Landing directly on a sample? Near it? … Are they more attracted to heat than to the actual cheese?” Elizabeth could not make sense of what she was looking at. From the hazy, seemingly random floating around of the mosquitoes in the lab she did not yet know how to make a story about attraction.

Good mentor that he is, Abram was helpful in teaching Elizabeth how to see. He demonstrated an example of this acquired vision in a lab meeting in June. Here, he was relating his latest adventures with the sunset cage. He related how he used LED office light panels (the kind which normally go into the ceiling) “for daylight.” White sheets were used for light dispersion, and LED string lights wrapped around the front and side for “crepuscular light”, or the dimming illumination of dusk. He had added this time a terra cotta pot he had sanded down “for them to cling onto easier” and a moist sponge “in case there’s anything stressful in the cage.” The light levels had been calibrated to the light in the rice paddies from which the *freeborni* were collected using a borrowed Lux meter. “So” he said to his colleagues, “what does a swarm of mosquitoes look like, and what do I look for in a cage?”

To prime his audience, he showed them a video in which a truck, a trailer, and two men stood against the sunset at the edge of a broad flat field, overwhelmed by a dense and enormous swarm of black dots. The mass dwarfed the men and their truck, and it assumed various shapes, the transitions between which happened both suddenly and nearly imperceptibly. Like a flock of starlings, it exuded its own kind of spooky chaotic intelligence. Abram explained that this was a video he took with Scott Carrol, a biologist at UC Davis. “You’ll notice the low, even light and the truck and trailer they’re using as a swarm marker. In the video the swarm is really big and really dense. I’m not positive this is *Anopheles* because at the time I wasn’t really paying attention, but these are definitely mosquitoes. With that in mind, here are the swarming behaviors I saw in the cage.”

Abram then switched to a second video, this one as grainy and minute as the last one was massive and obscure. We saw a few black dots floating hazily against an out-of-focus antiseptic looking space. “I want to point out this dark horizon line you see from here down. It’s all black cloth that Hans sewed in. What I looked for is this ‘dancing behavior’ males do when they initiate. So this horizontal looping they do around the horizon, and once more males are recruited they do a sort of vertical jumping flight path.” Abram stopped, and then set up the video to replay. In order to see this dancing, he told his colleagues, “Pick your favorite mosquito and try to track these behaviors in the video.” Abram did a short lesson then in learning to see mosquito swarming by priming his audience with the visual of a very large and unmistakable swarm, and then by suggesting a new pattern of attention which would make the most basic elements of the swarm (the male dance) perceptible to the uninitiated eye.

Abram was teaching his colleagues a different way to pay attention to mosquitoes. For example, to look for the swarm not in the form of its aggregated shape, but in the extremely small and delicate first dances which precipitate its formation. In this way, he could draw their eyes along a linkage between the movements of a single mosquito in the blurry video and the event of the swarm (and the sunset). This way of seeing, like Jong and Knols’ diagram, or the counted colored bodies on filter paper, held together durable-enough stories about mosquitoes and their patterns of behavior in relation to important objects and substances in their surroundings, patterns which were often named with the terms “preference” or “attraction.” These concepts also held together a series of paired but apparently unlike images: the human foot and the lukewarm cheese, the nylon thigh-highs, the petri dish of bacteria; human flesh and the lambskin condom; plant nectars and bottled orange juice and, of course, the sunset and its automata in the form of the cage. The concepts of preference and attraction are very useful to Abram (and others working with mosquitoes) because they offer a method for substituting unlike objects for each other through a practice of abstraction. Through this process, things that do not resemble or represent each other can nevertheless “function as” each other. The cheese can be like the foot because the two stimulate the same thing in mosquitoes, a stimulation articulated through these two concepts. Likewise, the cage may look nothing like the sunset, but it can “function as” one if it activates mosquitoes in the same way. This substitution through abstraction, as we have seen through the extensive attention to precise measurements, is not about a vague similarity. Rather, exactitude runs along a different axis than appearance, through the qualities of substances, objects, and environments which do things to mosquitoes. It is these qualities which are reproduced in variable new forms through the extensive, often surprising, frustrating, and confusing, play done by entomologists and others working directly with mosquitoes. These qualities acquire significance insofar as they are believed to engage with the preferences and attractions of mosquitoes. It is through these concepts that we find people producing a number of re-creations which bear no resemblance to their originals. It is the abstractions created in the movement between them that hold feet and cheese, flesh and condoms, and the sunset and the “sunset” together.

This method of abstraction has particular advantages when it comes to mosquitoes. We might return to Abram's comments about why he loves working with them so much. "They are the closest thing you can get to an alien" he told me. There is no ethics of the face-to-face with a mosquito. In their size, their facelessness, their sheer difference from us and their loathsomeness they are obscure. And yet, Abram manages incredibly complex feats of elicitation and persuasion with his charges. Coaxing finicky wild-type mosquitoes to survive in the lab requires of him complex imaginative efforts to think of what may be required for them to eat, to have sex, and to "live their best lives" in his care. And yet, Abram does not imagine that he can imagine the experiences of these mosquitoes; he does not speak with any seriousness of internal states. I contend that part of what is so useful about preference and attraction here is that both of these concepts permit a fluidity or agnosticism about mosquito mindedness. Like other insects, mosquitoes are believed by the researchers to have "simple" behavioral patterns; unlike a dog, a chimpanzee, or a corvid, for example, they do not believe that they "learn" or "think." However, they also do not respond as predictable machines. If there were clear, predictable input-output relationships between objects, substances and environments and mosquito lives and copulation, no part of this work would have been necessary. It is in the space from being mechanistic that Abram's work dwells. As he continues to learn from his experienced colleague, Anton, it is the work of a lifetime to get what we might call, following Evelyn Fox Keller on Barbara McClintock, a "feeling for the organism." Moreover, Abram and Anton both draw extensively on publications from other people working with mosquitoes who report their tips, tricks, trials and errors. Speaking about preference and attraction allows researchers to oscillate easily between statements which refer to mosquitoes' feelings (of attraction or preference) and ones which refer to qualities of substances or things (which are highly preferred, or attractive). The result is an emergent abstraction, mobile between dissimilar materials but never disembodied, held apart or above the shifting transformations they direct.

In “Suspending Belief” Epoche in Animal Behavior Science”, Matei Candea discusses behavioral psychology researchers working with meerkats and corvids. Among both, explicit reference to animal minds, or general “anthropomorphic” language which draws analogies between the feelings and behaviors of crows or meerkats and human beings, is professionally disallowed in published literature. This detached attitude, in which researchers deny propositional belief about animals’ inner lives, however, is paired with intersubjective relations mobilized in working directly with the animals, where working beliefs that people do, in fact, know what is going on with the animals with which they are in contact, is widespread and useful. He shows in his fieldwork with meerkat people and corvid people that they no rely on this intersubjective “trust”, although it is conceptualized by them as a “tool” and not eligible for entry as scientific “evidence”, the parameters of which are defined by a history of thinking in the field of behaviorism. Candea uses his description of what he calls the researchers’ “simultaneously detached and engaged attitude” to intervene in current anthropological conversations about nonhuman others and the relationships people make with them. He suggests that there are two main ways of thinking about attitudes toward animals in anthropology and cognate disciplines. One, which he labels the ontological turn, frames “Western” approaches to animals as based on a Cartesian refusal of perspective or personhood (referring primarily to Descola’s concept of a “naturalist ontology”). The other, which he associates with the growing literature in multispecies ethnography, says that it is morally wrong to think that we have no access to what nonhuman others think and feel. Like many of these writers, he distinguishes between propositional belief in nonhuman minds and a practical “trust” which takes place in engagements with them. However, while he sees these authors as framing a refusal of propositional belief as a betrayal of trust which extinguishes the possibility of meaningful interspecies relationships, Candea argues that this is not the case: that researchers do, in fact, establish meaningful relations of trust while avoiding propositional belief about animal minds. It is not that people fully "belong" to different ontologies, he argues, or that they "switch" between then, but that they take an active orientation to their own beliefs. He uses the Greek term "epoche" to name this "simultaneously ethical and epistemic 'spiritual exercise' of suspending judgement or withholding assent to one's immediate perceptions." Much of his approach is drawn from Jonathan Mair's work on the anthropology of religion. Anthropologists should attend, Mair argues, to people's active cultivations of their own beliefs- an active cultivation which Candea finds echoes among behavioral psychologists (Candea 2013).

Unlike these researchers, Abram is not self-consciously suspending his judgements in order to better study the subjective capacities of his mosquitoes. Mosquito behavior is not, strictly speaking, his interest or his purview. Moreover, he feels no temptation to anthropomorphize his mosquitoes the way others may feel with organisms that have more recognizable eyes. Rather, Abram aims to keep the insects alive and reproducing to be used for other purposes. As such, he needs to infer and create settings amenable to wild-type *Anopheles* life (well aware, as he is, that there is no one mosquito way of life). As such, hypotheses about mosquito mindedness or subjectivity emerge in the process of hypothesis-making, or what Abram calls "playing around" with their environments in the lab. Do they like a wet sponge to rest on? Can they hang on to sanded terra cotta better? How do they like the mouthfeel of sausage casings? In these moments, Abram imagines his way into the alien, but he retains a consciousness throughout that his imaginative exercises are based on no such feeling of intersubjective "trust" as described in regard to researchers working with more expressive animals. As such, he is not in the practice of withholding assent to his immediate perceptions regarding their subjectivity. This is because mosquito subjectivity is not part of the problem that faces him.

Abram needs to keep the mosquitoes alive and reproducing. To do this, he decided the best avenue was to create what I have termed here sunset$_2$ – an abstraction of the sunset under which they swarm and copulate on the islands of Sao Tome and Principe. Targeted thus at the abstraction of sunset$_2$, his attention was thus not focused on mosquito subjectivities, or what the world looks like from the vantage point of such subjectivity. These he considered epistemically inaccessible. Mosquito experience and mosquito worlds thus enter the frame only insofar as they become relevant to the question of *how to abstract sunset$_2$ from sunset$_1$.* What qualities need to be retained? Which can be carved away? The logic through which he conducted this process was drawn through mosquito worldedness without considering it directly. His question was not: what is the sunset to a mosquito? But rather: what about the sunset is necessary to mosquito copulation?

The behavior of mosquito copulation and swarming, rather than an interest in mosquito-being, motivated Abram. His project was one of learning how to entice mosquitoes to do things; how to captivate them with a sensorial milieu to elicit them to dance. Abram saw himself as innovating conditions under which mosquitoes could be stimulated to (do the) act; at the same time, however, he was also captivated. He was made to act by the behaviors and responses of his mosquito charges, when they danced or did not, when they died or hid or ate, and by his materials as he struggled with plywood, electrical systems, and the overwhelming plenty of

Lowe’s hardware. He wanted to elicit a performance from his mosquitoes, but they also elicited performances from him. The cage emerged as a material nexus of this interplay.

Abram and his mosquitoes both became trapped in this dance. Abram was put under requirement to the needs and preferences of his mosquitoes as he crafted for them an environment which worlded them both. That this environment could not be a tool of domestication posed unconventional problems for him. Abram needed to make a space in the laboratory that did not function like a laboratory from the mosquito’s point of view. Without knowing what that point of view was, he had to make something that acted on them as a sunset does. Relating this process opens up questions about interspecies communication. What can we do with and to each other without speaking the same language, or seeing the same world? The object of “sunset”, for example, shatters in this practice. There is the sunset of the island as Abram or his colleagues might imaging it. There is data on temperature, humidity, light and other physical factors that he could work to replicate with his own measurement devices. And there was the mosquito experience of sunset—cordoned off, for him, behind a permanent veil of impenetrability. Like Levi Strauss who knew that a communication of the sunset could not afford to take no account of the practical aspects of the matter, replication by measurement was, by itself, not enough for Abram to achieve an effect on his mosquitoes. Here we see how he moves between various strategies, and various sunsets, to elicit copulation. There was no notion here of reproduction, in which the qualities of the original could be fully known. We cannot, he insists, understand the mosquito’s sunset. Rather, his creation is an abstract work, identifying and then generating stimuli, sensations, and response patterns. From this we can think about ways of being together in a dissimilar world which do not reach for an enduring description of what really is, for all beings under the sun.

Chapter Four: The Relationship Based Model

Introduction

The UCIMI trial aims to prove that their idea for genetic modification for mosquito control can work not only in the laboratory but also in the field. By this they mean the whole world that is not the laboratory, especially those parts of the world that are home to *Anopheles gambiae*. The scientists who lead this trial have decided that small isolated African islands are the best parts of the world to conduct the trial on, because they would best demonstrate that their idea can scale up to larger spaces, and that it can bridge the qualitative differences between laboratory and field described in Chapters Two and Five. They want to release the mosquito designed in Irvine there. But first, they need permission. In this chapter, I address what is known as "community engagement", and I focus on the model the group has adopted to pursue it. Much of this work covers the labors of entomologists and population geneticists engaged in a project of biological control. I have been interested in previous chapters to outline the form that control takes. But what of control when we turn to networks of relationships between persons?

The UCIMI trial is unlike most other malaria control projects throughout modern history in its dominant focus on mosquitoes, as vectors. One of the key benefits of a genetic engineering approach, to practitioners, is that it relieves pressure on human behaviors. If the mosquitoes can be delegated the task of killing or eliminating themselves as vectors of disease, there will be less accommodations required by people living in endemic areas like using bed nets, taking pharmaceuticals, killing mosquitoes or eliminating breeding sites, or otherwise altering ecologies and built infrastructures. This hopefulness reflects a consensus in the field of malaria research that these existing tools and practices for combatting the disease are not enough to reach eradication. For much of malaria's history, however, and especially its modern history characterized by a tight interconnection with war and colonization, however, malaria control was also about controlling human populations. Different styles of governance of "the bitten" not just for malaria but also for other insect-borne diseases have emerged out of and perpetuated different social and political imaginaries of control. John Ford, for example, describes how British colonial practices of combatting Trypanosomiases, spread by the tsetse fly, disrupted existing means of limiting fly-human contact by taking from people control over their environments, mobilities, and work styles (Ford 1971) Approaches to governing social and ecological "spaces of biting" are linked with political programs and ideologies, as well as implicit theorizations about organization in human and nonhuman worlds (Carter 2007)

Genetic techniques are framed as removing insect-borne disease control from questions of human governance to some extent. Instead of proscribing human habitations and behaviors, the thinking goes, scientists can make a "flying public health tool" that works on its own (Beisel and Boëte 2013). Of course, these genetic techniques come with what the scientists call "social issues" of their own, most notably questions about regulation, safety, and risk. In this chapter, however, I look to the ways in which implicit theories about human relationships, and the systems those relationships form, are active in the trial set-up stage of this kind of work. At this stage, scientists face as their most pressing questions issues of consent. How to get people to agree to host an experiment in their countries, and in their homes? The ways in which project organizers approach this question reflect, to them, quite obvious common sense. Their strategies and language fit neatly within not only prevailing consensus among international organizations governing the ethics of such trials, but also more broadly accepted dictums about how people have relationships, and what relationships between people do in the world.

I introduce here the Relationship Based Model proposed by Ana Kormos, heading this work, to guide her engagement with regulators, politicians, health workers, and especially "communities" in the two islands the group has selected as potential trial sites: The Union of the Comoros and Sao Tome and Principe. How does her conceptual framework for thinking about humans relate to, and diverge from, the paradigm of control employed by her colleagues in entomology and genetics? What common roots do they have, and what can thinking within the same project on mosquitoes tell us about thinking on people and vice versa?

To approach these questions, I explore the conceptual underpinnings and intellectual history of the Relationship Based Model. I am interested in this chapter to trace the logic of Ana's model, both by putting it in the context of ongoing contemporary conversations around mosquito genetic engineering, ethics and community education and by unearthing its genealogy. I learned that Ana cites the work of two psychoanalysts in 1950's and 1960's London as its progenitures. I explore here how Enid and Michael Balint's work on "patient centered medicine" transformed into Ana's Relationship Based Model and show how, along the way, key elements of their thought and practice were repurposed and altered to suit new concerns in nursing, administration, and community education. Seeing how this idea changed over time highlights the historicity of Ana's own formulation. I ask here how it became obvious, as it is in her text, that forming relationships leads to the establishment of shared, as opposed to conflicting, interests. How did "relationships" come to represent an unqualified good? How did they come to be identified so strongly with a positive affective experience of being in a relationship? And what structural role do those affective experiences play in how organizations like the UCIMI think about the negotiation of interests, benefits, and risks—or what we might otherwise call politics?

