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Commons, Enclosures, Complexity

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Abstract

Complexity science promises to aid our understanding of nonlinear social and ecological processes, and its methods—such as simulation—can invigorate longstanding philosophical debates about modeling in geography.

Computer simulation/modeling may prove an indispensable supplement to interpretive methods such as ethnography and historical studies for understanding social processes as complex systems. Chapter One uses agent-based modeling (ABM) as a supplement to ethnography of an indigenous group that lost their common property system to capitalist enclosure. It also surveys the philosophical context of critical/quantitative dualism, shows how "critical GIS" literature has intervened, and proposes "critical ABM" as an extension to help bridge that dualism.

Chapter Two explores how the recognition of complexity in the discipline of biology has overthrown genetic determinism in favor of a comprehension of causality that propagates over many scales from the molecular to the environmental, and this, in turn, has provoked new possibilities for social theory. A myriad of developments in biology are encompassed in this recognition, from genomics and epigenomics to niche-construction, as well as Eldredge's evolutionary hierarchy theory and lively debates about the biological context of human sociality. Gene-culture coevolution is spawning a new transdisciplinary "behavioral sciences" paradigm; punctuated equilibria has overturned gradualism in evolutionary biology, which is spurring a "dissipative social systems evolution" paradigm which integrates chaos theory and ideas from Marx. I argue these latter paradigms can be seen as antipodes of complexity science which are ultimately complementary.

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Introduction

This thesis engages two interrelated themes reflected by two chapters, each of which has rather different scope. The first is about the use of computer simulation as a supplement to ethnography of an indigenous group that lost their common property system to capitalist enclosure. The second is an exploration of disparate strands of complexity science,[1] for which both ethnography and simulation are in the same category of methodologies that are needed to comprehend social processes as complex systems. That chapter entails a wild ride through current debates about the biology of human sociality, the repudiation of gradualism by recent paleontology and evolutionary biology and its implications for social theory, and other metatheoretical concerns. The two chapters may seem incongruous together, but they interpenetrate and, I hope, reciprocally illuminate each other.

Chapter One, "Experimenting with the Emergence of Capitalist Relations: Toward a 'Critical ABM'" revolves around the Lauje highlanders, an indigenous people of Sulawesi, Indonesia. They had been practicing a sustainable form of swidden agriculture in a system of shared access to common land stretching back countless generations (Li, 2014, 15)—until 1990 when a cacao tree-planting boom disrupted this tradition. Also known as shifting agriculture or slash and burn, swidden is a form of agriculture which typically uses forest clearings for three years and then lets them fallow. There is a very long history—going back to the earliest known states—of swidden cultivation enabling communities to be invisible to government control

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[1] Complexity science, in brief, is a plurality of approaches to investigating processes that involve heterogeneity of agents, interaction, and system sizes where the activity of agents and their interaction cause organized complexity, with emergent patterns for which statistical explanation is inadequate (O'Sullivan et al., 2012). These kinds of processes had resisted conventional scientific understanding until recent decades.

(Downey, 2020, 423; Scott, 2009).[2] Furthermore, the issue of swidden remains very much alive: it was long maligned by colonial and development officials as ecologically destructive (Food and Agriculture Organization, 1985; Robbins, 2012, 42) but its sustainability has been defended and hotly debated (Balée, 2013; Conklin, 1954; Geertz, 1963; Mertz, Padoch, et al., 2009; Vliet et al., 2013). Furthermore, it is estimated that 200—500 million people continue to rely on it (Downey, 2020, 423; Mertz et al., 2009).

The loss of their common property system, and the associated emergence of capitalist relations—defined as a shift from people experiencing participation in markets as opportunity to a coercive necessity for survival, driving them into dynamics of competition around productivity—was devastating to most Lauje highlanders. Tania Murray Li's (2014) ethnography of this process is immensely valuable. It provides a glimpse into a kind of phenomenon that cannot be directly tested experimentally. Even though the Lauje's case may well be anomalous—coercion was *not* also the key initiating mechanism involved in this emergence of capitalist relations—there are echoes of its dynamics going back to the enclosures of eighteenth century England (Li, 2014, 114). As for other accounts of the emergence of capitalist relations, for example Patel & Moore (2017), the data is almost exclusively historical and mostly from the relatively distant past. It is for this reason that the "in silica" experiments of computer simulation may prove indispensable for our understanding.

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[2] A lessor-known case is the Accompong community, founded by escaped slaves in sixteenth-century Jamaica. Rob Connell (2017, 79) wrote about parallels between the process of marronage in southeast Asia that Scott (2009) writes about and that process in Jamaica, but does not discuss the practice of swidden agriculture in Accompong. There is one mention of swidden in Accompong being documented from the seventeenth century in Barker and Spence (1988, 200). However, Spence (2022) told me that to this day swidden is practiced in Accompong and the community continues to own land communally. Furthermore, he said that the swidden practiced there is very similar to what he observed in the Jeneberang watershed area of south Sulawesi, Indonesia in 1978, while he attended Hasanuddin University in Ujung Pandang.