The Context of Genetically Modified Mosquitoes and Community Engagement

In 2017 the Vector Genetics Lab was just beginning their partnership with UC Irvine on the field trial to test the gene drive-bearing *Anopheles gambiae* mosquito as part of the wider UC consortium project led by the UC Irvine Malaria Initiative. The Vector Genetics lab's remit was to be the fieldwork portion of the trial. Fieldwork, in this area of research, usually means that scientists have to go somewhere. The "field", for them, is hypothetically anywhere outside of the laboratory. The field is not only a geographic marker, but a biological one: scientists doing fieldwork are interested in what they call "wild-type" mosquitoes, or mosquitoes whose ways of being (most importantly, here, their genetics) are considered to be in some way locally distinct. To explore mosquito genetics, it is normal practice for scientists at the Vector Genetics Laboratory, especially those trained in entomology, to look to human and geographic landscapes to understand mosquitoes. This direction of inquiry is described in more detail in Chapter Five. Fieldwork, however, also moves in another direction, with which most of the researchers feel significantly less comfortable. Human places are important for mosquito genetics, but they are also important for humans. In this chapter, I address what the UCIMI and many others in this and adjacent fields call "community engagement." This refers to the interface between the scientists, usually conceptualized institutionally, and people outside of their work, usually conceptualized as "communities."

Fieldwork planning is something Greg and Anton have done “a billion times”, and they know how to get together itineraries, lists of supplies and personnel, agree on budgets and grapple with collections permits and transfer agreements. They have decades of experience setting up the logistics of entomological projects outside of the United States, but never something this large or this high-profile. When they signed on to the project, “it was never our intention to take on regulation and community engagement work, but it sort of evolved that way. We thought it would be as it was in the past, we would contact entomologists and get going that way, but here we needed to be transparent about GE, and it became a whole thing.”

The “whole thing” that is genetic engineering (GE) is quite a big thing indeed. Aware of widespread popular distrust of genetically engineered food crops produced and sold by multinational corporations like Monsanto, mosquito geneticists (and most people involved in projects, such as those aimed at invasive species, which propose to produce and release genetically engineered organisms) are extremely wary about the topic of “public opinion.” If they are to lose in “the court of public opinion”, some researchers fear that their projects may be undermined or disallowed altogether. While gene drives are considered for a wide range of different uses, people with stakes in all of them are drawn together on this topic: should one project attract enough public disapproval, it could mean regulation for everybody.

A key issue related to that of public opinion is community engagement. A lack of community engagement was widely seen in the field of mosquito genetics to have harmed public opinion in the case of Oxitec, a British company which produced “sterile male” *aedes aegypti* mosquitoes (the main vectors of yellow fever, dengue, chikungunya, and the Zika virus) using genetic engineering. The company, now owned by the multinational corporation Intrexon, released several thousands of these mosquitoes in the Cayman islands in 2009, but critics said that they did not adequately inform the local population that they were doing so beforehand or conduct adequate risk and safety assessments (Subbaraman 2011; Reeves et al., n.d.). Bad blood around Oxitec contributed to a several-years long controversy in Key West, Florida, where the company hoped to use the same insects and a group of locals organized against it. They cited many concerns, but a sentiment that the company was secretive and untransparent in their dealings fueled much of their antagonism. As far back as the 1960s, genetic techniques for the population suppression of mosquitoes created controversy when a US led WHO funded program in India was accused of using the country as a testing site for the creation of insect bioweapons and the expansion of US neo imperialism ("Oh, New Delhi; Oh, Geneva" 1975).

Gene drives offer enormous potential for transforming organisms and ecosystems. Hypothetically, they could be used to modify (or suppress) the population of any sexually reproducing organism. Should genetic engineering techniques continue to progress, as many in the field expect them to, what *could* we make living organisms into in the future? Current applications mostly focus on reducing the numbers of invasive species, or countering what people see as their harmful effects on ecosystems. But malaria- fighting mosquitoes-- very likely UC Irvine's— are probably going to be the first. Researchers in this field are aware of themselves as operating a sort of "test case." If it goes well, this could open the door to many other uses of the technique. If it "loses" in the court of public opinion, that could mean and end not only to this project but to many others and potentially, in the eyes of some scientists, to a potential new way of being with the natural world.

Part of why these mosquitoes are attractive as a first use is because malaria itself is currently so damaging to human health. There were 229 million cases of malaria in 2019 according to the most recent WHO World Malaria Report, and 409,000 deaths. If public opinion is a court room, proponents able to make a strong case for “the cost of doing nothing.” As one Washington Post headline put it, “What’s Scarier? Tinkering with mosquito DNA or malaria?” (*The Washington Post* 2015) Nearly all of the people working at the UCIMI, however, are “hard scientists” with training in entomology, population genetics, mathematics, or biology, and many of them expressed an uneasy bemusement at what felt to them like a foray into the softer sciences.

The UCIMI mosquitoes (or those of their “nemesis” Target Malaria) are widely seen as having fairly good chance of flying out into the world and of being received warmly by the people in it—or at least a better chance than other projects, like ones focused on conservation for example, whose cost of doing nothing is considered less dire. But researchers feel they have learned from the Oxitec example that success requires them avoiding the creation of what they call mistrust between themselves and the public. This is a fundamentally consent-oriented framework for thinking about the relationship between science and publics: the ultimate goal is acquiring consent and keeping it. More approval is always better, and to win that approval one must invest time and resources into understanding what they broadly understand as culture.

The regulation of genetically modified mosquitoes has been, from the beginning, a confusing question. Regulations exist for the importation of natural enemies in classic biological control work (“Guidelines for the Export, Shipment, Import and Release of Biological Control Agents and Other Beneficial Organisms” 2021) which can have significant off-target effects (Follett and Duan 2012). Regulations also exist for the movement and uses of genetically modified agricultural crops. It has not been immediately obvious from the beginning of discussions about genetically modified mosquitoes in the early 2000s, however, which regulatory authorities are responsible for these organisms. Genetically modified insects, neither clearly technologies nor natural organisms, which both kill and are beneficial, are among a class of new "bio-objects", "biomedically produced life forms that challenge juridical, political, ethical and cultural ordering systems" (Vermeulen, Tamminen, and Webster 2012). As the WHO puts it in their Guidance Framework for Testing Genetically Modified Mosquitoes, "Humans have a complex relationship with the environment, variably acting in ways that either instrumentalize nature or protect it. Genetic engineering complicates this relationship by introducing the ability to do both things at once" ("Guidance Framework for Testing of Genetically Modified Mosquitoes, Second Edition" 2021). The writers of this overarching guidance framework acknowledge that the elimination of species made possible by population suppression techniques, is objectionable to some who believe that all species have intrinsic value, and that others may find the permanent and wholesale modification of existing species "unnatural." "There is no way" they write "to resolve conflicts about deeply held philosophical and cultural beliefs regarding the moral status of species, or their rightful place and that of humans in a shared, complex and interconnected environment. There are however, two ethically significant points to consider in the context of GMM research. First, judgements about interventions based on characteristics of what is natural or non-natural should be avoided, since provenance has no bearing on an intervention's potential for harm or benefit; for example, vaccines are not natural, but pathogens are. Second, in accordance with the principle of justice, the opinions of those most impacted by a GMM release should hold the most weight, since they bear the bulk of the associated risks and burdens" (91).

The question about how to govern them often comes to be one of legal analogies: what is this new organism most *like*? Luisa Reis-Castro has described how different choices about how to draw analogies with genetically modified mosquitoes, what she sees as competing processes of bio-objectification, result in different forms of governance (Reis-Castro 2012). I saw the issue come up very explicitly during fieldwork with Brazilian regulators, politicians and public health authorities in 2016 who were organizing the beginning of a large-scale roll-out of Oxitec's aedes aegypti mosquito. While provisional commercial approval had been issued by Anvisa (the Brazilian health regulatory agency), full commercial approval has remained pending for nearly a year. Public officials involved in the process of initiating and managing the trial in Piracicaba, a city in which I conducted fieldwork, speculated at length about the difficulties of regulating a "lab created insect" through Anvisa, the purview of which is food and drugs. A local ecologist involved in the process speculated on Anvisa's reaction to the insect: "I don't know if I have the competence to judge this. Because the people don't eat mosquitoes. It's not a medicine." The head of zoonosis control also reported uncertainty. "Is it a drug, or…?" Piracicaba's District Attorney, Maria Christina, seemed confident that Anvisa would issue full commercial approval- as soon as they figured out what to approve the insect as. "Because you have the normal biological control. Like when you use wasps to kill bugs and… that's okay. You may need some authorization from the agriculture ministry. But now it's not just the wild animal you are releasing. You created an animal. You just created a new animal. Whose use is to combat disease." Maria Christina saw Anvisa's delay as reflecting the time it might take to create the necessary bureaucratic infrastructure to approve an unprecedented object. Piracicaba's Secretary of Health, Pedro Mello, likewise attributed the delay to Anvisa "not having any experience with this kind of agent, it wasn't a drug, it wasn't a pesticide, it was completely new so they were trying to develop something to fix it." Mello, however, believed Anvisa's approval to be unnecessary because the insect was "much more an environmental issue" because it "doesn't bite." Male mosquitoes do not bite. It may be pointed out here, however, that .2% of the transgenic insects released are female, due to error. Because females do bite, it is unclear whether this claim is true in practice as well as theory. CNTBio, the Ministry of Science, Technology and Innovation, therefore seemed to him the only truly relevant authority.

Is it like a drug, a pesticide, a wasp? The mosquito shifts in these moments between comparisons with environmental technologies, like GMO crops, and biomedical technologies, like drugs. Stabilizing the nature of the insect's body was contentious both within the language of the law and in the minds of local officials dealing directly with constituents and national regulators. Aedes do bem, as the Oxitec mosquito was marketed in Brazil then, did not arrive as an obvious and durable object in Piracicaba. Rather, the habits and obligations imposed by this new mode of vector control worked to materialize it. Choices about language and categorization worked dialectically with the scientific and social infrastructures which created viable materiality and sociality for the insects in Piracicaba. The insect here emerges as a "biolegal entity" (Lezaun 2006) dependent upon labors of framing as much as on scientific research and rearing facilities.

Genetically modified insects are governed by the Cartagena Protocol for Biosafety, an agreement under the Convention on Biological Diversity which functions at the international level. It influences regulatory processes and risk assessments as they are conducted on the national level, but most projects like that of the UCIMI are governed also by national regulations, many of which (as in Brazil) need to be pulled together in an ad-hoc manner. Often, this involves investment from the organization running the trial to fund training and organization of local regulators. This is considered a form of "capacity building", other aspects of which we will turn to later. The WHO also publishes a guidance framework for the testing of genetically modified mosquitoes, the most recent of which was published in 2021. Tony James, the PI of the

UCIMI project, has been a major contributor to discussions about regulatory infrastructure and GEM testing (Ramsey et al. 2014).

While the legal regulation of genetically modified mosquitoes remains in flux, all participants to public discussion on the topic agree that “respect for communities should be an overarching ethical goal in GMM trials” (“Guidance Framework for Testing of Genetically Modified Mosquitoes, Second Edition” 2021). Good community engagement, for trials of genetically engineered mosquitoes (GEMs) also called genetically modified mosquitoes (GMMs) does not mean individual informed consent. “Simply living in the vicinity of a GMM release is insufficient grounds to require informed consent from any individual for an open release of mosquitoes” (“Guidance Framework for Testing of Genetically Modified Mosquitoes, Second Edition” 2021). This is because person living in the vicinity of GEMS/GMMs is not considered a “human subject” of research because they are not “intervened upon directly or deliberately.” In place of informed consent, the idea of “community” grounds a trial as ethical or unethical. It is not people who must be asked (since, in any case, the number of people potentially “affected” is hypothetically limitless if gene-drive bearing mosquitoes spread over the entire world and transform the species anywhere it exists). Instead, it is the authorization of communities which is sought. What exactly this means continues to be a topic of quite vague discussion. “While many who call for ‘public,’ ‘community,’ or stakeholder’ engagement have attempted to define it, general definitions are unavoidably broad and, therefore, vague.’ As one team which set about to make explicit what researchers seeking to engage in community engagement put it, “we…found an extreme lack of clarity and communication in the field about what to do and why” (Schairer et al. 2019). Community interest is not simply the aggregate of individual interests; it has “values goals and preferences” of its own, which are formed and expressed in culturally specific ways.

Social sciences, and the discipline of Anthropology, are gestured to as having some expertise on this matter.

The WHO itself acknowledges that “the mechanisms for accomplishing successful outreach and engagement are still not well understood.” What does appear clear to the authors of this guidance (primarily scientists involved in GEM research, including Tony), is that “this kind of activity should not be conceptualized solely in terms of public education, or of simply informing stakeholders and publics of things researchers know”. Working with this “deficit model” of engagement is believed to have been shown to lead directly to “fail” insofar as it increases opposition and mistrust (Kleinman, Eisenberg, and Good 1978; Wynne 1996; Hansen et al. 2003; Currey and Clark 2010). Instead, there has been a shift in thinking towards what are sometimes called “co-development” strategies. “Traditional knowledge deficit approaches are often based on the perception that publics fear and/or do not understand new biotechnologies, which tends to result in top-down activities designed to educate publics about the benefits of the technology in order to secure acceptance or consent for a field trial. Co-development emphasizes the importance of authentic partnerships with communities and publics, particularly where the product will be developed and implemented (“Gene Drives for Malaria Control and Elimination in Africa” 2018). This means that local “experts, communities, stakeholders and publics” are “engaged” and feel some “ownership” of new technologies. This kind of work is “collaborative”, focused on “listening and sharing” and does not limit itself to implementation but gestures toward the possibility that communities may play a role in the design of new technologies.

In this framework, knowledge is not produced by experts and then explained to the lay public; rather, knowledge is produced by both parties together, although the actual mechanisms by which this may happen in practice remain quite vague. Notably, a distinction is drawn between ordinary people and “organized opposition” by groups or people intransigently opposed to the research in question. In their analysis of “techniques of elicitation” like those recommended to GEM trial project managers, Javier Lezaun and Linda Soneryd point out a similar distinction, conceptualized by their interlocutors as the difference between the “general public” and “stakeholders” who “come to the consultation armed with predefined views” (Lezaun and Soneryd 2007). These people are not the primary audiences for what Nikolas Rose called “experts of community”, practitioners of consultation, engagement and, today increasingly, co-production (Rose 1999). Rather, it is members of the public who have un-formed opinions who are considered most representative of general “communities”, because their opinions can be seen to have been changed as the result of public consultation efforts, presumably an index of open-mindedness and lack of prejudice.

The Relationship Based Model

Ana Kormos was hired by the UCIMI to serve as their resident “expert of community” to use Rose’s term. She is their Engagement Program Manager, and has a Master’s of Public Health Degree from UC Davis. Before this, she was the Chief of Clinical Operations at OLE Health. Ana has been in charge of organizing the regulatory, communications, and community engagement “strategies” for the UCIMI, as they call them. She is the point person through whom communications with public health and government officials in Sao Tome and Principe and the Union of the Comoros run. She has arranged a financial arrangement with the UNDP to manage the program’s funds, help to recruit and manage local personnel, and assist with the procurement of equipment and supplies needed for fieldwork trips to those countries. Part of Ana’s job is the formulation of a theory of work with communities.

In 2020, she published an outline of the working model for the group’s engagement. “Application of the Relationship-based Model to Engagement for Field Trials of Genetically Engineered Mosquitoes” proposes a unified framework for regulatory and community engagement. The relationship-based model she writes, “provides a framework for investigators who wish to establish meaningful and effective dialogue, collaboration, and relationships of trust in the communities where their research is conducted.” It “is unique in that it provides a framework that supports decentralized decision-making and emphasizes the importance of stakeholder and community leadership in the development and implementation of community and regulatory engagement strategies, definitions, and decisions.” Consonant with the shift to “co-production” in theorizations about effective community education, Ana’s model is “collaborative” and “not top-down”; it is for “unique individuals who do not respond to a ‘one size fits all’ environment”. It “ensures that communities have a central role in directing program activities and strategies within existing systems” and “reduce[s] asymmetry among the program, external experts, academics, and the recipients of the technology.” Decision-making is “decentralized” but “stakeholders and individuals should be leading conversations that inform decisions”. Instead of a “position-based decision-making process” the relationship-based model offers “one that is knowledge-based…those who are in the best position to determine the adequacy and efficacy of the decision being made”. It is a strategy that, she warns funders, requires “the establishment of a shared understanding between research program leaders and funders that flexibility will be necessary regarding project goals and timelines”. To participate in the model, participants in the UCIMI need relationship-based communication skills, which “include active-listening, open-ended questions, understanding individual communication styles and roles in relationships, identifying assumptions and communication roadblocks, reflecting (paraphrasing and restating both feelings and words of the speaker), and developing shared language and definitions associated with the program. Group training and activities for the application of these skills help build capacity for good communication and relationship development.” Notably, communities supersede even nations themselves, as “external research teams, social scientists, regulators, and other experts may present a framework or a set of guidelines and suggested definitions for thinking about these questions, but at the national level, the relationship-based model places the stakeholders and community leaders in a position for determining the answers specific to their country” (Kormos, Ana, Gregory Lanzaro, Ethan Bier, George Dimopolous, John Marshall, Joao Pinto, Adionilde Aguiar dos Santos, Affane Bacar, Herodes Sousa Pontes Sacramento Rompao 2021)

Ana’s model is broadly in line with the approaches to community engagement recommended by the WHO, with Tony’s broad framework, and with the approaches of other GEM field trial projects [although in their regulatory strategy the UCIMI differs significantly from Target Malaria, as discussed in chapter X). Hers is unique, however, in its overarching emphasis on the creation of “relationships.” The main thesis of her position is that people involved in the UCIMI need to form relationships with communities, and that the existence of relationships, once formed, will necessarily contribute to a sense of agreement about the project. She states the primary goal of the Relationship Based Model as “facilitating the establishment of relationships of common interests” or, phrased elsewhere, “to establish shared, as opposed to conflicting interests.” In this chapter, I ask: how did it come to be obvious that forming relationships leads to the formation of shared, as opposed to conflicting, interests? How are relationships assumed to change interests? What does "relationship" mean here, and how do organizations form them with communities?

On Balint Groups

Ana writes that her Relationship Based Model evolved from an idea originally formulated in 1969. In the article Ana cites as the inception of the model, Enid Balint describes the work of her husband, psychoanalyst Michael Balint, with primary care physicians in 1950's London. "The possibilities of patient-centered medicine" puts forward Michael's theory of "doctor as drug", expounded at further length in his 1957 book *The Doctor, His Patient, and the Illness* which is still cited regularly in medical practice education (M. Balint 1957; E. Balint 1969). Michael distinguishes between two classes of medicine: in illness-oriented medicine the doctor diagnoses a problem and treats that problem as the object of intervention. In the second, which he called "patient-centered medicine", the doctor focuses on an individual as a "unique human-being" to form an "overall diagnosis" not limited to the identification of a nameable pathology. I diagnosable pathology may be identified, but it is to be analyzed in terms of the whole that is the social person. Balint's text continues to be cited in discussions among medical practitioners about how to deal with "difficult" patients, often believed to be suffering from psychosomatic illnesses or emotional disorders.

Michael Balint was born Mihaly Bergmann in 1896 Hungary. Along with his first wife, Alice Balint, a prolific psychoanalyst in her own right who also trained and wrote extensively in Anthropology, he did several years of psychotherapy and training with Sandor Ferenczi, a close associate of Sigmund Freud's. Both Jewish, the two moved to Berlin in the early 1920s fleeing growing anti-Semitism in Hungary. Together, they published on the relationship between mother and child, contributing to the literature on object-relations within psychoanalysis at this time. Alice's writings on motherhood influenced Jacques Lacan (A. Balint 1949; Borgos 2019; Sklar 2012). They fled to Manchester in 1939 on the eve of WWII where Ana died abruptly of a brain aneurysm in 1941. Michael's parents both committed suicide in 1945 after the Nazis occupied Budapest. In 1949 Michael moved to London and began working for the Tavistock clinic. He married Enid in 1953.

The Tavistock Clinic was founded in the 1920s to treat shell shock victims and civilians suffering from "nervous disorders". By midcentury, the clinic was pioneering work in systems theory approaches to therapy, drawing on the ideas of Gregory Bateson and others interested in applying communication theory and cybernetics to the study of "human systems". The family, for example, can be seen as a system from this viewpoint, analogous to an ecosystem, with inputs and outputs and potentially pathological patterns of interaction (Bateson, Jackson, and Haley 1962).

Michael's work was with his eponymously titled "Balint groups" in which he and other Enid met with groups of general practitioners (GPs) weekly for up to three years to discuss and follow up on the cases they had seen in their normal course of practice. The groups would talk extensively about the feelings aroused in the GPs by the various encounters they had with patients, the way their relationships changed, and the different responses elicited from patients by alternative behaviors from their GP. Michael proposed from the notes generated of these meetings several theories about how patients and doctors come to agreements about their illnesses, and how the nature of the relationship between doctor and patient affects the trajectory of care. This method was explicitly ethnographic and sought to include in the analysis of patients and doctors not only their behaviors but their thoughts and feelings in response to one another. Michael and Enid proposed that psychoanalysts taking part in such work must regard themselves not as educators, dogmatically instructing GPs on psychoanalytic techniques, but researchers exploring with them the problems at hand. The groups met throughout the 1950s and 1960s, and inspired other similar groups, but the practice largely died out in the 1980s (M. Balint 1966; Hull 1996; E. Balint 1974).

Participating GPs learned what Michael and Enid called "six-minute psychotherapy." They believed that this training could help doctors to establish one-on-one relationships with their patients as "whole social persons." By this they intended to include the individual's relationships with others in his or her life in the doctor's evaluation. The work, by definition, took place in a system of two: "The events that I wanted to get hold of could be observed only by the doctor himself; the presence of a third person however tactful and objective would inevitably destroy the ease and intimacy of the atmosphere. Such a third person would see only an imitation, perhaps a very good imitation, but never the real thing" (M. Balint 1957). Such relationships could allow doctors to practice "patient-centered medicine" by holistically integrating medical diagnoses into the context of individual lives. A key part of this process was learning to be used by one's patients. The doctor administers himself as a drug, learning in "the subtle ways in which their patients change in their use of them… to be 'used' in different or more varied ways." The goal was not to arrive at a diagnosis like "castrating mother" or "oral dependence" which would function much like "broken leg" in the illness-centered model of medicine. Rather, it was about close and receptive attention to the needs and emotional dynamics of any one encounter— seeing them shift and learning to use this capacity for shifting to arrive at a rapport through which patients and their doctors felt themselves to be in agreement. Enid acknowledges in her reflections on the Balint groups the feelings of fear and distaste this aroused in some GPs: “In my experience what has prevented them from doing this in the past is not because it involved them in a new way of thinking but because they feared that if they let the patient loose, so to speak, they would be overwhelmed; patients would get too close to them and would become unbearably dependent and demanding” (E. Balint 1969).

The Balints were notably un-theoretical in their approach to psychoanalysis, preferring to “think in cases” (Bar-Haim 2020) which illustrated, but did not prove, insights they found useful. One such case study is of a woman named Grace. In her 50s, she worked as a mechanic, was married, had no children, had a history of irregular menstruation and complained of headaches. Her doctor, working to receptively draw her out (as he presumed her physical suffering was, in some way, psychosomatic) learned that her earliest memory was of seeing her mother in a nice dress. Her father ripped the dress off of her mother’s body and her mother later slapped her, telling it was her fault. Her mother later gave birth to a baby brother, but then left the family. Grace was put into an orphanage. No one came to visit her until she was 14 years old and was taken by a family to work as a domestic laborer. Grace was deemed by her doctor to be pathologically “unfeminine” though “not masculine”. Pathologies included her desire to work, even though her husband encouraged her to abandon it, her failure to have children, and her relief when her husband stopped demanding sex. The doctors debated whether it was worth it to spend time on such a person (“I find that when there is a history of psychosomatic disturbances like dysmenorrhea and so on, by the time that they are 50 or 60 I wonder if there is any point in going through it”), but later praised the doctor for leading Grace, through receptive listening and subtle guidance, to realize that her husband did love her because he “made her tea and cut her sandwich". Happily, she came to regret not having children and to rightfully view her career as a poor substitute for love from a man (E. Balint 1969).

Control Then

Enid's account of her and Michael's work, posing it for the first time as "patient-based care" is an extraordinary document. How did this text evolve into Ana's, and why? Ana's text is about how an institution like the UCIMI ought to manage its interactions with people, organized by them into "communities" to minimize the risks of opposition to what they want to do with, in, and around their places of life. Enid's is about how primary care physicians can make their interpersonal relationships with patients into objects amenable to prescription. Neither text uses the language of control, but both offer advice to institutional authorities about how to preempt or manage conflict, dissent, or potentially "difficult" people. Both are part of a literature on regulatory practices and techniques. And both focus on the key term "relationship" as a way of conducting this kind of work not through the subjugation of others desires or the crushing of dissent, but through the promotion of types of subjectivity, forms of desire, and positive self-interest which bring others' behaviors into alignment with the goals of doctors or researchers. It is "relationships" which are posed in each as the interface through which such interventions become possible. Relationships are here figured as objects of work whose manipulation can change the opinions, interests, and actions of others. For each, "relationship" is the key tool by which such change is accomplished not by the doctor or the scientist, but by the patient or the community.

The advice for doing so, however, has changed in the trajectory from Enid to Ana. What was the Tavistock Clinic when Michael and Enid were doing their work? How did they conceptualize their patients as "whole persons" and what was the framework for intervention? Peter Miller and Nikolas Rose call the Tavistock a "laboratory of governmentality", pointing out how their research on group processes served as a test bed for innovating new forms of social control (Peter Miller 2008). "The Tavistock has been a key element, model and example in the development of an expertise of subjective, interpersonal and organizational life and its wide extension in modern society." (Miller and Rose 1988) The "regulatory expertise of subjective life" they offered was not the implementation of a coherent top-down program of social control coming from the state or the institution itself but "were invented in relation to a range of distinct political rationales and changing sets of political, social and moral concerns." During the time of the Balints' work with GPs in the 1950s and 1960s, the Tavistock Clinic was being incorporated into the National Health Service, while the Tavistock Institute for Human Relations was spun off as a separate entity that carried out projects with corporations like Unilever, Shell, Bayer, and the National Coal Board focusing on management techniques, women in the workplace, and workers' reactions to new technologies. From this research emerged a framework that came to be called the sociotechnical approach, which called for an attention to people and objects to be studied together as part of complex organizational dynamics in the workplace. The group also did extensive work with public health organizations and hospitals from a management perspective, providing advice about the administration of cleaning staff, the management of wards, and the physical organizations of hospital spaces. The Clinic during this time was led by Wilfred Bion, a psychoanalyst who had contributed significantly to the British war effort, using his studies of institutional group dynamics to contribute to a new method of officer selection using his "leaderless group" idea, as well as to research into strategies for enemy interrogation techniques, propaganda, training and morale. After the war he and other members of the clinic played a role in the "de-Nazification" of Germany by helping to select individuals to be promoted in government and administration and fostering democratic processes. In the 1950s, the Clinic provided an active link between psychoanalytic theories and models of social organization, beginning to work especially on attachment theory, marital relations, and what were called family systems ("A Brief History of the Tavistock Clinic" 2021).

The Clinic at this time was invested in a systems theory approach. This applied to families, corporations, hospitals, and armies alike. Each could be viewed as a set of actors, human and nonhuman, interacting in complex ways. It was the complexity of these interactions that was especially appealing to practitioners interested in how actors within such systems might be controlled. This way of thinking about groups of people and things as interacting networks grew out of ways of thinking about organisms holistically. Ludwig von Bertalanffy, a theoretical biologist, is credited with founding general systems theory. Rather than understanding a system by breaking it into its constituent parts, Bertalanffy offered theoretical modelling which took the system as a whole as its object and looked at its relationships with other systems and its mechanisms of growth and change. Talcott Parsons' structural functionalism followed in this school, analogizing social systems to biological or mechanical ones. Field theory, which originated in physics, was applied most notably by the Gestalt psychologist Kurt Lewin to the study of social systems in the 1940's, analogizing the interrelations of molecules to interdependencies in social groups (Lewin 1939). The emergence of cybernetics in the midcentury period also addresses the regulation of systems. Gregory Bateson's collaborative work on schizophrenia is an example of dominant family systems theory approaches to family dynamics which borrowed heavily from cybernetics. While a coherent history of systems theory approaches during this time is beyond the scope of this chapter, holding together these various practices is a technique of looking at individuals not in terms of fixed inborne qualities, but in terms of their location in networks of relationships. It is through these relations that various actions, responses, qualities, or capacities actualize, and it is through labor on and attention to these patterned relationships that effects are believed to be possible to achieve both in the behaviors and experiences of individuals and in the patterns of their interactions. Balint's work in the Tavistock Clinic during this time was in a context of widespread attention to the ways these theories or insights might be applied to techniques of regulation and management. The Tavistock's work from hospitals to coalmines broadly envisioned an increasingly "democratic" society, in which conflicts could be resolved through "working through" negative interpersonal issues which contribute, at the large scale, to conflicts. A systems theory approach promised the potential of techniques towards the maintenance of both harmony and freedom without repressive coercion. E J Miller, director of the Tavistock Institute's Group Relations Programme and J E Neumann, core faculty in its Advanced Organizational Consulting Programme, both wrote on the applications of psychology to the study of corporate, political, and social organization through a systems framework that envisioned groups as dynamic interrelated systems, akin to natural and mechanical processes. Understanding the laws of such systems, they believed, could allow managers of all kinds to design the functions of such systems like social mechanics, opening up not only new vistas for control over people, but new meanings of what that control entailed (Miller, EJ; Rice 1967). If human organizations were increasingly figured as complex recursive systems, organizations like the Tavistock proposed to offer new kinds of techniques to the would-be helmsman (Fraher 2004). The Tavistock as a "laboratory of governmentality" also brought with it ideas about what its object of study was, and what tools were appropriate to manipulate it (Peter Miller 2008). We can see here how the meaning of control the Balints picked up, used and modified in their work with general practitioners both indexed and moved forward ideas that were emerging as basic assumptions about human relationships. As we will see in the following section, however, their terminology was to have a life beyond and after this viewpoint on the social. As intellectual contexts changed, both through time and across disciplines, their suggestion for techniques of control born out of this image of society were repurposed to new kinds of aims and to new kinds of assumptions about relationality.

Backwards Citation

After Enid's 1969 article cited by Ana as the beginning of what would later become her Relationship Based Model, little evolution occurred for the next thirty years. In the 1970s two articles were published on the Balints' work, one a portrait of Michael written by Enid. In the 1980s the article is not cited at all. In the early 1990s one article analyzed Enid's practices of "imaginative perception" and "open communication" within the history of psychoanalysis.

Things shift in the latter half of the decade, as articles like "Understanding Patient: Implicit personality theory and the General Practitioner" expressed as taken-for-granted that GPs are "expected to advance beyond biomedical illness categorization (the disease centred method) and develop an understanding of the 'whole person' in order to inform clinical decision making." We can see how Enid's formulation of Michael's "patient-centered medicine" had already entered common parlance, at least among British GP's. The article complains, however, that doctors have no clear model of individual behavior in which to understand whole persons, and suggests that some more robust theory, and training in it, would be necessary for doctors to practice this patient-based care (Bower 1998). Even in this shift to language of training and theory, however, we see some divergence from Michael's work. Strictly un-theoretical, he proposed a notion of relationships between doctors and patients that was deeply resistant to proscribed and standardized protocols. In the year 2000 an article "Patient-centeredness: a conceptual framework and review of the empirical literature" addressed just this problem in a literature review of the concept of "patient centeredness". Their articulation of the approach offers a clear and interesting point of comparison with the Balint's formulation. "Five conceptual dimensions are identified: biophyschosocial perspective; 'patient-as-person'; sharing power and responsibility, therapeutic alliance; and 'doctor as person'." Here we can see that relations are foregrounded but with a different meaning (Mead, N Bower 2000).

For the Balints, the relationship in question was between the GP and the patient; in engaging personally with an individual, the GPs in the Balint groups were not in any straightforward way meeting their patients' perceived needs to feel that they were in a relationship. Rather, the GP was learning to use the relationship, which existed no matter what their manner of engagement was, and was not necessarily of a positive valence, to adjust the patient's orientation towards other factors, systems, or people in his or her life (for example, coming to agreement with the GP about the nature of her illness, coming to believe in a husband's love, learning to see her mother as a toxic force in her life, acquiescing to normative gender roles, or being emotionally transformed by experiencing care). The relationship was not only, or even primarily, experienced by the patient, but affected doctors as well. As Enid wrote: "Our doctors told us that although they had not altered their way of treating all their patients their whole work had nevertheless changed since they joined our seminars. They could not tell us how, but they felt themselves to be different kinds of doctors, even different kinds of people since they started work with us. They insisted, nevertheless, that their ordinary 'surgeries' i.e., the time they spent in their consulting rooms, were much as they had been before they started; we found this puzzling.". Doctors and patients were both changed, not by an act of will, but by assuming new structural positions in systems of relational dynamics. Enid gestures towards the fear and discomfort this sometimes caused doctors: "they feared that if they let the patient loose, so to speak, they would be overwhelmed; patients would get too close to them and would become unbearably dependent and demanding" (E. Balint 1969). The kind of receptive listening the Balint's prescribed was a risky business: by opening the door to relation, the doctor loses some degree of control over how he, too, may be changed. Enid and Michael were interested in the system of two formed by doctor and patient, and how that system interacted with other systems like the family.