Chapter Two, “Complexity Evolving: Norms, Games, Space,” is an extended discussion of active areas of research in complexity science that warrant the attention of geographers. Specifically, this includes the transdisciplinary "behavioral sciences" paradigm proposed by Herbert Gintis (2007, 2014), and secondly, the "dissipative social systems evolution" approach of David L. Harvey and Michael H. Reed (1997, 1994), which synthesizes Marxian ideas with complexity theory.

Both utilize new developments in biology: gene-culture coevolution in the case of Gintis, and punctuated equilibria/evolutionary hierarchy theory in Harvey and Reed. Both strongly resonate with the call by Stallins (2012) for geographers to engage with "new organism-environment interaction" in biology as a way to contribute to key debates in geography about scale and environment-mediated causality. At stake in these debates are fundamental challenges to our conceptions of the biological context of human sociality and culture, with striking implications for how we think about the possibilities of human cooperation and social change.

Pertinent implications for the dynamics of capitalist relations, commons and enclosures pop up again in this second chapter. But the focus here is more on the implications for geographers and political ecologists in general.

Finally, there is a concluding chapter. With its examination of the epistemology, history, and politics of modeling in geography; its computer simulations of the dynamics of commons, enclosures, and capitalist relations; its elucidation and integration of relevant developments in complexity science, it is hoped this thesis will help advance the incipient theory and practice of simulation and complexity science in geography.

Chapter One

Experimenting with the Emergence of Capitalist Relations: Toward a ‘Critical ABM’

Introduction to Chapter One

When mentioning the devastating emergence of capitalist relations among the Lauje highlanders, above, I made the claim that computer simulation may prove indispensable for our understanding—in supplement to interpretive methods such as ethnographic and historical studies. Practitioners of those interpretive methods may bristle at this idea, as such collaborative supplementation is not yet common, and has been known to be fraught with tension (Yeh, 2016, 37). I will up the ante even further and suggest that simulation may enable us to model how people break out of capitalist relations, forming fugitive self-governed commons (Connell, 2020) and mounting insurrections (Bernes, 2019).

Unfortunately, these claims cannot be backed up by my simulation here, which is available online (Fram, 2022), of the emergence of capitalist relations among the Lauje (Li, 2014). I struggled over the last year to create a simulation model that would give a sophisticated game theoretic treatment of social norms and ecological dynamics around the sustainable common pool resource (CPR) of the Lauje highlanders, as well as its embattled collapse. But time constraints forced me to focus more narrowly on the dynamics of enclosures and the loss of the Lauje's usufruct, common property system circa 1990-2010. My model simulates what happens when highlander groups engage in the cacao tree-planting boom effecting these enclosures, with varying degrees of concentrated capital accumulation, from even development to very concentrated accumulation of wealth. And it also models the (mostly counterfactual, in this case) possibilities of punishment for cacao-farming as an overuse of the CPR of the forest, which might have turned the Lauje's fortunes around.

In any case, my model is a humble beginning toward practical applications that would substantiate the bold claims for simulation above. Mostly what you will find in this chapter is theoretical discussion that points in that direction, and a reflection on the potentials and difficulties of this kind of application. But lest I be accused of pure speculation, let us consider a concrete example of what simulation can contribute when supplementing an interpretive method like ethnography: Stephen J. Lansing and James Kremer's work on the water temples system of irrigation in Bali, Indonesia (Lansing & Kremer, 1993; see also the Netlogo version of Lansing's model and discussion in Janssen, 2020; and an extension of Janssen's version at Lansing, 2022).

Over at least a thousand-year period, a system of elaborate rituals has been used by rice farmers in Bali, in which local, direct-democratic assemblies, organized as water temples, coordinate planting and fallow periods in a way that effectively manages pests and equitably distributes water. In the 1960s, the development plans of the Green Revolution (Perkins, 1997) imposed a regime of hybrid “miracle” varieties of rice, fertilizer and pesticide use, and constant planting of as much rice as possible—not allowing any fallowing. Within a few years the results were disastrous due to pest infestations and water distribution problems. By the 1980s, many rice farmers wanted to return to the water temple system, but the powerful funders of the Green Revolution could not be convinced, and farmers faced fines and even jail time if they did not comply. Lansing and Kremer built a simulation model that effectively demonstrated that the complex system of water temples was far more efficient than the Green Revolution system. They were even able to convince the funders of the Green Revolution to cease imposing their development plans for rice cultivation in Bali (M. Janssen, 2020, 17).