By contrast, in the 2000 article, the *feeling* of being in a relationship is one among several needs of a patient. It sits alongside physical needs and it is contrasted with a baseline of experiencing no relationship. A 2003 book whose title riffs on Balint's book *The Doctor, His Patient, and the Illness* identifies this feeling of connectedness explicitly as a need which must be met by medical service providers. Patients report more satisfaction with their care if they feel they have a "stronger" relationship with their doctor, and if they feel they play some role in decision-making processes of care. Doctors are here encouraged to provide these emotional and affective services alongside the provision of medical care to increase overall satisfaction ratings among patients (F. Smith 2003). Other publications from the early 2000s reflect this consensus: "The concept of patient-centeredness is complex, but is generally seen as an approach that emphasizes, on the part of the health professional, attention to patients' psychosocial (as well as physical) needs, the use of psychotherapeutic behaviours to convey a sense of partnership and positive regard, and active facilitation of patients' involvement in decision-making about their care" (Mead, Bower, and Hann 2002). It is the affective sense of partnership and positive regard that is aimed at here. We thus see relationship retained as an important concept, but without the structuring framework of a systems theory approach. Before, relationships were links between actors within systems; here, relationships are affective, positive experiences to be provided to patients.

Just slightly later in the first decade of the 2000's we see a shift from "patient-centeredness" to "relationship centered care." The shift is subtle, and for the most part, the language and framework of "patient-centeredness" is retained, but with more emphasis on how relationships serve to create positive affect. In a 2006 article "Relationship Centered Care: a constructive reframing" we see relationships defined not only as between two people but more broadly "with self and others". One can be in a relationship with another person, but one can also be in relationship with society in a more general sense, or internally with oneself. We see, moreover, a focus on what is called the "quality" of relationships; in high quality relationships "all participants appreciate the importance of their relationships with one another." Presumed here is the idea that relationships have a positive valence, and that strengthening them strengthens this positivity. Relationships are a good thing; more of them is better. The formation and maintenance of "genuine relationships" is, by definition, "morally valuable". We see a transition from the doctor learning to administer himself, to the doctor learning to administer the feeling of "being known as a person" (Beach 2006).

Ana cites the shift in relationship-based care to a focus not only on relationships between individuals but between communities to the book *Relationship-based Care: A model for Transforming Practice* which "ushers in a new consciousness of the deep dimensions of health and healing. At the core of this new consciousness are relationships and caring." Published by a consultancy company, the book promises more efficient and cost-effective healthcare for hospital administrators who adopt this new consciousness. Among its basic assumptions, the book tells us that "feeling connected to one another creates harmony and healing; feeling isolated destroys spirit" and "transformational change happens one relationship at a time". We see caring and positive affect as strongly associated with, even definitional to, relationships as they are formulated here. We are also operating in a scarcity situation: more relationships are needed. Without their cultivation, they may not exist. This understanding of relationships as by definition caring and positive is posed as biologically natural in the epigraph to the book's introduction which cites Dean Ormish, whose claim to fame is the creation of a diet plan. Ormish tells us sagely that "We are creatures of community. Those individuals, societies and cultures who learned to take care of each other, to love each other, and to nurture relationships with each other during the past several hundred thousand years were more likely to survive than those who did not" (Koloroutis 2004).

To be in relation is to be caring, to love, and to nurture. The only negative kind of relationship is the lack of relationship, which can always be remedied by the establishment of one, preferably one of quality. This formulation of relationship-centered care remains broadly dominant, and directly shapes Ana's own Relationship Based Model. It also presents us with a very interesting theory of relationships as a uniformly morally and affectively positive force. Relationships bring people together and make them feel more satisfied with what occurred between them. It is not that by talking it out people discover that their interests are more aligned than they thought, it is t*hat the act of relating aligns interests* by creating positive affect. Relationships, by definition, lead to convergence of purpose. Creating more and better relationships, then, can align people's interests without any material change occurring. There need not be bargaining when there is good feeling. Relationships, similarity, and agreement all effortlessly cluster together.

This is not, of course, the only way of being in and thinking relationships. In her opus on this quintessential anthropological topic, Marilyn Strathern specifically critiques this assumption that relations imply underlying similarity. "[R]elations so conceived fail to challenge a prevailing orthodoxy in political action, namely that it proceed through demonstrating similarity or convergence of purpose ("common grounds", "joint interests") when parties reach decisions together." (12) Strathern gives us alternate examples of people conceived of being related through (rather than in spite of) differences and traces a historical trajectory whereby the assumption that relations reflect or create similarity and convergence came to dominate broadly Western thinking (Strathern 2020). It is the very obviousness of this pairing which emerges clearly and, I would argue, is advanced, by the Relationship Based Model as Ana articulated it. Ana was not creating this notion of relationships out of whole cloth; as we can see, she drew on a dominant paradigm common in her background area of healthcare provision and she moved this thinking forward a small and reasonable step while translating it into a new field. Translated into the arena of GEM field testing, the Relationship Based Model makes perfect sense as an adaptation of the relationship-centered care paradigm. It is consistent with demands among regulators, scientists, and others writing in the ethics and policy space about this topic to "co-develop" trial projects with communities who are involved. It offers a language for talking about this co-development that borrows from the archetype of the doctor-patient relationship as articulated by this paradigm and adapts it to the institution-community relationship she was being asked to articulate. It successfully avoids the imposition of dominance or authority over such communities by scientists. Communities can be said to be leading the process if enough emphasis is placed on cultivating relationships with them. Who makes up these communities, and how they work, are questions Ana still struggles with. She does not, however, see these difficulties in locating communities as threatening the model which centers them. This model is also useful insofar as it offers a way to balance power dynamics between people and research institutions which have the capacity to become unbalanced without an adjustment of material conditions. Historical examples of people being harmed by these power dynamics like, for example, malaria research which has used unethical human testing (Comfort 2009), can be countered with documents such as Ana's which promise to balance them.

Her translation of relationship-based care to the Relationship Based Model appears, within this intellectual trajectory, so reasonable as to hardly register as an innovation. Much of the language she has developed to speak about the Relationship Based Model and to represent the UCIMI to communities and other publics is so common as to feel anodyne. I would argue, however, that the very difficulty of attending to language like this warrants increased scrutiny. What are the assumptions put forward by statements which feel so commonplace?

Her model presumes that relationships reflect and create similarity and convergence. Agreement, then, does not need to be achieved on the level of structural or material bargaining, but the affective level of care and nurturing. Failures of convergence can thus be attributed to a lack of relationships and remedied by the administration of a feeling of being listened to and related with. The goals of two parties to an interaction (for example, the UCIMI and people living in a village in Sao Tome) can thus converge into partnership through affective transformations alone. These affective transformations, the creation of the feeling of being related to, are thus presumed to lead to transformations in practice, or cooperation around a common goal. The institution must listen to create a feeling among people in communities of being seen; communities will acquiesce if this feeling is successfully evoked. Of course, a willingness to be in relation on these terms is a pre-requisite for such work. Experts of community like Ana are well aware of examples in which projects are stymied by such refusals. We saw earlier the distinction drawn by the WHO in its Guidance Framework on the Testing of Genetically Modified Mosquitoes between "the general public" and "organized opposition" which does not converge through dialogue. Lezaun and Soneryd name this distinction using the ancient Greek meaning of "idiot", "private individuals who are exclusively dedicated to the privacy of one's own" (Lezaun and Soneryd 2007). Idiots, in their formulation, are "mobile"—their opinions are changed through dialogue. They are not committed to a social vision which might intractably conflict with the aims of an institution with which they are in dialogue. They are, according to the WHO framework, members of the public. They are willing to be changed through relationships. So-called "organized opposition" are recalcitrant people with preconceived ideas about a project, committed to something other than the idiot's privacy of one's own. Such individuals and groups hover spectrally around conversations about GEM—people like Helen Wallace and her organization GeneWatch ("GeneWatch UK - About GeneWatch" n.d.). Researchers do not believe they should enter into dialogue with such individuals; they rather see them as competitors for the opinions of members of the public. As Tony put it in one meeting "Those anti-GMO people-- they're worse than we are! Trying to influence local people."

Ana's Relationship Based Model may have emerged out of Enid's formulation of her and Michael's work, but it did so circuitously. Along the way, the conceptualization of relationships changed, as did prevailing notions about how they could be used in social control. For Michael Balint and his contemporaries at the Tavistock, patients and doctors were both located in a structuralist web of relationships. Those relationships existed no matter what, but if doctors listened to information about patients' lives and attended to the patient's location in her own web of relationships he could both gain a deeper understanding of what ailed her physically and mentally as well as change his own connection to her in a way which might make her more amenable in the bargaining process they engaged in together around medical care. This structuralist framework geared around bargaining fell away as the language of patient-centered care shifted in the early twenty-first century to a focus on affect and good will. Along the way, relationships shifted from things which always exist but may have any strength or emotional valence to things which do not exist at all without affective cultivation and strengthening which are definitionally good and transform a situation not in the context of bargaining but instead of it. Along with this shift came a new notion of how patients, and later analogously communities, are interacting with doctors and researchers. We see an assumption that members of the public act on private interests, motivated by positive feeling. Social control is to look and feel like warmth and care. It is accomplished by relationships with are more than usually "human." Individuals who are not motivated by such human relationships, whose interests are inflexibly linked with broader social or political agendas, are excluded from dialogue as hopeless. It is rather the idiots, in Lezaun and Soneryd's parlance, to whom the Relationship Based Model is aimed.

What does this *Really* Mean?

The kind of language Ana uses in her description of the Relationship Based Model feels numbingly familiar if one is frequently immersed in literature on ethics, community engagement and genetically modified mosquitoes. Experts of community like her are employed by many institutions, whether governments, universities, or NGO's, which feel that they must give ordinary people a voice but are unclear on how to do so. It is a field with its own jargon and techniques that, while evolving, are relatively established. But most of the people involved in the UCIMI do not have contact with this field. They are primarily laboratory scientists who face these requirements as hurdles to be overcome on the way to "real" work and action. Ana may speak as the voice of the UCIMI in the world of community engagement, but moving into the laboratory she becomes the foreigner speaking an unfamiliar language. The contact, then, is not only between the institution and communities in the field, but between the institution and itself.

On September 21, 2020, Ana was presenting at the lab's weekly meeting. Everyone who worked at the lab was gathered in a conference room where she intended to update them on the new website she played a part in designing for the UCIMI. As they all got started, she confirmed that, because of the coronavirus pandemic, the team would not be going to either the Union of the Comoros or to Sao Tome and Principe that year. She had hopes, however, that they could schedule monthly virtual meetings to maintain their relationship with the Ministry and Health Delegation in Sao Tome and Principe (given a history of problems communicating with their counterparts in The Union of the Comoros, the UCIMI was investing and hoping for less). That Wednesday, in fact, she was scheduled to zoom with the Health Minister of Sao Tome and Principe to "say hi…refresh, reconnect, and plan another zoom meeting." Then the lab got down to business, going page by page through the new website and soliciting input and corrections from lab members who had been asked to review the text of the site beforehand. When Ana arrived at the website page presenting the Relationship Based Approach, Travis chimed in. Travis was a long-time senior researcher at the lab specializing in population genetics and bioinformatics. He is the husband of another former lab member, Yoosook Lee; they both later moved to take jobs at the University of Florida, which is also where Greg got his PhD. The team read the short blurb Ana had prepared summarizing the Relationship Based Model "and what that means to us."

"The relationship-based approach sounds nice" Travis started "it's a little… I don't know the target audience for this, but it would be nice to say our goal is to have communities, or local countries… our goal is to enable them, not to come in and fix the problem. Or give them the tools to fix the problem." Ana practiced the active listening approach as outlined by the Relationship Based Model. "So what you are saying, Travis" she countered "is to incorporate that information in the title. To change that. Is that what you are saying?" "No", he replied. "I mean, like, maybe the first bullet point a short sentence saying our goal is to give impacted countries or whatever the tools to deal with malaria themselves." "Isn't that kind of what it says?" Christine, the administrative assistant joined in on Ana's behalf. "It says places communities and stakeholders at the center of decision-making processes." "That's what it says in executive speak." Travis answered "I don't know who the audience really is. I guess it's probably funders." Greg attempted to redirect the conversation, but Ana refused. "I want to take note of what Travis is saying. Basically, the target product will be a tool used and driven by people in malaria endemic countries. That is what you are saying is not clearly stated here." Travis flailed. "The contrast, like Oxitec or many things, they're making a product which is basically a mosquito that acts as a pesticide. They come in, or Gates they come in and set up health clinics. We're kind of more along the lines of setting up the health clinics sort of a thing. I should just shut up because I have no idea how to describe this sort of thing." Greg intervened authoritatively. "Our goal is to bring facilities and resource material and human resources so they could make the mosquito themselves. And that would be so it would be made by them and it would be about whether to use them or not." Anton added mildly: "It's like technology transfer, right?" Ana seemed irritable. "Well, it's stated right here above this. It states that our aim is to collaborate with malaria endemic countries to develop new tools. That's an important point we wanted to drive home, and that's part of what Travis was saying if I understood him." Anton bridged the situation. "I mean, it's like, if you consider Oxitec, they are into making the tool. The country is paying Oxitec. Whereas we are not selling a product the country has to pay for. We are selling a tool the country can use or not." Greg said, "that's what it means to say we're not for profit." "I think Travis is trying to make that connection more obvious", Anton said. "It seems like it's there to me" Greg replied, and Christine added "I've read the entire website and it's clear if you read the whole thing. It's a fair point that each time you say it to say it really well because nobody is going to read the entire website, so it's good to make sure to say it completely every time." Greg suggested that the website simply display Ana's paper on the Relationship Based Model: "It's nice because it's colorful and relatively simple and it explains the whole thing."

"I think Greg hit the point I was saying" Travis continued. "Our goal is to basically engineer local mosquitoes. Or ideally have local labs engineer local mosquitoes. Probably that just is not appropriate in the level you're talking here. So you can ignore me." "I mean, that is our goal" Greg said "But all of us, I mean all of us on this side, Tony, Ethan, myself, have some concern about whether we're going to be able to pull that off. But that's what we'd like to do.

Get people there trained, get the labs equipped so they can make these mosquitoes themselves so they'd be making that decision. Yes, we want our scientists to make these things and they will be releasing them so they will make them and load them and take them to a site and no gringo hands are going to be on this thing from beginning to end." Travis, encouraged, continued "The wording technology transfer, I think you said that, that might want to go in there. Because the technology could be the mosquitoes." "Yeah, but I think that's sort of stating the obvious" Greg said, turning away. Marc joined him [discuss Marc and Travis "asshole" fight?]: "it is stated explicitly in the blue box right above that one. If the country decides appropriate they will develop the mosquito and the implementation strategy."

The conversation veered back to a discussion of more technical points about the website before they reached the Community Engagement page, which showed a picture of Greg and Anton examining some larvae in the field. "I'm thinking broadly about the optics" Anton, who is white and South African, offered, "if you look at the UCIMI team it's all people from the North and no people from the countries we are actually working in. So it doesn't give the appearance that people we are working with in Sao Tome are necessarily part of the team. It's just an optics thing; someone is looking for the first time and they go: where's the black people?" "Maybe we can say "North American team" Marc offered. Anton continued "I know you have the people represented, but it's a little deeper into the website. Not even for the optics, but for the people in Africa to feel that they are almost equal partners. They are almost just as important to this thing as we are." Ana bristled "Well they are. If not more important. I like Marc's suggestion for North American language. Just to speak to that, this site has been shared already with the Comoros and Sao Tome and Principe people to get photo permissions and put their info in here. There are a lot of additional photos. The entire PNLP team in Sao Tome and Principe is going to be pictured on the Sao Tome and Principe page, we're just making sure their names and titles are correct. They had great things to say. They were really excited and they liked the page, but at the same time I hear what you're saying. Everyone has brought this up. Everyone has brought up that the UCIMI is an American group of white people." "The PI is not" Greg said abruptly. "The overall head of the whole thing is not." Anthony James, while he appears white, has African American ancestry and identifies as black. Ana said gently "but if we're just talking about appearances. But we need to represent ourselves accurately and be honest about who we are and where we're from and how our work is organized, who is doing what. For me, that's more important than trying to provide a representation of something that's somehow covering all of our bases in terms of political correctness."

Travis suggested, following Marc in a conciliatory gesture, that the white-presenting people might be identified as the "UC team." George Dimopolous would have to be an honorary UC person, which they discussed, but that would permit them to have a tripartite structure of UC team, Sao Tome and Principe team, and Comoros team. Christine suggested a new page with these site teams, but Ana said they wanted to "avoid at all costs another page financially." Greg suggested a tab that says "African sites", and Christine suggested it could be incorporated into an existing page, and then Marc suggested that there could be a division on the site between UCIMI USA and UCIMI Africa. "Then they're getting equal billing and if you go to the UCIMI Africa link it would look more like the USA link featuring people. Maybe break our parts about site selection into different parts of the website. If you wanted equal billing, you'd make North America and Africa parts similar in terms of teams." "The issue with that is that we'd have to rewrite all the legal documents" Greg said. "And the teams are more fleshed out here, they're still developing there" Marc conceded. Greg repeated that workers in The Union of the Comoros and Sao Tome and Principe could not be considered part of the UCIMI without a lot of legal paperwork, and then brushed the topic aside by suggesting that pages with images of them simply open first on the website.

Conclusion

The conversation relayed above was not at all unusual, but I think it is worth relaying at length. Travis' beginning comment, which he began by saying "it's a little…" seemed to grope in a way that felt familiar to me for a clearer articulation of what we are *really* talking about. He called her language "executive speak." This is a form of language familiar to us all, anodyne and clean-washed, hitting our ears more like a familiar earworm than like new information. How does language do this trick, speak without saying much and keeping itself safe? And why? The conversation above is interesting for a number of reasons. For one, we can see Travis trying to subject this genre of speech to the practices of work he is most used to. In the discussion section of one of his papers, for example, he would try to state as simply and as clearly as possible what he sees his work demonstrating, and why it is relevant. To Travis, Ana's language felt evasive, and he was trying to be helpful in translating that language into what he thought were more straightforward statements. We can see, however, from Ana's icy responses and from the general lack of interest in this direction from the rest of the team the discrepancy between Ana's aims and Travis'. I want to suggest here that what feels like "executive speak" in Ana's work is not a fault but a success. Analyzing her Relationship Based Model is difficult because the language slides through one, rendered hard to see and hear by its very familiarity. Ana writes in the style, using the same language and phrases, of other experts of community. Within this expertise, community is rendered in a very specific way that no one is likely to confuse with, for example, the experience of existing within temporally complex interweavings of warm, hurtful, or ambivalent relationships with others.

The word "community" has become increasingly popular in some areas of cultural discourse today. We speak much more fluidly about being "part of that community", and easily absorb critiques that people who are "not part of that community" may not have standing for certain kinds of roles or statements. Why has this word increased so much now, and what is it doing? If we look to Ana's work, we can see how "communities" can be invoked as whole, coherent-enough, objects. The term avoids the specificity of words like neighborhood, ethnic group, family, or even nation. The nature of the relationships which compose it can be left unspecified. In community engagement spaces, the word is attractive for reasons I would argue are likely similar to the reasons it has become so ubiquitous in areas like philanthropy, charity, or international development. In all of these fields, a structure of accountability, scaffolded by money, points straight up. Philanthropic organizations are accountable to their funders; the loss of their support is an existential threat to the work and the livelihoods of the people doing it. It has been pointed out that this accountability structure is part of the reason that such organizations often fail the people they are meant to help—the faces often featured on website homepages or brochures. I suggest that the language of communities functions to redirect attention from this structural issue. When organizations with philanthropic or charitable aims talk about "listening to communities" they can create a narrative of a flow of power and obligation that is reversed. To speak, at great length, about communities "in charge" or "in the driving seat" is often a good way to avoid pointing out that an organization and its employees are not actually obligated to those people. Their displeasure, or refusal, are not existential threats. That happens on the other side of the funding stream.

This may be an overly critical read of the function of "communities" within charity-speak, but it is a discourse directly related to the language game Ana is involved in here. Ana's goal is partly to acquire consent from members of the government in Sao Tome and Principe and the Union of the Comoros to conduct research and experiments in their countries. Functionally, however, these decisions are often made in direct consultations between UCIMI leadership and their contacts in country before community engagement enters the picture. Ana's real work is more focused on operating the experiments as they occur in such a way that they meet ethical guidelines, do not generate negative press, and are seen by scientists and regulators invested in this space as having occurred in a way that was fair to people living in the experimental sites. To achieve this, Ana sees it necessary to build good will. In this chapter, I have been concerned especially with the link between good will and relationships presumed within the Relationship Based Model. Relationships are seen to lead inevitably to good will. The more contact people or groups have, the more they talk, the more they will understand each other, the more their interests will converge, and the more similar they will become. This is an interesting and specific view of human interactions, one that is significantly different from the systems-oriented framework within with the patient-centered medicine concept emerged. We can see how broad consensus over how people work is revealed by looking at what people consider to be the obvious ways to go about managing or regulating them. The proposal to form relationships with communities is a hard one to parse, on the face of it, even if the UCIMI had a clear idea of what the communities they want to be in relationship with are and who is in them, which they do not. How does one have a relationship with a community? One certainly cannot, if we take Michael

Balint's notion of relationship as a starting point. That relationship had to happen between two people. It was of mixed, ambivalent moral and affective valence. It could be threatening, or unpleasant, and retained within itself the possibility of altering either or both parties beyond their expectations or consent. A relationship with a community, even a very vaguely defined one, is, however, possible with Ana's reformulated version of the Balints' idea. Because relationships became over the intervening fifty years not ambivalent interpersonal links but positive experiences they can hypothetically be generated in a much more diffuse and general way. Their generation is, also in this context, separable from their invocation. To speak of building relationships with communities can have its own effectiveness outside and apart from anything that happens with people who might be defined as part of those communities. We can see this concern for appearances in the conversation about race that came up in the lab discussion about the website.

In what way is Ana's Relationship Based Model related to the relational modelling of her colleagues in population genetics? After all, the formal representation of relationships is the core work of the VGL and considered the complex but necessary prerequisite to the form of intervention they imagine through gene drives. From one angle, we could reach back to the work of the Balints, taking place as it did in an atmosphere of general excitement around systems theory and cybernetics. They were part of a diffuse cluster of people thinking about how to use these ideas to regulate and manage systems of people by formalizing their relationships and envisioning their interrelation in holistic systems. We have seen how the patient-centered model diverged from this point of origin as it was picked up and changed, especially around the turn of this century. But we could return to that moment to see the origins of some techniques and patterns of thought which persist in what Ana and her colleagues tend to think of as diametrically opposed styles and areas of work. We saw in the introduction to this dissertation the ways in which population genetics modelers take many core ideas from a cybernetic lineage, both in the mathematical techniques they use for representing and visualizing mosquito relationality and in the broader framework of thinking about complex, dynamic systems engaged in feedback loops with their environments. What became less useful and relevant for thinking people since the middle of the last century has continued to be useful for thinking mosquitoes, with some change along with considerable intensification and refinement. We can see from this perspective a story of divergent paths, in which questions about the regulation and management of humans moved away from a rich node of thinking in midcentury social and biological sciences, leaving this path to be more visibly owned by practitioners of the non-human.

Chapter Five: The Fertile Field

Introduction

The ultimate aim of the UCIMI project is to provide a strategy by which the human disease of malaria can be completely eradicated from continental Africa. 409,000 people died of the disease in 2019, and 229 people fell ill; 94% of them on the continent ("Malaria" 2021). They plan to work towards this goal in stages. Having designed a version of the *Anopheles gambiae* mosquito in the laboratory which spreads a refractory gene through colonized mosquito populations, the next step is to test this project out in the real world by trying out the same idea in a space they view as intermediary between the laboratory and the continent: the small island.

Islands have long been a rich site for biological study and experimentation. Charles Darwin famously developed many of his ideas about the evolution of species from observations on the Galapagos islands. Researchers at the VGL draw heavily from the field of island biogeography, which proposes a range of mathematical techniques for understanding and predicting biodiversity in isolated areas (MacArthur and Wilson 2001). Like many islands, Sao Tome and Principe and The Union of the Comoros both have higher rates of endemism, or the existence of species not found elsewhere, than is typical of continental environments (Loiseau et al. 2019). As Anton put it, when the researchers were grappling with confusing and potentially spurious genetic sequencing results, "you never know… weird things happen on islands." We will see how islands become important as biosecurity mechanisms: by demonstrating genetic isolation, researchers hope to show that any engineered mosquitoes are not likely to escape their island release site. But islands also do other kinds of unexpected things: island biogeography, for example, shows them how organisms can take strange or anomalous forms in isolation. In this chapter, I draw out the tensions in this work between a scalar, potentially universalizing, goal and the strange specificities of what I here call "local mosquitoes." Local is defined in this work primarily in relation to its counterpart in the laboratory; local mosquitoes are wild-type mosquitoes, unpredictable genetically and behaviorally compared with the better-understood colonized insects of routine lab work. Local is, of course, also a product of the tools used to study mosquito habitations, and the frames of possible interventions (Kelly and Lezaun 2013).

I show how local mosquitoes trouble distinctions between organism and environment. The techniques of analysis used by the lab allow them to turn the mosquito inside out: either finding stories about geographies and histories within the body of the bug or using what they know of geographies and histories to understand and predict informational mosquitoes. Genes, places, and histories are all intermingled here, alternately coming to the fore or vanishing as researchers choose different tools and frameworks of analysis. The mobility of their practices allows the researchers to fluidly transition between bugs, people, ways of life, historical events, long durée geological history, ecologies, and topographies in pursuit of what they name as dynamics. Patterns of relational change, dynamics show how things are related in the present, but they can also be extended into the past to understand phenomena as diverse as the beginning of agriculture and the effects of Hurricane Katrina. Extended into the future, they both serve to answer concerns about safety and risk, as well as to provide data the researchers want to use to plan future experiments and generate for themselves expectations of their outcomes.

A description of this fluidity will take us through some of the mathematical modelling techniques used both by the Vector Genetics Laboratory and by John Marshall's lab at UC

Berkeley, which specializes in biostatistics and epidemiology. A look at these techniques makes clear, however, that while the work is focused on mosquitoes, both engineered and wild type, and on the islands where the group hopes to release them, this work is not *about* the bugs or the islands per se. Rather, information about both insects and places become feed for the models, which are both heuristics for learning about dynamics as well as, in a very real way, these dynamics themselves. Both analytic and object, it is these relational patterns that the researchers are focused on. Living people and mosquitoes, real places in the world, become important through this work which aims its gaze beyond them. We see emerge the fractionated tension between the requirements to attend with extreme precision to the realities of insect and human life in specific places and the goal of generating an experiment whose purpose is proof of concept, which can show that the same dynamic can hold anywhere.

It is clear, however, that the local is not only swallowed up or used by the scalar vision of the group. There is no final stage at which the project will be rolled out into non-local mosquitoes, devoid of the embodied spaces, relationships and histories they are busy accounting for in these island test sites. Researchers sometimes use the word “stupid” to speak about “stubborn” mosquitoes that do not adapt—like those which die in the laboratory rather than be colonized, for example. But we also see how they frame “learning” as a question of the body, and the result of intergenerational transitions among mosquitoes. The kind of mosquito intelligence they indicate here is what makes mosquitoes such intransigent foes, to borrow a dominant language of conflict and war in the field. They can “learn” to avoid bed nets, feed at different times, breed in different places and in different ways. It is this intelligence, profoundly divergent from what we think of as human intelligence, which necessitates, according to proponents, genetic methods of modification or suppression. If mosquitoes are too wily for us, they may not be too wily for each other. And it is this resourceful, adaptive capacity of mosquitoes which is folded into the project as a strength: the ability of the nascent relationship between engineered and wild type mosquitoes, and between engineered and wild type genes, to gain a foothold in any place, now or in the future, will be a question of enrolling what is local, specific, and particular about mosquitoes into this new dynamic.

A look at the tensions which emerge between scalar goals and local research highlights the ways in which they are interdependent because of the nature of interventions aimed at the sexual interface between laboratory and wild type. A look at the management of such tensions also reveals that local specificities are not small roadblocks, gradually cleared away in the process of making the continental possible but are rather the source of effectiveness of the project itself. We see how this mutates slightly traditional narratives about change and innovation which circulate around this project in regulation, science journalism, and in some of the ways UCIMI leadership describe themselves. Implementation is not an after-effect, a simple follow through of change made possible when the new mosquito was designed in the laboratory. It is the main event, iterative and laborious, unpredictable and ongoing. A view of this work as a practice of learning to surf on the waves of infinite and unknowable localisms can open up a new perspective on the future of genetic engineering that is wilder, more varied, and more precarious than we often think.

Working in the Field

Robert Kohler begins his book *Inside Science: Stories from the Field in Human and Animal Science* with a relatively obvious observation: “In nature and human societies, the course of every action and event depends on the context in which it happens". His thesis is that "scientific investigations of these activities are likewise best carried out in context. How could anyone think otherwise, one might ask, when the force of situations is a fact of daily experience? Yet in the culture of modern science- at least at its high end- true knowledge is taken to be what is true everywhere, in any context, or in no context. So the scientific procedures considered exemplary- experiment, hypothetico-deduction, randomized trials, statistical proofs of two-variable cause and effect- are those that eliminate or define away contexts as extraneous complications" (R. Kohler 2019).

Kohler makes the case for context, arguing that "resident science", produced in situ through a mutually situating relation between observers and observed does not deserve the relatively short shrift it gets in current prestige hierarchies of knowledge production. It is less visible, and less valued, but no less characteristic of modern science, and no less integral to ways of knowing the world. Citing the philosophers Samuel Gorovotz and Alisdair MacIntyre, he advocates for an almost upside-down framing of the relationship between abstract sciences and "sciences of the concrete": perhaps the former are not foundational to the latter, but parasitic on them (Gorovitz, S 1975). As has been most notably demonstrated by Bruno Latour in his work with scientists, all knowledge is made in concrete contexts, and it is only through a complex series of transformations that some conclusions attain the status of universal laws. Kohler makes the case that scientific practices immersed in and inseparable from their contexts are not "merely" the preliminary, baseline, or raw data fed into the more rigorous machinery of detached or abstract generalizing, nor are they the passive field onto which such generalizations are applied or tested. The relationships of situated practice he is interested in often work in different ways than hypothesis testing in the laboratory- often more analogical and exploratory- but they are no less rigorous for their lack of detachment from the world.

The UC Davis Vector Genetics Laboratory is among several University of California and other institutions working on the UCIMI field trial discussed here. Their remit, specifically, is the "fieldwork portion" of the trial. In this chapter, I explore what this fieldwork looks like, in expectations and in practice. Gene drives, and the mosquitoes that bear them, come along with an implicit narrative about how the grand changes they are considered to be capable of bringing about may come to pass. The mosquito itself, developed over years of painstaking work at UC Irvine, is often at the center of these narratives. Having "worked" in the laboratory (that is, having spread the malaria-refractory genes successfully through laboratory colonies of mosquitoes housed on-site in Irvine insectaries) the mosquito is now ready to "work" in relatively the same way in the setting of continental Africa. In between the laboratory and the continent, however, lies the "field site." I begin here with a discussion of the "field site selection process" whereby the team located appropriate places (all islands, as we will see) into which they could release the mosquito, observe its behavior, and extrapolate these observations to predict what may happen on a larger landmass. At this stage, field sites as "contexts" are evaluated for their expected ability to stand in for or represent other places. Field sites thus play a part in a scalar narrative of gene drives. If they work in the laboratory they can work in the field site, and if they work in the field site, they can work anywhere. The abstract principle expands from laboratory to globe through the verification step of an open field trial with no real transformation of what is happening to the mosquitoes. It is primarily the size of the endeavor which changes here. The project itself is envisioned as moving in a way not unlike the gene drives themselves. Because the gene drive is passed down to not half but all of an organism's offspring, its spread (or ‘saturation’) is hypothetically exponential: the more mosquitoes have it, the more will have it next generation. The rapidity of this process is among its primary selling-points. The project of people, money, and stuff which accompany these mosquitoes are likewise cast as an expansive force. Success in the laboratory, for both scientists and mosquitoes, is the seed in which enormous global change is already contained.