The great merit of Lansing and Kremer's model is that they were able to show that the kind of self-organizing dynamics of the democratic assemblies/water temples—which managed water distribution and pests much better than the Green Revolution strategy—could emerge through the evolution of local interactions without central planning in their model as well. What the water temples were able to achieve could be seen game-theoretically as a sophisticated equilibrium in a modified prisoner's dilemma game.

Lansing and colleagues (Lansing, 2006; Lansing & Cox, 2019) have argued that there are many examples of sustainable, self-organizing, complex socio-ecological systems akin to the water temples system—examples that are invisible to moderns and that are being lost at an alarming rate. I argue that the swidden agriculture system of the Lauje highlanders is one such example.

Chapter One is organized into four sections. The first section is a general discussion of the type of simulation that I will focus on: agent-based modeling (ABM).[3] (See the section "ABM: Background and Methodological Issues," below.) This includes background on complexity science[4], which, as noted above, is a plurality of approaches to investigating

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[3] There are a range of different kinds of computer simulation modeling types and software packages. For a discussion of the sometimes confusing, important choice of which to use, see Downey (2005, 11).

[4] Complexity science has gone by many names. I will refer to it interchangeably as complexity theory or complexity science. Part of what makes complexity science difficult to define is that there are multiple classifications and typologies (S. M. Manson, 2001, 2003; David O'Sullivan, 2004, 283; Williams, 2020, 16). Other terms which are synonyms for complexity science, according to some interpretations, include self-organizing systems theory, the science of non-linear dynamics, deterministic chaos, or simply "chaos." "Such confusion, however, should not obscure the emergence of a new world view that recognizes a dimension of reality which until quite recently was marginalized by conventional science. All these labels have a common referent: i.e., entities that are systematically structured via the reciprocal interactions of their parts, and whose long-term, orderly behavior can destabilize and become unpredictable" (D. L. Harvey & Reed, 1994, page 375). Nigel Thrift (1999, 48-50), has written thoughtfully about complexity theory's confusing three-pronged history, with currency not just in science but also the world of New Age culture and in the world of business management gurus (Thrift, 1999, 42-48). Despite his enthusiasm for it, Thrift (1999, 133) was no doubt correct that "complexity theory is, to an extent, just another business opportunity." The same year he published those words, "a formidable 34 books on complexity and business [were]

processes that involve heterogeneity of agents, interaction, and system sizes where the activity of agents and their interaction cause organized complexity, with emergent patterns for which statistical explanation is inadequate (O'Sullivan et al., 2012). These kinds of processes had resisted conventional scientific understanding until recent decades. It is only now that this kind of work is becoming recognized to the extent that the first Nobel Prize was awarded for it in 2021 (Castelvecchi & Gaind, 2021).[5] ABM is an especially good fit for such investigations. In fact, simulation methods like ABM and complexity science are inextricable (Manson & O'Sullivan, 2006, page 684). Furthermore, even beyond the use of simulation, theoretical developments in complexity science are important and can illuminate our understanding of nonlinear environmental and social systems.

The second section, "Placing ABM in Critical Quantitative Methods," is a historical discussion of the dualism between critical social theory[6] and quantitative methods within geography (Kwan & Schwanen, 2009; Sheppard, 2001). This will help contextualize the absence of ABM in critical geography—and frame its promise here, as ABM is interestingly situated as a new style of quantitative/computational methods, particularly well-suited to bridge the critical/quantitative divide (Bergmann et al., 2009; Millington et al., 2012; O'Sullivan, 2018; O'Sullivan et al., 2018).

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reviewed in a special issue of *Emergence* (Lissack et al. 1999 [cited in O'Sullivan])" (David O'Sullivan, 2004, 292). For an incisive and humorous critique of the latter trend, see Frank (2000).

[5] For a large "collection of research, review and opinion articles that celebrates the direct contributions by the awardees and the discoveries they have inspired," see: https://www.nature.com/collections/ejgjechidj/

[6] I follow Crampton (2010, Pg. 16) and Blomley (2006) here in understanding critical theory and critical geography as encompassing feminist, Marxian, post-structuralist, post-colonial, green, disabled, queer, and other approaches aimed at emancipation and changing the object of research.

The third section, “Splits in the World of ABM” posits a fracture in the theory and practice of ABM both in and outside geography, revolving around choice of topics as well as computer languages.

The fourth section, “Commons and Enclosures,” is a detailed discussion of my application of ABM to the emergence of capitalist relations in the case of Li's (2014) work on the Lauje.