The fieldwork portion of this trial led by the UC Davis VGL is, like many of the resident sciences Kohler describes, relatively less prestigious than the abstract laboratory work of the geneticists at Irvine. The goals of fieldwork within this trial are to make sure that the gene drive and the refractory gene do, in fact, spread through the trial site populations, and to check for anything strange that might happen. Among the strange things the scientists consider possible are target-site polymorphisms (discussed in Chapter Two), which would prevent the genes from spreading, other kinds of genetic mutations, resistance from human populations who may refuse or revoke their support, or political upheavals that may remove government officials with whom they have been negotiating. There are other possible unusual events that they must check for, but do not take as seriously, such as horizontal gene transfer (whereby the engineered genes might get into the genomes of beings that aren’t mosquitoes) or harmful allergic reactions among humans bitten by the engineered mosquitoes.

Fieldwork, here, is mostly posed at the outset as making sure that nothing that isn’t supposed to happen happens and making sure that what is supposed to does. It is not framed as an inventive, or particularly creative, process. In this chapter, however, I show how a funny thing happened on the way to implementation. Rather than serving as raw data for input, or passive recipients of invention, field sites did lots of interesting and surprising things. This was, of course, no surprise to the experienced entomologists leading the work, for whom deviations between the laboratory and the field and surprises in "nature" or "the wild" are to be expected. Following the course of this work, we see the dramatic importance of the context Kohler speaks for in Inside Science. But we also see how object and context get mixed up. For mosquito population geneticists, as we will see, place becomes a quality internal to mosquitoes. The lives mosquitoes lead affect their genomes. Context thus shifts: from being a place in which to implement the same population genetics phenomenon of the laboratory to become an integral part of those genetics themselves. In one sense, the VGL is engaged in locating and creating a space in the world that can function like a very large laboratory; they want the island to act as a scaled-up version of the cage. As we will see, however, the practice of establishing such a place runs up against the problem and constraint of the wild. The long-term goal of all of this work is to have mosquitoes spread the drive among wild-type mosquitoes, in places of human habitation. Deviations from a laboratory setting found in these localities, therefore, are not issues to be smoothed over: the viability of the work lies in its ability to adapt to new places. Their fieldwork is as much about learning local mosquitoes as it is about creating an experimental site. We will see how both of these modes of action interact, sometimes entering into conflict with each other.

A discussion of the ways in which this relation between the insides of the mosquitoes and the outsides of their life-worlds leads us through the pathways of what I call "mosquitoey thought." Like the insects themselves, field entomologists pay little heed to distinctions of nature and culture, or to intimacies chosen or imposed. Mosquitoes have left their marks on the human genome through thousands of years of malarial contact, and mosquito relationships with humans fundamentally shape their genomes as well. *Anopheles gambiae*, like several other anthropophilic mosquitoes which feed preferentially or exclusively on human blood, do not exist without people. Their genomes, behaviors and bodies evolved specifically for us, intertwining our two species in a powerful intimacy. *Anopheles gambiae* would not *be* without people; the ways it is in any specific place (genetically and behaviorally) depend on the kinds of relationships it is in with people and other people-loving beings. These ways of being, both genetic and behavioral, are distinct. In the language of field entomologists, they constitute distinct and locally-specific "wild-types." Because the UCIMI program is oriented around mating events between the laboratory mosquito and these wild types, local specificity enters the heart of the project through that sexual interface. If the genetics of local mosquitoes vary (due to, the entomologists will tell us, the varying local lives they lead), so will the sexual interface between laboratory and field vary. How much, and with what outcomes, is the primary question of this open field trial. Kohler points out in *Inside Science* that "residence is less a spatial than a relational and ecological instrument of thought." In this chapter, we will see how both people and mosquitoes situate relationally and ecologically in field sites, and how practices of residing constitute and transform each.

Kohler begins the term "resident science" with anthropology and its method of participant observation. The term has the benefit of treating together both human and natural sciences through the way they produce knowledge with relations in place. Fieldwork practices for anthropologists involve long-term residence with and participation in the daily lives of others. Residing, participating, and entering into relationships with others in the field allows the anthropologist to use him or herself as an additional tool of study. Like mosquitoes, we do not only place ourselves in contexts, but are shaped by them. The self becomes an instrument which registers its own change; transformations both within and outside of the body and mind are made available for translation into forms of knowledge, often through ethnography. It would be impossible, in examining my interlocutors' field, not to grapple with the peculiar, nested doubling my own research creates. Sao Tome and Principe and the Union of the Comoros constitute the field for the researchers with whom I work; they constitute mine. In this chapter which deals with how they constitute and address their field, I also must make clear how I do the same. Our field sites are overlapping, but not coextensive. Our methods for translating these fields also differ. For the scientists, island sites are transformed into maps, models, data, and genes. The islands themselves become, at some points, complex mathematical abstractions when researchers try to simulate on computer programs the passage of mosquito genes through populations in space. My own daily interactions with scientists in the process of abstracting mosquitoes in these ways gets transformed according to different rules, and with different goals. The text of this ethnography thus becomes a subject, as well as a tool, of inquiry.

Scalability

Gene drives are a genetic element that also acts as a tool. In the body of an organism like a mosquito, the gene drive accomplishes its action during reproduction. If linked with an allele (an allele is a version of a gene) such as the one developed at Irvine making *Anopheles gambiae* mosquitoes refractory to the malaria parasite, the "drive allele" will "cut" or "snip" the growing offspring's DNA at precisely that location on the genome. The cell then repairs this break with "homology directed repair", using the drive-containing chromosome as a template. So, if you are the incipient offspring of a mother bearing allele A and a father bearing allele B, you would normally have a chance of being heterozygous for that gene, in which case you would have both allele A and B. But if your father's B allele was linked with a drive element, at some point in your formation that spot where your mother's A is located would be "cleaved" or "cut" by the drive. Your body would try to repair this damage, and it would look to its intact allele on the other chromosome for reference. Using that reference, that B from your father, it would copy the intact allele, making you homozygous: at that spot in your genome, you would be BB.

Gene drives are not new, per se, nor are they completely unnatural; scientists noted that some genes accomplish this act of "selfishness" all on their own over sixty years ago. But contemporary gene drives are considered by some to be the beginning of an entirely new relationship with evolution largely because they are so precise. This precision is made possible by the genetic editing technique CRISPR which, like the gene drives it can be used to create, "snips" or "cuts" bits of DNA with unprecedented accuracy. A scientist can know exactly where she will "cut" DNA, and she can do so in the same place every time. Gene drives have been demonstrated in laboratory settings to "act" in very precise ways, copying a chosen allele into newly homozygous offspring reliably and predictably.

Gene drives as material (if invisibly small) objects are thus often posed as constituting a definitionally expanding force. What a gene drive system is linked to could be hypothetically anything- that B of your fathers might give you red hair or, hypothetically, wings. The gene drive is seen not as the substance of change, but the action of it. It is a mechanism for movement, through generations, of engineered traits. Gene drives are best visible at the population level. On your own, it is possible that you may have ended up BB with no intervention whatsoever. But if you are one of a generation of a thousand people all born homozygous for BB, the effectiveness of the gene drive becomes visible. Gene drives are valuable in hypothetical future application for their speed and their irrevocability. Once your parents have died, and every older person bearing an A along with them, the thinking goes that there will never be any more AB (or AA) people around. And if people, like mosquitoes, lived only about a week and had a couple hundred babies each, this transformation would not take all that long.

What gene drives offer, to proponents, is a radical alteration in the rate and mechanisms of change. No longer subject to the coin-flip of normal Mendelian inheritance, the drives allow genetic elements to propagate in every body born, to "burn through" or alternately to "saturate" populations very rapidly. Gene drives are a technology of scale. The population may be a few hundred insects in a laboratory colony, or it may be every individual of that species on the continent of Africa; writ large or writ small, the "behavior of the drive", understood as the rates of inheritance across generations, remains stable. Mosquitoes (within species) are interchangeable with each other. The geographic landscapes in which the mosquitoes live and breed are important primarily insofar as they block (for example, a mountain range) or facilitate (for example, a road through which people drive unwittingly carrying eggs on their shoes) contact and mating. The UCIMI project aims at transforming the *Anopheles gambiae* mosquitoes of continental Africa. Their first open field trial could, hypothetically, take place anywhere that meets a set number of criteria. The phenomenon of the project can be zoomed down to the lab or up to the continent, or somewhere in the middle without changing.

Anna Tsing, in "On Nonscalability" tells us a nonscalable story of the history of the kind of expansion we see in this gene drive narratives. Scalable projects depend on an ability to expand without transforming; the nature of the project does not change as more elements are added, nor when its basic premise is shrunk down to a microcosm. This ability to expand without changing, Tsing writes, depends on an eradication of transformative relationships. How did it come to feel natural, this idea of expansion as the way that humans inhabit the earth? Classic twentieth century population genetics played a role, she points out, in blocking "attention to diversity making processes, because it was a science of expansion. By taking scalability for granted, it asked how populations expand… Diversity was the current scoreboard of varied but similarly autonomous strategies of conquest." We can think here of the birthplace of modern biological control in late nineteenth and early twentieth century California, described in Chapter One. Like the sugarcane plantations Tsing discusses in her history of scalability, this was a landscape of rapid and massive extinguishment of native entanglements, both human and nonhuman. Remade as terra nullius in the minds of some large-scale farmers, the California citrus orchard, like the Caribbean sugarcane plantation a couple centuries earlier, was repopulated with people and plants intentionally isolated, engineered for alienation and control. Biological control projects reckoned with the consequences of scalability— with the unique vulnerability of what Tsing calls nonsoels, or landscape elements without transformative relationships (Tsing 2012). Extracted from the relationships of "natural enmity" that they theorized held the naturally inexhaustible expansionary tendencies of population in check, inadvertently imported pests were seen to mount a formidable scalar project of their own.

At one level of discourse and practice, the UCIMI project offers a traditionally scalar narrative. Like a twentieth century economy of scale, it promises to expand across the globe without changing. Among those charged with the implementation of this scalar program, however, transformative relationships come to seem much less extraneous and eradicable. Zooming in to the field we do not see a smaller version of the same phenomenon. Mosquitoes are not actually very interchangeable. And a terra nullius cleared of transformative relations would, for *Anopheles gambiae*, necessitate a world cleared of people and their worlds. The UCIMI project as a whole does not share one way of talking, doing, or thinking, even about itself. The view from the laboratory feels significantly different from the view from the field. Both, as we will see, must in turn be learned by the modelers who translate these places into digital abstractions. We thus see, as Tsing pointed out, scalability riddled with nonscalability. Unlike weeds or matsutake, however, transformative relations do not only characterize those forms of life which grow up in the ruins of scalability projects. What expansion means, what it looks like and what might challenge it, appear differently in lab and field, not to mention boardroom and regulatory conference.

The UCIMI's ideas about its own scalability are sometimes phrased as a "philosophy." This most often happens when they are contrasting themselves to their primary rival, competitor or "nemesis", Target Malaria. Target Malaria is working on a very large-scale project that is very similar to the UCIMI's. They want to use genetically engineered mosquitoes, equipped with gene drives, to eliminate malaria from Africa. As we have seen in Chapter Two, they propose to do so using a population suppression strategy, whereby *Anopheles gambiae* mosquitoes simply disappear when they cease to be born. This is contrasted with the population modification strategy of the UCIMI, which plans to use the gene drives to spread a malaria refractory gene to all the mosquitoes, which would prevent them from transmitting the parasite to people. With population modification, the mosquitoes are still there but they are different. These two strategies were highlighted by the UCIMI in the way they presented their findings about target site polymorphisms, which they believe to have firmly demonstrated are likely to be an issue for the Target Malaria strategy but not their own. Expanding by killing all the mosquitoes or expanding by transforming them produces different images of scalar growth, but that is not the only difference between the two groups' "philosophies." Target Malaria, funded by the enormously wealthy Gates Foundation, wants to establish a coherent regulatory structure for continental African release before initiating an open field trial. Their philosophy is to have the science and the regulation in place before putting mosquitoes on the ground; deployment would thus, be a culmination of their work, in theory. Once initiated, the scale would be immediately continental.

The UCIMI, by contrast, speaks of their philosophy as one of incremental growth. While Target Malaria is working with the United Nations to draft broad international legislation on gene drives (which, in a continental setting, could not be restricted by country borders), the UCIMI is working with The Union of the Comoros and Sao Tome and Principe directly (with help from the African Union). Their aim is not to draft broad legislation to address the legal ambiguities of gene drive applications, but to get legal permission to release them in one geographically isolated country. Their theory is that, having been released once and shown to operate successfully, the next stages can be addressed from the new vantage point created by that success. Their plan is to "start with simple spaces and move to more complex ones." The project is envisioned as capable, in the future, of its own growth potential once it becomes a reality in some "field" or real-world place outside of the laboratory. The field site is posed as an intermediary stage between laboratory and continent, with a medium level of complexity. While they have been encouraged to stop saying the word "simple" of their island test sites, the VGL team "thinks in terms of high and low complexity… We know island biogeographies tend to be less complex. The bigger the population, the more complexities it carries; the smaller, less." Part of this complexity is what Tsing calls transformative relationships. These, she points out, are "the medium for the emergence of diversity." Tsing's lauded "diversity that might change things" is, of course, not necessarily a welcome prospect for the UCIMI team. What if those relations change the phenomenon they produced in the laboratory? What if the project will not scale? Fieldworkers are tasked at the outset with addressing these concerns. As we will see, however, what they find through studying mosquitoes in place, enmeshed in a variety of relationships with places and other species, is not only a yay or nay on the scalability of the project, but a slightly transformed image of it. In studying the transformative relationships that are posed as a potential threat to scalability, thinking on their scalable project is itself flexibly transformed.

Field site selection

My first conversation with Greg Lanzaro was in early May 2017. I was there to interview him and Yoosook Lee in preparation for a fieldwork project I was preparing to set out on in Brazil and Florida. I was studying discourses and controversies over the implementation of the Oxitec mosquito in both places, and I wanted to get a broader idea of the field of mosquito population genetics to put my research in context. To my surprise, Greg started to talk to me about a big project through the UCIMI, not yet announced. It was to take place on some islands in Africa, he told me. The islands hadn't been selected yet, but they had a shortlist. Of those, however, he spoke only of the Union of the Comoros. He described the clear blue waters of this "tropical paradise", before telling me that the team still did not have a social science person on the project. The UCIMI field trial was still not public, but at that time Greg had already agreed to do the fieldwork portion of the trial with his lab. In the Spring of 2017, that looked to him like it should take about a year and a half. They'd visit two potential sites that Fall, and two more in the Fall of 2018, evaluating the facilities and the people, and collecting data on populations there and at landfall to assess genetic isolation. Once a good island had been selected, the next thing to do would be to set up a base there and get the whole thing going.

Greg's timeline did not shake out that way in practice. As of the Summer of 2021, the group is still in talks with both the Union of the Comoros and with Sao Tome and Principe, but the Union of the Comoros looks increasingly unlikely as a potential site. A new article outlining their field site selection criteria is in press. No base has yet been set up, although Joao Pinto of the University of Lisbon has agreed to relocate to Sao Tome and Principe to be their resident point-person at some time in the near but as-yet unspecified future.

In the field site selection process, islands appear as spreadsheets of criteria, broken up in the early stages of the process into tiers. In tier one was vector species composition and isolation, and texture of topography (what species are there? How isolated are they from the mainland? What does the geography of the place look like?). In tier two the group considered insecticide susceptibility, colonizability, and malaria (how many people have malaria there? How many mosquitoes do? How easy would it be to get the mosquitoes of this place to live and breed in the laboratory? How many of them have developed resistance to insecticides?). Tier three included criteria the team believed they could learn only by visiting locations. It included security, willingness, human resources, insectary capacities, and laboratory facilities (are people likely to oppose or support our project? Will we have to pay bribes? Could we be in danger? What kinds of bureaucratic institutions exist there, and how could we work with them? Do they have insectaries and laboratories?). Only islands were seriously considered because they are likely to house genetically isolated populations. This means that the mosquitoes on that island are not interbreeding with mosquitoes on the mainland. This is extremely important to gene drive research because, if there were interbreeding with mosquitoes in other countries, the gene drive would be circulating there too, potentially without approval from the government in question. And if that other country were in continental Africa, it would both defeat the purpose of the field trial (which is to test the mosquito in a closed system) and abrogate significant issues of international law. The ability of researchers to contain gene drives is one of the primary open questions posed by opponents of the research: how can you test for safety and efficacy if you cannot be completely sure about containment? Genetic isolation is represented by Fst values, which are a measure of genetic difference. Populations with higher levels of difference probably aren't interbreeding as much as populations with lower levels; above a certain point, scientists believe they can confidently say that populations are not interbreeding much at all.

The Union of the Comoros and Sao Tome and Principe are attractive sites because they both have *Anopheles gambiae*, they both have malaria, and they both have some "geographic texture"— landscape elements like mountains, lakes, or uninhabited forest area which impede the spread of mosquitoes. Seeing how the gene drives "move" across such textured landscapes is considered good for generating predictions of movement in even bigger and more varied geographies, as well as "more realistic" than laboratory colonies who don't have any geographic texture at all. The UCIMI was attractive to local governments and mosquito control authorities because, while both countries have exceptionally good malaria control programs, they have not been able to reach zero incidence of the disease. The thinking goes that, if you are a wealthy tourist from a non-endemic country, you are likely to be more willing to book a vacation at a place with *no* malaria *than* low malaria. That difference between low and no has been, historically, extremely difficult to achieve. Pursuing it, not only for public health but also, to a large extent, for tourism revenue, is a major incentive to cooperate with a genetic engineering trial.

In the initial years of the field trial project, the Union of the Comoros was the presumed favorite as a trial site. Located close to Madagascar, the team conjectured that they might be able to move from these small islands (there are three in the Union) to the larger one of Madagascar and then on to continental Africa. This was part of a plan to "start with simple spaces and move to more complex ones." However, a planned trip there in February of 2018 was abruptly canceled. The Deputy Director of the Ministry of Public Health had agreed for a group of researchers from the VGL, including myself, to come for a trip which would include regulatory discussions and preliminary mosquito collections. Just days before our flights he notified Greg that he had not received authorization from his superiors to meet with us and asked that no one enter the country. In the wake of this surprising news, the VGL struggled to stay in contact with people in Ministry of Health and University there. It became clear over time that part of this issue was technical: because of frequent power outages, most people communicate primarily using cell phones, rather than computers. The California researchers were sending emails, but their Comorian counterparts preferred Whatsapp. Even after establishing contact in this way, however, difficulties with communication pushed the Comoros to the project's "back pocket" where it remains today. Similar issues occurred with a planned trip to Sao Tome and Principe the following year when new elections ousted the Minister of Health with whom an agreement had been signed. Despite that, communication between the VGL (primarily Ana) and the Ministry of Health in Sao Tome and Principe has been frequent. They have hired Adionilde Aguiar dos Santos to help coordinate the project, and she and Ana maintain a close relationship.

The field site selection process began from a bird's eye view, in which potential field sites were visible as tabulated lists of geographic, entomological, and infrastructural qualities. As fieldwork progressed to discussions with partner countries, researchers zoomed in to what they see as a lower level of abstraction. With this process, however, the field sites themselves did not remain stable. New information emerged. People (and mosquitoes) live in these countries, and they are both very busy doing all kinds of things which intersect with the plans of the UCIMI.

Local Mosquitoes

The VGL has still not been able to begin large-scale collections of mosquitoes from either Sao Tome and Principe or The Union of the Comoros. The former are actively still in negotiations, and the latter have flatly refused the group to collect until an agreement is finalized, which is not looking likely. The researchers did discuss the possibility of covertly smuggling mosquitoes out of the country on trips officially designated for discussion only but decided the logistics of mosquito preservation and the risks of destabilizing their relationships with locals were too great. They do, however, have some samples from both places from previous fieldwork trips, as well as genotyped mosquito samples from large databases like AG1000, an international collaboration which provides genetic sequences for *Anopheles gambiae* mosquitoes collected by researchers around the world. Samples are tagged by the location where they were collected, and researchers can compare their genomes to see genetic variations across place.

The relationship between genes and places is perhaps the central concern of VGL fieldwork practices. The purpose of the open field trial is to see what happens when the *Anopheles gambiae* mosquito designed at UC Irvine is released into a “real world” space. This is often phrased as a question about the “performance” of the genetic modifications themselves. How do they move through populations in space? In other words: if the group releases several thousand mosquitoes onto one of these islands, how long would it take for all of the *Anopheles gambiae* on that island to carry the refractory gene? What would intermediary stages look like?

This is frequently envisioned as a question of saturation: the gene (with the help of the gene drive) saturates a population like a drop of colored water saturating paper. The progress of the gene across space happens through the mechanism of reproduction: "movement" here means the birth of new *Anopheles gambiae* mosquitoes with this engineered trait. Movement across geographic space is impeded when reproduction does not happen. It is largely assumed that the main reason why mosquitoes would not reproduce is if they do not have contact with each other. For example, they may not cross a large mountain range. They may also not cross large spaces which are uninhabited by people. *Anopheles gambiae* do not live without people; in a deep forest, miles from human habitation, the lab believes it is vanishingly unlikely for *Anopheles gambiae* to live, and their preliminary research largely bears this out. Barriers to mosquito movement and reproduction create what they call "population structure." A completely unstructured population would have relatively similar genes regardless of where they are. Like a well-shuffled deck of cards, a mosquito randomly selected from anywhere is likely to be the same amount of similar or different (as measured by the number of genetic variants shared or not) as one from any other location. To return to the metaphor of watercolors, a structureless population would be a sheet of paper evenly washed in brown; one with lots of genetic structure would have blotches of distinct colors. Scientists expect the mosquitoes of an island to be somewhere in the middle— some distinctions of shades would be discernible, but there would also be some blending of the colors.

Understanding population structure is considered central to predicting the movement of genes, including genes designed by people. More difference, or more structure, is interpreted as evidence of less interbreeding. This applies, for example, to the question of genetic isolation. Demonstrating that island trial sites are genetically isolated is a crucial prerequisite to conducting work. But what does it mean to say that an island is genetically isolated? How do you know that mosquitoes aren't somehow travelling to and from the island? The researchers consider it impossible to track the movements of individual mosquitoes, although they do include data on the movements of boats and airplanes which could, hypothetically, carry mosquito eggs (most airplanes are treated with insecticides before takeoff). Instead, they think it is more reliable to interpret genes for evidence of past movement (and mating). The distinction between these two approaches emerged at a lab meeting in the Fall of 2019, when the group brought up once again what they called "the astronaut mosquito paper" in which another researcher had claimed that mosquitoes can and do fly much higher and further than models like the VGL's accounted for. The entomologists of the group were highly suspicious about mosquitoes' abilities to survive the wind and cold temperatures of such a journey, but the primary objection was that "If they are truly mixing, the genetics will show that. It doesn't show that." If the population of *Anopheles gambiae* on an island and the population on the closest other landmass are "highly differentiated", or very different from each other, researchers see this as incontrovertible evidence that mosquitoes are not travelling between the two. If they were travelling, they would be mating, and if they were mating, they would be more genetically similar. The measures they use for genetic similarity are called Fst (fixation identity) values. An Fst value tells you essentially how much difference there is between two mosquitoes, or between two populations of mosquitoes. In Fst values and discussions of genetic isolation we can see the one central tenet of mosquito fieldwork at play: (genetic) difference means (geographic) distance. To be "further" means to be less related. As we will see in a discussion of modelling efforts, "distance" acquires complex meanings here, in which relatedness and geography blend.

Here, though, we might linger over what relatedness and geography look like in the field. Genetics and place are intimately linked in the formatting of genomic data storage. The location from which a mosquito was collected is recorded, either by coordinates or (sometimes confusingly) by village name. This is important for the creation of maps which show genetic structure. If they can create a good map of what genes are where in an island right now, researchers believe they will be better able to register changes to the distributions of genes when engineered mosquitoes are released. By extrapolating out these registered changes, they can make predictions about how distributions of genes in a larger area, like continental Africa, might change should the same thing happen on a larger scale. Islands are important here because they have "reduced biological complexity" both in terms of species composition richness (there are fewer different species on islands, on average) and genetic complexity (there are fewer variations in the genomes of *Anopheles gambiae*). This reduced complexity does not hold steady for any kind of island; the histories of the islands themselves is an important part of the type of storytelling these researchers practice which holds together places and genes. Oceanic islands like Sao Tome and Principe, which formed which rose to the surface from the ocean floor, are older and usually have more separation in species types and distributions than continental islands, which used to be part of a larger landmass. Lacustrian islands, which exist in the middle of lakes (like Lake Victoria) are too unstructured; they have too little genetic complexity. Testing the mosquitoes by releasing them there would not tell you very much about how the genes "move" or "behave" around geographic or other boundaries. Bridge islands, which were formerly connected with mainlands, are not very good for the opposite reason. They have too much genetic complexity. Polymorphisms, or parts of the genome where some mosquitoes have one bit of code and others have a different one, are too prevalent on such islands, which had until recently been open to genetic mixing with continental populations, which have a lot of them. Sao Tome and Principe (two islands) and The Union of the Comoros (three islands) are all oceanic islands. None of them have ever been connected with mainlands, and all are volcanic. This led the researchers to expect that there had historically been little genetic mixing between the islands and other places, and they calculated Fst values using samples from islands and mainland sites to confirm this. Satisfied that mosquito travel between prospective sites and mainland areas must have been very low in the past, as evidenced by lots of genetic dissimilarity, the researchers felt confident predicting very little mixing (or "gene flow") in the future. This, they hoped would satisfy regulators and allow them to pursue a strategy of working at the national level. Because they felt they could say with statistical certainty that the GEMs would not travel out of the islands, they felt they could make the legal and regulatory case that it was not the immediate concern or business of neighboring countries whether one of these island nations decided to allow a release. Verifying containment also allowed the open field trial to serve as a more manageable experiment. Like the MONIAC computer made to model the UK economy in the 1940s with water, the island, once contained, is a closed system. This closure renders it a sort of very large device for the monitoring of flows- here, of gene flows.

So, to be a local mosquito is a question of place and of genetics. Local mosquitoes are referred to by the scientists as "wild-type" mosquitoes; this term refers to mosquitoes that live outside of the laboratory— by default, in a specific location on earth. Laboratory mosquitoes are not seen to bear locality within themselves in the same way. Laboratory strains have origins, of course. The "Rockefeller strain" for example, originally came from Cuba; the "Liverpool strain" originally from West Africa. The sites of collection are obscured by naming practices which refer users to the laboratories that colonized them. Even these names, however, do not denote placed-ness in the same way that the collection site tags do for wild types. Both of those strains are examples of mosquitoes that have been colonized for many, many generations in laboratories across the world. They are standardized laboratory organisms; their genomes serve as predictable reference points. Their behaviors are predictable, and very distinct from "wild-type" mosquitoes. They are un-picky eaters, feeding easily from animal blood encased in artificial membranes. They do not swarm to copulate. They will not die very easily if there is too much space, or too little. Genetically and geographically, laboratory mosquitoes are thought of as placeless. The laboratory is not thought to be "local" because it is standardized. The life experiences of a mosquito in Liverpool and in New York are expected by the scientists to be much the same. Because the environments and experiences of these mosquitoes are much the same, scientists say, so are their genomes. Mosquitoes are exceptionally plastic organisms; their bodies and behaviors change relatively quickly in new environments. The process of turning a group of "wild type" mosquitoes from a local place into laboratory mosquitoes is known as colonization. Some wild-type species are considered easier than others to colonize (see Chapter Three). Over time, genes connected with traits or behaviors favorable to the environment of the laboratory are selected for; those which lead "stubborn" or "stupid" individuals to die in the process of colonization are not. Although laboratory strains are considered placeless by the researchers, this placelessness reflects how they think locality works for mosquitoes in general. The lifeways and environments provided by a specific location on earth, they say, incentivizes certain ways for mosquitoes to be in body and behavior. They adapt; their genes, as well as their interactions with a place, are specific to it. This is how a researcher can look at a dead mosquito in a small tube of ethanol in her laboratory in California and see in it (after a series of intervening steps) information about the place it, and its ancestors, were born.

Locality is a quality of the body, measurable genetically. It is this locality that VGL researchers are tasked with studying. To understand the genetics of the mosquitoes, they believe they need to understand the worlds they are in. Here we see how context and object become blurred: the inside and the outside of the mosquitoes change places depending on the methods and questions at hand. We could take, for example, cisterns. On Grand Comore people traditionally gathered rainwater in cisterns located near their homes or in village centers. Communities raise and share fish to live in these cisterns which eat the larvae of the mosquitoes, which seems to work quite well, according to Anton's interviews with locals. On Moheli, another island in The Union of the Comoros, or most other places, *Anopheles gambiae* breed in puddles or other small amounts of fresh water open to the elements. But when the researchers visited Grand Comore they found very few immature mosquitoes in these kinds of places. If most of the *Anopheles gambiae* on that island are breeding in cisterns, how ought the researchers to expect seasonality to influence their life cycles? Cisterns do not dry up like puddles when there is little rain. Does cistern living affect the density of their larvae? The time they spend in various life stages? How might things change now that climate change has brought many cisterns low, causing the government to install public water access? Do people even know anymore where all the disused cisterns are?

In Sao Tome as well, mosquitoes lead lives distinct from their abstract descriptions in laboratory spaces. *Anopheles gambiae* normally strongly prefer to feed on people, but the researchers believe that on these islands they are feeding on dogs. There are "like a gazillion dogs" in Sao Tome; "they should call it dog land." How does this affect malaria transmission? Why would the mosquitoes be doing that? Abram pointed out that most houses there are raised on stilts. Might the mosquitoes be entering the houses less, and feeding on the dogs sleeping in the shade of the houses? “People there, unlike here, we love our pets; dogs don’t sleep inside people’s houses they sleep outside. So, if you have a mosquito that likes to feed on dogs it’s going to follow the dogs.” How much are they feeding on pigs or chickens? And how can the researchers figure out what the mosquitoes are feeding on? They figure that they need to capture some adults to test the blood still inside of them. “We gotta get underneath those houses” Greg said. “We need to bring guys like Abram who can crawl underneath those dog-infested spider encrusted houses. Me and Anton have done our share of that. It’s time to pass the baton to the next generation. That’s how it works.”

Researchers refer to mosquitoes “learning” to feed on dogs. They also refer to them “learning” to circumvent human strategies for avoiding them. Anton pointed out that in Sao Tome, for example, “people recently found mosquitoes previously feeding indoors late at night have shifted their behavior to feed earlier and not indoors” in response to people’s use of bed nets and Indoor Residual Spraying (IRS) of insecticides. He presented a map of places where that new behavior has been documented. “We should see when we go if they are avoiding indoors. They’ve been using IRS and bed nets for a while, so selection may be happening.”

What do they mean when entomologists or population geneticists say that mosquitoes “learn”? Are they watching other mosquitoes do something and figuring out inside their heads that it might be advantageous to do the same? The researchers I work with would say absolutely not, mosquitoes do not *think* in that way. Rather, “learning” in this context would mean for them something much closer to the word “adaptation.” It is not cognitive. There are many people who study mosquito cognition, but this is not what VGL researchers are thinking of when they speak in this way, which is common in their field. Rather, learning is biological. It is a change the body does with its environment; it is a way of becoming with a certain set of materials and relationships. Researchers say not that mosquitoes “learn” to avoid the indoors the way a human child or a corvid might, but that some simply do for no very good reason, the scientists say, and those mosquitoes live longer or feed or mate more. Their offspring are likely to do the same. The researchers believe that such processes would probably show up in the genes of these mosquitoes, if they knew where to look for it. “Learning” is not a story about sentience putting material to work. It is a mindless response of the body. Mosquitoes learning to avoid bed nets or Indoor Residual Spraying by changing their feeding habits is not of a different order, for them, than the introgression of the KDR variant, another major focus of research. This allele confers resistance to most major insecticides, and in the past decade it has been “introgressing” into Sao Tome. Some mosquitoes with the allele made their way onto the island at some point, mated, and passed it on to their offspring. Over time, more and more mosquitoes have come to have the allele. This is part of the argument in favor of genetic methods for malaria control— the argument goes that mosquitoes evolve resistance to all insecticides after some time; that insecticides will never work well forever. Researchers at the VGL have also worked on other projects tracking the introgression of KDR through populations in other places, mostly in Africa. The stories they tell about how KDR moves work according to similar rules as the movement of the malaria refractory gene, except that the gene drive gives a “leg up” to this movement, making it “faster” by having more mosquitoes of a next generation homozygous for it. Researchers do not speak of their engineered genes as being “learned” by the mosquitoes, but there is, for them, no hard division line between learning and changing. A population of mosquitoes which has “learned” to feed on dogs likely has some genetic variants different from a population that does not. The same goes for a variety of other behaviors specific to other places— like, for example, the culex mosquitoes of Sao Tome which live in the small holes dug by crabs there. The genetic changes which accompany a new way of living are no more a matter of choice than the inheritance of a malaria-refractory gene developed in the laboratory. Localities can be studied either by expanding outward from the mosquito to a description of people, their houses, their dogs, their cisterns, and potentially any number of other ways they organize their life-worlds. Or it can be studied by intensively zooming inward to the mosquito, locating the polymorphisms inside the genomes of each. Traditional mosquito control practices have works on this outward-extensive level, by altering mosquito life-worlds to minimize contact between them and human beings by killing them or warding them off from human bodies or homes. Genetic approaches work intensively by changing the bodies of mosquitoes not through learning or adaptation but through "tricking" such systems by "cheating evolution" with gene drives. For the population geneticists, both strategies exist within the same strategic universe only, they would claim, differing in effectiveness. The inside of the mosquito and the outside of its worlds are not entirely different things, but two views of the same interconnected phenomenon. What comes to matter locally are things that the researchers believe affect mosquitoes; things that may change their bodies and behaviors. And those bodies and behaviors, shaped in this way by interaction with geographies and other species, are local in their DNA.