Agent-Based Modeling: Background and Methodological Issues

Agent-Based Modeling (ABM) is a computer simulation method that is quickly becoming a standard modeling technique across many disciplines, with a wide range of uses, from theory-development to hypothesis testing and predictive modeling, as well as “hindcasting” (A. J. Heppenstall et al., 2012; S. Manson et al., 2020; O’Sullivan, 2008; O’Sullivan & Perry, 2013; Parker & Derek, 2017; Torrens, 2010; Wilensky & Rand, 2015). Although ABM has been used in ecology, anthropology, sociology and a variety of subfields in geography (S. Manson et al., 2020; Parker & Derek, 2017; Torrens, 2010), there has been very little work with it in critical geography (but see Bergmann et al., 2009; Haklay et al., 2001; O’Sullivan, 2002), and none yet to my knowledge in political ecology. And this absence is in spite of calls for such experimentation by prominent geographers (Millington et al., 2012; O’Sullivan, 2004, 2018). Given the promise of ABM to aid our understanding of many different kinds of nonlinear environmental and social systems where experimental science is difficult or impossible (O’Sullivan & Perry, 2013, page 16), this absence invites explanation. This section and the next offers such explanation.

Uri Wilensky (creator of the computer language, NetLogo, which is used exclusively for ABM[7]) and Rand define it thus:

> An *agent* is an autonomous computational individual or object with particular properties and actions. *Agent-based modeling* is a form of computational modeling whereby a phenomenon is modeled in terms of agents and their interactions. [It is] the use of multiple interacting, heterogeneous agents to create models of complex systems." (Wilensky & Rand, 2015, Pg. 1, xviii).

One simple yet powerful example to help illustrate this is an ABM of predator-prey relations. It is possible to code simple rules of behavior for "sheep" and "wolf" agents in a model, in such a way that the results closely match those of the famous Lotka-Volterra equation-based model (Wilensky & Rand, 2015, 15). As with the simultaneous equations of differential calculus used in the latter, we get the sinusoidal curves that show cycling of the two populations with predators in ascendance while the prey population troughs, and vice versa. Because predator-prey relationships have been studied empirically and modeled mathematically for many years, the complementary approach of ABM is illuminated. Equation-based modeling approaches must make simplifying assumptions about the homogeneity of populations, treating individuals as average quantities, and it must start by defining global behaviors, without making any mechanisms explicit. ABM, in contrast, starts only with simple rules of heterogenous individuals, making all assumed mechanisms explicit, and the global patterns emerge on their own. In this case, even the math comes out more accurately in predicting extinction rates with the ABM method. This is because it avoids the "nano wolf problem," where fractional

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[7] Note that any computer language can be used for ABM, but some, like Netlogo, are especially designed for it.

populations can make a comeback, which has been shown to cause underrepresentation of extinctions with the equation-based model (Wilensky & Rand, 2015, Pg. 17).

The concept of emergence is central to complex systems theory, to which ABM is closely tied.[8] Wilensky and Rand define emergence as:

> *The arising of novel and coherent structures, patterns, and properties through the interactions of multiple distributed elements*. Emergent structures cannot be deduced solely from the properties of the elements, but rather, also arise from interactions of the elements. Such emergent structures are system properties yet they often feedback to the very individual elements of which they are composed (Wilensky & Rand, 2015, Pg. 6).

The absence of a centralized coordinator of global patterns is an important aspect of emergent structures. This idea of order without an intelligent designer has been shown to be counterintuitive and difficult for people (including scientists) to grasp (Wilensky & Resnick, 1999). This difficulty persists despite the ubiquity of emergent structures, as has been demonstrated, in part by using ABM, in such phenomena as murmuration flying patterns of birds and optimization of food gathering by ants, and many other examples from physical and social sciences, as well as thought experiments like "cellular automata" (O'Sullivan & Perry, 2013, page 18).

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[8] "Although complexity researchers have chosen computational modeling as a tool of choice, in some ways the models created complexity research: the rise of complexity theory is due in no small part to the broad availability of increasingly powerful computers, programming languages, and frameworks such as object-oriented modeling" (Manson & O'Sullivan, 2006, page 684).

Unlike an ABM of predator-prey relations, or gas molecules colliding, for many phenomena the rules of behavior are not well known. We may nevertheless have some idea of the target global behavior for which we are trying to find rules. In this case, experimenting with rules and properties can lead to a better understanding of phenomena.

This is probably more or less the case with using ABM for any social science phenomena when the representational infrastructure primarily consists of words and texts. As Wilensky and Rand put it:

> words and texts are not dynamic representations, so they cannot give you immediate feedback as to the consequences of the assumptions embedded in them. By capturing social science theories in dynamic ABM representations, we make their assumptions explicit, and they become demonstrations of the consequences of their assumptions. If someone wants to disagree with your model, he or she must show how either an assumption is incorrect or missing or show how the logic of the interactions is flawed. The model serves as an object-to-think-with and a test bed for alternate assumptions…ABMs serve as powerful complements to text-based explanations (Wilensky & Rand, 2015, Pg. 20).

This brings up the issues of verification and validation. Verification is comparing an implemented ABM to its associated conceptual model to make sure the ABM is faithful to it. Validation is comparing an ABM to a real-world phenomenon to see if it yields insights into that phenomenon. There is much debate as to the validity of model validation. Naomi Oreskes (discussed in O'Sullivan, 2004, page 280; Oreskes et al., 1994) has argued that validation is flatly impossible. This is due to the "equifinality problem"—many different models may account for the same results. One response to this problem is the pragmatic answer that "a valid model is one that is useful," and much depends on the purpose of a model (O'Sullivan, 2004, page 290). It is worth admitting frankly, as Sterman (2002) argues, that "all models are wrong," (first attributed to Box, 1976), though some are useful (discussed in O'Sullivan, 2004, page 290). This is "akin to the realization in post-structural social science that multiple competing accounts of the same settings are possible, and that faced with a diversity of accounts the context and intent of each must be an important element in the evaluation process […] This is unfortunately not how most work is currently evaluated. Frequently, the presentation of a model is overwhelmingly technical" (O'Sullivan, 2004, page 291).

The deductive-nomological model of explanation that dominated twentieth-century science, where universal laws were sought through deductive reasoning, is inappropriate with ABM. Yet much analysis and interpretation of ABM is presented in a similar style, using aggregate statistical analysis, obtained by a "parameter sweep," whereby multiple runs of a model are automated to explore the full range of behaviors exhibited with different starting parameters (Wilensky & Rand, 2015). It has been suggested that while these statistical analyses may be useful, narrative approaches, where "modelers become 'makers' of stories" deserves much more experimentation (S. Manson & O'Sullivan, 2006; Millington et al., 2012; O'Sullivan, 2004).

Suppose we admit that all models are wrong, that validation of ABM is not possible. Even more, following O'Sullivan (2004, page 288), let us agree that there is "a serious problem for the prospect of learning about the world from models. A claim to be doing experimental science using models as 'computational laboratories' is all very well, but it must be acknowledged that the experimental subjects are models, not the world itself.” Then what business have I making strong knowledge claims, as above, that ABM represents extinction rates more accurately than Lotka-Volterra, for example? And why might it be a good idea to model the emergence of capitalist relations among the Lauje with it? Computational modeling such as ABM brings critical epistemological questions about representation to the fore. But these questions apply, much more equally than is widely recognized, to any form of representation, including equation-based models like Lotka-Volterra’s, cartography, GIS (Geographic Information Systems), and text-based conceptual models. It is ironic, as Wilensky and Rand (2015, page 17) have pointed out, that many people tend to accept, at face value, a model with simultaneous differential equations (even though it is indeed a model, and a significantly simplified one at that), but they tend to be much more skeptical and critical of computer-simulated representations—even though it can be demonstrated that the latter are more rigorous, as in the Lotka-Volterra case.[9]

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[9] It is worth quoting McElreath and Boyd (2007, page 8) at length for a contrasting perspective: “There is a growing number of modelers who know very little about analytic methods. Instead, these researchers focus on computer simulations of complex systems…it [is] tempting to give up on analytic methods, since most people find them difficult to learn and to understand. There are several reasons why simulations are poor substitutes for analytic models. Equations…provide intuitions about the reasons an evolutionary system behaves as it does…Analytic models…tell us things that we must infer, often with great difficulty, from simulation results. Analytic models can provide proofs, while simulations provide only a collection of examples…It is difficult to explore the sensitivity of simulations to changes in parameter values…When simple analytic methods can produce the same results, simulation should be avoided, both for economy and sanity. Computer programs are hard to communicate and verify. There is as yet no standard way to communicate the structure of a simulation, especially a complicated “agent-based” simulation, in which each organism or other entity is kept track of independently…In contrast, analytic models have benefitted from generations of notational standardization…It is much easier for other researchers to verify and reproduce modeling results in the analytic case…The apparent ease of simulation often tempts the modeler to put in every variable which might matter, leading to complicated and uninterpretable models of an already complicated world…We had a world we didn’t understand and now we have added a model we don’t understand. If the temptation to over-specify is resisted, however, simulation and analytic methods complement each other. Each is probably most useful when practiced alongside the other. There are plenty of important problems for which it is simply impossible to derive analytic results. In these cases, simulation is the only solution. And many important analytic models can be specified entirely as mathematical expressions but cannot be

Part of the problem with evaluating ABM as a methodology is that it has so many varied applications. And very different epistemological claims are made on its behalf by researchers in different fields. There are "what if" applications of ABM that make no claim to represent the known empirical world at all (O'Sullivan, 2004; Wilensky & Rand, 2015). And there are applications on the other side of the spectrum, as we have seen, that make very strong representational claims for ABM. Most applications of ABM make claims that are somewhere in the fuzzy middle of this spectrum, because ABM mostly attracts modelers of complex systems with interacting, heterogenous actors, which are known to be intrinsically unpredictable to some degree, non-deterministic, and path-dependent.