Modeling

One of the reasons that the cisterns mattered was that researchers needed to give information about seasonality and life stages to their colleagues at UC Berkeley. John Marshall's Center for Computational Biology there is contributing to the UCIMI trial. He and his postdocs have been using data from Irvine and Davis to produce mathematical models that aim to predict the spread of the refractory gene throughout a trial island. One way of doing so is to produce maps of the islands that can simulate the percentage of mosquito populations in geographic areas which will inherit the refractory gene across a given set of time. In this kind of modelling, they create parallel worlds similar to the artificial life forms described by Stefan Helmreich in *Silicon Second Nature*. Through these "virtual alternative sites for evolution", the "computer guys" as researchers in other laboratories contributing to the consortium call them, propose that they can predict possible futures given enough data about the present.

This data of the present is expected to be provided by the VGL. In joint meetings between the Berkeley lab and the VGL at Davis, the Berkeley modelers wanted to confirm with the entomologists and population geneticists at the VGL that the data they were inputting into these models was accurate. This confused the entomologists most. Accurate to what? To laboratory mosquitoes? To wild mosquitoes? Which wild? Which season? Where? The computer guys in these moments turn to the entomologists as if they are keepers of the real, representatives of some first nature of which they must know crucial details accurately. This is true, but we ought to be careful here to remember that the entomologists do not have more claim to experience of the object of the whole group's research than the computer guys do, because this object is not bug.

The object is not bug for the computer guys or the entomologists or the population geneticists. The object of what they are trying to do may be bug for the people who live in their field sites, whom they call communities, or for the fishes that eat their larvae or the bats that eat their adult forms, if they could talk. But what the trial might mean to them comes later; for the many people busy making its instantiations in the bodies of actual mosquitoes living under the stilted porches of someone somewhere, eating their dogs alive, the object is not the bug but what they call "dynamics."

Dynamics, they say, are "patterns of ongoing change." A dynamic is a relationship over time. This relationship might be between populations: are *Anopheles coluzzii* or *Anopheles gambiae* becoming more prevalent in village X relative to the other? They might be between genes: is the KDR allele for insecticide resistance spreading in population X? They might extend back into the past, as well population geneticists use preserved specimens collected in the same place over years or decades to look at changes, or they can extend into the future as the mathematicians generate predictions. While they always deal in time they can also, from another point of view, be quite independent of it: the dynamics of the engineered gene in caged mosquitoes in Irvine can be imagined on the larger scale of an island, or a continent, unchanged. Looking at mosquitoes, whether in the laboratory or outside of it, does not tell anyone very much about dynamics. Entomologists' understanding of mosquitoes as living organisms, at the level of the body, is considered important to the dynamics of mosquito genetics, but their domain holds no special claim to the heart of the project. Mathematical models, which are considered abstractions of patterned changes in mosquito genetics, and mosquito bodies, which are imagined as concrete instantiations of those patterns, can both "show" these dynamics in the past, present, or future, but neither really "are" them. The models are seen as partial, but so are the bodies which are primarily recruited as evidence of dynamics in the present and the past. Sequenced (that is, subjected to a series of transformations to become code), they can show snapshots which, once related to each other, produce some generalizations. These generalizations are often visualized using various forms of graphs and maps. Spacemix graphs, for example, might group sequenced mosquito samples by "genetic distance" measured by similarity of variant alleles. A map of an island might be overlaid with colored dots showing the species collected there. A cross-coalescence map might project species histories into the deep past, looking like variably exhausting staircases chaotically growing to intersect each other. Similar, but often more mathematically sophisticated techniques, are used by the Berkeley lab to project dynamics into the future. Their MGDrive, or Mosquito Gene Drive Explorer, links "inheritance cubes" showing life cycles and reproductions of mosquitoes, circulations of the parasite between people and mosquitoes, and networks of interconnected mosquito populations to generate maps that can simulate how mosquito genetics of an island might change over time should engineered males be released in different places, or in different numbers.

Are these visualizations descriptions of the dynamics, or are they the dynamics themselves? For the researchers, they are both in quite a specific way. As one population geneticist explained to me, techniques like this work by staging the action within a system that is itself the object of attention (evolutionary dynamics). The system is the description and vice versa. In this way, descriptive tool and analytic object are one and the same, except that the modelled system is simplified to make visible dynamics which hold true in more complex situations but are obfuscated by an excess of complexity. In this way, "dynamics" are both a heuristic and the object of their own method. Simplification is what allows this kind of fractal quality while still creating something new (description). The models are connected to the real world by the data that is "inputted" into them or used to alter them in what the modellers call "fitting." The Berkeley lab are asked to fit their models both to laboratory data from Irvine, about how the gene "performs" in laboratory colonies, and to field data from the VGL about what genes and behaviors they have observed from mosquitoes that already live on the trial sites. The models, freestanding, then embody relationships without regard to either input, capable of assimilating new information to predict how futures might be different given choices in the present. They are seen as an intensification or a purification of relationships which do exist in nature but are more visible and tractable in this formal mode.

In the MGDrive simulations, for example, the relationships between genes generated by the physical relationships of copulating offspring are extrapolated to the level of an island field site. At the scale of the laboratory colony or the scale of the island, as the researchers see it, the dynamics may be the same (they may also be altered by factors in the field, which is the purview of the VGL to bring to the attention of modelers). The dynamic is holographic in this way: like Strathern's description of relations, it is "an example of the field it occupies, every part containing information about the whole and information about the whole being enfolded in each part" (Strathern 1993). Visualizing the dynamics verifies that they are there in the laboratory colony or the field population; it also makes them insofar as they are not theoretically there to be encountered outside their simplified digital state. This digital state is rendered as distinct from the field, but not of an entirely different plane. Researchers say it is different because of what it excludes about the field; what it retains is seen to be the same, just perceptible. Strathern points out that relations have "the power to bring dissimilar orders or levels of knowledge together while conserving their difference" (Ibid). Models for the UCIMI allow dynamics to appear as objects in their own right, though always ready to dissolve into relations between objects of another order (genes or bodies). The laboratory and the field can thus both be translated into and out of these models, testing for errors, for possibilities, for what might happen. They appear in some moments independent of scale, at other constitutive of it. They create from the chaotic flows of data generated by both laboratory and field objects for common discussion and manipulation. The objects they create are, like the work of their collaborators, a function of perspective and scale. Anton, the entomologist, works at the scale of the mosquito, and the objects that he sees are bodies. He knows the patterns of their dusty scales and the spans of their wings and the small differences in the breathing apparatuses of larvae which can distinguish species that habit different depths and qualities of water. None of John Marshalls postdocs know much more about mosquitoes than the average person; they situate their work at the level of relations between relations, as expressed mathematically. It is the population geneticists who focus most on dynamics, bridging as they do in their work something of the knowledge and the practices of both. The mathematician Benoit Mandelbrot has the lovely quote “the ball of twine has two dimensions to you and I, but how many if you were a flea?” What the object is depends on your relation to it, and what capacities of seeing you have in your body or your tools. Dynamics, as the object of research and intervention for the UCIMI, are a prosthetic kind of object, sensible only when they take things away from reality. The images they produce of these dynamics give the shape of a process, thing-ifying transformation. Thus objectified, patterns of ongoing change become more amenable to questions and prediction. Are we right about the past? What might happen in the future?

VGL researchers note the divergent ways of knowing practiced by modelers and by entomologists. Most of the time, this divergence is articulated as a productive tension. Anton is sometimes mystified or frustrated by questions about mosquitoes which seem ignorant of the complexities and variabilities of their bodies and lives. He is not overly optimistic about, for example, the current excitement over machine learning algorithms for mosquito speciation, believing that their input data is currently inadequate and unlikely to get much more adequate without creating a sort of Borgesian map that would cover all the species in Africa and their minute variations. But others in the lab counter that inhuman intelligences can be helpful precisely because they bypass the expert recognition of someone like Anton. Morphological identification by sight is extremely difficult for mosquitoes, and it is only his several decades of dedicated expertise which allow Anton to look at a mosquito and recognize it— that's *Anopheles funestus*! Likewise, an experienced entomologist may believe that they can see what is happening with mosquito populations: they discuss observations, for example, of more mosquitoes biting dogs or avoiding bed nets. Mathematizing mosquitoes dissolves them into data that does not look like bodies. It disallows recognition of the object of research. One benefit of this practice, for the researchers, is what I might call "strategic unknowing."

In the opening to *The Psychoanalysis of Fire*, Gaston Bachelard has an extended soliloquy about scientific objectivity. "Sometimes we stand in wonder before a chosen object; we build up hypotheses and reveries; in this way we form convictions which have all the appearance of true knowledge. But the initial source is impure: the first impression is not a fundamental truth. In point of fact, scientific objectivity is possible only if one has broken first with the immediate object, if one has refused to yield to the seduction of the initial choice, if one has checked and contradicted the thoughts which arise from one's first observation. Any objective examination, when duly verified, refutes the results of the first contact with the object. To start with, everything must be called into question: sensation, common sense, usage however constant, even etymology, for words, which are made for singing and enchanting, rarely make contact with thought. Far from marveling at the object, objective thought must treat it ironically." This quote may strike one as odd. Are not the experiences of the object valid? Does Science, in some grand sense, hold a monopoly on objective reality, and is that dependent on some divorce from what is seductive, enchanting? What I have called strategic unknowing might also be connected with Bachelard's ironic treatment of the object, but this irony is kept in mind by practitioners as a technique, not a revelation. By failing to recognize something— here, a mosquito— mathematical formalisms are believed by participants to offer potential new insights inaccessible by expert knowledge like Anton's. By failing to recognize the body, the dynamics can be made visible. A computer does not recognize anything. For many modelers, this inability is a strategic strength: a point of view agnostic about meaning can sometimes shuffle, regroup, or dissolve data in ways that can stimulate people to put them back together again with a new kind of a story. Lack of recognition here does not move the researchers further from their object of research, because that object is not bugs, per se. Nor is it only the genes they see inside of those bodies when they pulverize them, push them through a centrifuge, and translate them to code for the computer to read. Rather, it is the patterns of changes those codes undergo as they interact with each other.

Just as the object of the trial is not "bug", neither is the field the island, exactly. The island as it exists today is sought as important data for the trial. This is in similar ways and for similar reasons that information about local mosquitoes is pursued; often, the two projects are inextricable in practice, as when researchers must construct topographies to add to their stories about genetic structure in island populations. But just as the "real" bugs do not hold a greater claim to the heart of the trial than their airy data-fied selves, the island sites in which the team is interested are not important in all their multifarious reality. Only the elements of the islands which are hypothesized to be significant to the dynamics of mosquito population genetics are included in models of the places. This is because, just as the real insects at the level of the body are not focused on as the object of the trial, neither is the purpose of the trial the island nations themselves per se. The researchers are likewise used by their "in-country partners" to pursue various ends, like increasing tourism by eliminating malaria. The UCIMI team selected Sao

Tome and Principe and The Union of the Comoros from long lists of African islands, organized by their possession of various qualities the group believed would make a good test site. Foremost among these, we have seen, were genetic isolation, population structure, malaria, and *Anopheles gambiae*. The point of the trial is not to make any one of these islands different; that is a weigh-station on the road to continental malaria elimination the group pursues. We have seen how this logic is organized by a scalar form of thinking which moves from the laboratory to the island to the continent while maintaining the dynamics. The purpose of the islands within this project is their *transformation into working models themselves.* At the planning stages, qualities or elements of the islands, like topography, weather, temperature, rainfall, and species distribution are rendered as graphs or models on various Californian computers, but these projects are in service of what they think of as a larger scale of modeling effort. The models will flee the computers, they hope, and take root in the land. The island site eventually chosen for release of the genetically modified mosquitoes is to serve, they plan, as a working model, constructed this time not out of data and bytes but out of the material stuff of the living earth. Bounded by the ocean, the island the researchers already conceptualize as a closed system becomes a mechanism for registering change to mosquito populations. Of course, none of this change is registered by looking feeling, or being in the field alone; it rather becomes visible as it is translated back into data which are visualized as trends, maps, and forecasts. But even though the working model of the island will require these additional tools to fully *use* the island itself becomes a working model for them as soon as it has been comprehensively recorded in the form of "baseline data", or information about the place, its mosquitoes, and its people as they already are. Like a children's water table, it can then register the flows that move through it. Once transformed into a working model, the island is not envisioned as working *like* the laboratory— it is useful because the mosquitoes there are wild in their genetics, because it has topography and plants and animals and people busy luring mosquitoes and warding them away. These factors are seen to be qualitatively unlike the laboratory, as the wild-type mosquitoes are unlike their laboratory organism kin. These unlikenesses are useful, insofar as they reflect qualities believed also to exist in continental Africa. The island is seen as the real world, but simpler. It is more complex than the laboratory— there are more factors to account for in the working of the model— but it is framed as ultimately knowable, at least on the parameters that matter to the UCIMI. Modeling in this work is thus double layered: the Berkeley lab (as well as the VGL) create models of island sites, constructed out of data collected by entomologists, satellites, or maps, which they then use to make of the island a physical working model that they then translate back into computer models for surveillance. The island as a physical place, with a history and a smell is integrated, but only along the way. It is a means to an end, another source of data to feed the models that make dynamics. Transforming islands is only the goal of the group if we truncate their practices of scaling halfway through.

Conclusion

Doing fieldwork for researchers affiliated with the UCIMI project means going somewhere. Attendant on this going is a whole set of different work practices, which are contrasted with the ways of the laboratory. Researchers see these practices as an opening outward to a world of potentially infinite complexity. Unlike the laboratory, these complexities are not controlled or predictable. They make choices about what to account for and how against a background assumption of overwhelming excess. This is true of mosquito genetics. As we have seen in Chapter Two, data from the field increasingly persuaded scientists that standing genetic variation in wild mosquito populations is widespread but varied enough to permanently elude any project of accounting for it in advance. This figures the wild as fundamentally unknowable. This does not mean, however, that it cannot be worked with. The genetic variability of wild type mosquitoes is a potential threat to a project which envisions one central idea scalable as far down as the cage and as far up as the continent, but it is also the biggest asset of a techniques which works by co-opting local mosquito biologies to spread the malaria refractory gene.

From the vantage point of the laboratory, a narrative of progression from insectary cage to continental Africa makes intuitive sense, a natural unfolding of a basic idea embedded in the body of the engineered mosquito. Fieldwork activities, from this central perspective, are focused around clearing away obstacles for this progression, and facilitating it by doing things framed as accessories to the primary task at hand, like securing an island experimental site, generating baseline data about mosquito genetics, and setting up the infrastructure for future phases of the project. This narrative of work is true, but it also runs at an angle with the actual practices of fieldwork. The field, by which my interlocutors mean wild-type mosquitoes, landscapes, and everything in between, does strange and surprising things. In accounting for the specificities of the local, they also produce a different image not only of the current project but of its future. The core of this new image is not the idea embedded in the body of the mosquito which gradually expands to cover the globe. It is rather the dynamics, or patterns of ongoing change, within populations. Not particularly natural to begin with, the dynamics of mosquitoes in their two field sites can be read also as stories about deep geological time, about human histories, and about mosquito learning and adaptation. The aim of the project, from the field, is to create a new dynamic, or a new pattern of change, with the engineered genes and the gene drive which changes the way they are inherited. Here, it is a matter of constant mutable interweavings between laboratory and field that matters, rather than the implementation of a fixed program. A recognition of local variation and complexity emerges in a variegated tension with the scalar goals of the trial which, nevertheless, does not enter into direct conflict with it. The universal persists, but as just one among many elements of the field. Real mosquitoes, people, and places of this stage of the trial figure more prominently in field work, in which researchers are often actually facing them. These real places, and especially the mosquitoes which inhabit them are, however, not the primary aim of fieldwork. Deep understanding is in the service of better-honed dynamics which, in turn, can be applied to predictions about a scaled-up future. We see thus constant motion between perspectives on the mosquitoes—from local mosquitoes to informational mosquitoes, from bodies here and now to augurs of the future--- which collectively feed into a notion of patterned change that is itself in flux.

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