I will argue, shortly, that this methodology may be a good mesh for investigating the non-deterministic, historically contingent nature of capitalist relations, because they exhibit emergent properties that result from contradictory, heterogenous, interacting agents in struggle. But first I will address the question, why has so little of this kind of work been done, applying ABM to Marxian topics, or to critical social theory more generally, or to political ecology? This requires a discussion of the history of controversy around quantitative methods more generally within the discipline of geography.

Placing ABM in Critical Quantitative Methods

It has been argued that there is a false dichotomy, or dualism, between "critical geography"—which, as noted above, encompasses the gamut of approaches aimed at

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solved, except numerically…For these reasons, we would prefer formal and simulation models be learned side by side."

emancipation and changing the object of research (Blomley, 2006; Crampton, 2010, Pg. 16)—and quantification (Kwan & Schwanen, 2009; Sheppard, 2001). According to these theorists, quantification has been gendered and raced, and pedagogy around quantitative methods has been discriminatory against women, non-white people, and generally against people who are not "on the professional career ladder, […those] deemed to be outside the centers of calculation of contemporary society" (Sheppard, 2001). Additionally, quantitative methods have been equated with positivism by both critics and positivist proponents, in a way that misrepresents the diversity of quantitative approaches and perpetuates critical/quantitative dualism (Sheppard, 2001).

Quantitative methods have had a tumultuous history in geography. The "quantitative revolution" of the 1950s–1960s culminated with one of its main figures, David Harvey (1972), rejecting the paradigm and embracing Marxism. Quantitative geography was equated with positivism and linked with neoclassical economics (Massey, 1973; Sayer, 1976), and mathematics was widely rejected in Marxian (and more generally, human) geography, with some important exceptions, such as Eric Sheppard, who has also discussed this history (Sheppard, 2001). In some ways this episode recapitulated what happened when the Frankfurt School founders emerged from heated debates with the Vienna Circle of logical positivists in the early 1930s, developing a pivotal form of "Western Marxism," with a strong vein of antipathy to mathematics. To them mathematics was the key language of instrumental reason, antithetical to Critical Social Theory, with a strict, uncritical adherence to empirical facts that bolstered the status quo (Handelman, 2019).

When Geographic Information Systems (GIS) were ascendent in the 1990s, human geographers went on the attack (Pickles, 1995). These early attacks have been criticized as kneejerk or “unexceptional” (David O’Sullivan, 2006, 784), but they eventually led to more nuanced debate and to the emergence of the whole subfield of “critical GIS,” with a large body of literature and practices (Kwan & Schwanen, 2009; O’Sullivan, 2006; Sheppard, 1995; J. Thatcher et al., 2016; J. E. Thatcher et al., 2018), which has gone some distance toward bridging the “critical/quantitative” divide in geography.

Indeed, critical GIS—overlapping and connected with a “critical quantitative geographies” discourse (Kwan & Schwanen, 2009; O’Sullivan, 2006; J. Thatcher et al., 2016; J. E. Thatcher et al., 2018)— is probably the most significant bridging of the critical/quantitative divide that has occurred both within and beyond the discipline of geography. There continue to be outgrowths from this phenomena, for example the panel discussion on “Critical GeoAI” (geographic artificial intelligence) at the American Association of Geographers conference in 2021 (Crampton, 2020). The new subdiscipline of critical physical geography is also very much aligned philosophically with this bridging of the critical/quantitative divide (Lave et al., 2018).

There are two components to critical GIS[10]: first, getting one’s “hands dirty,” putting GIS to different uses than those for which it was intended. Like analytical computing paradigms generally, GIS was largely engineered to serve capital accumulation and state power (J. Thatcher et al., 2016, 819). But we have seen it can be “hacked” for uses in critical social science, as well as for political interventions toward social justice. A second component of

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[10] For overviews, see especially O’Sullivan (2006) and Thatcher et al. (2016, 2018).

critical GIS is a sustained, critical, collective inquiry into the political-economic and social history of GIS itself—interrogating the reciprocal shaping of GIS by the social and institutional forces that created it, and in turn the reshaping of the social and institutional landscape that has occurred because of GIS.[11]

It is here that I envision a "critical ABM" fitting in with currents of other scholarship. There are many parallels between GIS and ABM. First, as with GIS, there is a need for a sustained, critical, collective inquiry into the politics, economics, and social history of ABM itself. Secondly, there are opportunities for using ABM in critical social science and toward interventions for social justice and challenging the status quo.

Critical ABM seems like an obvious and natural extension to critical GIS, which has been fairly widely discussed, and ABM is an exploding, closely allied field to GIS, literally connected and synergizing together (O'Sullivan & Perry, 2013; Wilensky & Rand, 2015). David O'Sullivan, a prolific proponent of critical GIS and critical-quantification[12] (O'Sullivan & Manson, 2015; O'Sullivan, 2006; J. Thatcher et al., 2016; J. E. Thatcher et al., 2018), has also been a prolific proponent of ABM (S. Manson et al., 2020; O'Sullivan et al., 2012; O'Sullivan & Perry, 2013;

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[11] As several scholars have noted (David O'Sullivan, 2006; J. Thatcher et al., 2016, 819), charting the political economy and social history of GIS remains partial and incomplete. Nevertheless, seen from a broad perspective of scholarship that has called for this kind of work on technology more generally (Feenberg, 1999), this component of critical GIS remains some of the most impressive (Clarke & Cloud, 2000; C. M. Dalton & Thatcher, 2015; C. Dalton & Thatcher, 2014; M. M. Haklay, 2013; McHaffie, 2002; Sheppard, 1995; Sui, 2008; J. Thatcher, 2013; M. W. Wilson, 2012). For critical social history of culture and political-economy around computers more generally, see Turner (2006) and Worden (2012).

[12] It is notable that O'Sullivan co-authored one of the main textbooks (Sullivan & Unwin, 2010) used in the UC Davis graduate course in quantitative methods in geography, as well as the sole textbook on ABM in geography (David O'Sullivan & Perry, 2013).

Sullivan, 2009). O'Sullivan has stated that he thought critical ABM sounded like a good idea, but that he had not previously heard of such a thing (O'Sullivan, 2021).

In fact, although the term "critical ABM" may not have yet appeared, this work has already begun. Nigel Thrift has written enthusiastically about the potential for complexity theory (and its generative modeling, which includes ABM) to synergize with human geography, but also, as noted above, he has tempered this enthusiasm with warnings, for example, "complexity theory is, to an extent, just another business opportunity" (Thrift, 1999, 33). And David O'Sullivan and others have maintained a sustained critical inquiry about ABM in a series of papers (S. Manson & O'Sullivan, 2006; Millington et al., 2012; O'Sullivan & Haklay, 2000; O'Sullivan & Manson, 2015; O'Sullivan, 2002, 2004, 2008; O'Sullivan et al., 2012; O'Sullivan, 2018).

We have arrived at an answer to the question, why has so little of this proposed kind of work been done, applying ABM to Marxian topics, to critical social theory more generally, or to political ecology in particular. This absence appears to be an artifact of the tumultuous history of critical/quantitative dualism, both within geography and beyond. I will soon turn to elaborating on why it makes sense to pursue this absence as an opportunity. But first a brief extension to this discussion on divisions within the field of ABM itself is in order.

Splits in the World of ABM

ABM is part of a family of computational approaches, taught alongside the "Big Data" approaches of artificial intelligence, machine learning, social network analysis, and data mining.

But in some ways it is the black sheep of this family, not as favored (O'Sullivan, 2018, page 28).[13] When it was taught as a component of a computational social science class on this whole family of methods, which I took at UC Davis in Fall 2020, very few students chose to work with ABM on their final projects, despite much encouragement, and professor Hilbert said this was the usual pattern (Hilbert, 2020). This may be because the other methods in the family are more like inferential statistics on steroids, and are generally much more useful for making money—whereas ABM generally has a more humble approach to prediction, and much more emphasis on explanation and understanding (O'Sullivan, 2018). This reflects a longstanding dualism in the deployment of computers in the sciences, where ABM is on the side that emphasizes emergence and is often associated with complexity science (O'Sullivan, 2004, 2018). O'Sullivan (2018, 31) writes, "while it is tempting to suggest that complexity-oriented, bottom-up modeling is inexorably associated with antiauthoritarian and more open approaches to knowledge, while Big Data, top-down, classificatory and inferential statistical approaches are aligned with powerful interests, it is demonstrably untrue…Closed-form calculation might be used to optimize the efficient production and equitable distribution of medical or other public services, while simulation can be (and almost certainly has been) used to explore possible strategies for the illegal invasion and occupation of another country."

There appears to be a discernible split in the culture of ABM within the discipline of geography, partly reflected in the choice of computer languages. Torrens (2010) wrote the last major survey of ABM in geography. But this paper was quite skewed toward the "spatial

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[13] This is true both in general, and specifically in the history of computational approaches within geography, where, according to Nigel Thrift, there was a "ghettoizing of complexity theory" and simulation-oriented methods *within quantitative* geography in the 1960s (discussed in David O'Sullivan, 2017, page 29; Thrift, 1999).

sciences” and did not consider much human geography. For example, Torrens did not cite O’Sullivan’s 2008 paper, “Geographical Information Science: Agent-Based Models.” Torrens only has one mention of Wilensky, who created Netlogo, the popular computer language used by hundreds of thousands of people to build ABMs (Wilensky & Rand, 2015). It would not be surprising if Torrens does not want to be associated with Wilensky or Netlogo. There are other computer languages used for ABM that have an air of being serious and scientific, whereas users of these latter languages often attack NetLogo, with such barbs as “using NetLogo signals that you don't know how to do real programming” (Abe, 2012).

Wilensky is a major proponent of ABMs in the tradition of Seymour Papert, who was a protégé of Piaget, in child psychology, and developed some of the earliest ABMs for children to learn with. There are many people who find NetLogo embarrassing and post screeds on the internet about how “NetLogo is not a real computer language” (Benjdavies, 2014). NetLogo uses terms like “turtles” generically, for agents, part of the legacy of its coming from an “environment of play,” used for teaching children. Moreover, Wilensky’s whole philosophy with the NetLogo language, of “low threshold, high ceiling,” is all about making ABM very accessible, yet capable of advanced work. Wilensky emphasizes that ABM bypasses the “gatekeeping” effect that mathematics has tended to have in the modern world, for example, in his treatment of the Lotka-Volterra model, discussed above.

In contrast to Torrens (2010), David O’Sullivan and George Perry (2013), wrote the only available textbook on ABM specific to geography for use with NetLogo. Furthermore, O’Sullivan has discussed the twisted history of the relationship between complexity science, ABM, and human geography in several papers, which were ignored by Torrens (S. Manson & O’Sullivan, 2006; O’Sullivan, 2004, 2008; Sullivan, 2009).[14]

If it is true, as I have conjectured, that there is a split in the world of ABM applications in geography, reflected in the choice of computer languages as well as topics, then it is probably a messy fracture with splinters of each half on both sides. Note that Wilensky is on a veritable crusade, arguing that widespread ABM literacy will lead to a “restructuration” (Wilensky & Rand, 2015, 4; Wilensky & Resnick, 1999) of knowledge that will nullify the gate-keeping function of mathematics in the modern world, radically democratizing access to understanding of complex systems subjects to all people and at much younger ages. I both appreciate this sentiment and see how it could put off those who might see it as naïve or even undesirable to challenge the status quo in this way. The aspirations of Wilensky’s project in this respect also strongly align with that of the proponents of critical quantitative geographies (Kwan & Schwanen, 2009; Sheppard, 2001). In this sense, the split in the world of ABM scholarship may mirror splits around critical/quantitative dualism. As O’Sullivan (2018) and Thrift (1999) have written, we must resist the temptation to think that ABM is intrinsically antiauthoritarian, or that it will necessarily bring about a future of openness. Nevertheless, the enthusiasm of ABM proponents like Wilensky is understandable because ABM does hold interesting promises.

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[14] Note that only O’Sullivan papers that might have been commented upon in Torrens’s 2010 review were included in the last citation. There are several other papers he has authored or co-authored in this realm since then (S. Manson et al., 2020; Millington et al., 2012; O’Sullivan & Manson, 2015; O’Sullivan et al., 2012; O’Sullivan, 2018). Also note that two papers that O’Sullivan authored or co-authored *were* discussed in Torren’s review (M. Haklay et al., 2001; O’Sullivan, 2002).

Now that I have introduced the methodological issues around ABM, discussed the problem of critical/quantitative dualism, noted new efforts to bridge that divide with critical GIS and critical quantitative geographies, and contextualized the split strands of ABM research in geography, I hope the significant absence of a critical ABM is clear. Now it is time to address the problem of the emergence of capitalist relations among the Lauje highlanders.

Commons and Enclosures

To reiterate the argument about the Lauje as I have developed it thus far, the Lauje highlanders were practicing a sustainable form of swidden agriculture with a system of common property that ensured access to land up until the 1980s, when a boom in planting cacao trees disrupted this tradition. The reason cacao was so disruptive was that the permanence of this crop effected a shift from temporary use of clearings to permanent private property.[15] As competitive farmers accumulated land, others were deprived of the free access to this resource which had enabled them to survive on food that they grew for themselves. They were forced to sell their labor, and thus with a form of self-initiated enclosures, capitalist relations had taken root.

My ABM models what happens when highlander groups engage in this boom, with varying degrees of concentrated capital accumulation, from even development to very concentrated accumulation of wealth. And it also models the (mostly counterfactual, in this

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[15] Highlanders had begun growing tobacco in recent centuries. This crop was grown for market exchange, and did cause some soil degradation, but did not lead to private property enclosures (Li, 2014, 25).

case) possibilities of punishment for cacao-farming as an overuse of the CPR of the forest, which might have turned the Lauje's fortunes around.

In this section I will give background information on the Lauje highlanders and the way in which capitalist relations emerged among them, and then proceed to discuss my approach to modeling this emergence with ABM, and its limitations. For a more detailed and technical discussion of the ABM, see the appendix. Also, perhaps the best way to understand an ABM is to run it and see for oneself the various results that it produces. My ABM can be downloaded here: https://github.com/FramL/Commons_Enclosures_Complexity

Background on the Lauje Highlanders

The Lauje highlanders remained surprisingly insulated in a remote area of Sulawesi, Indonesia and retained noncapitalist ways of living until the 1990s.[16] Their system of shared access to common land, which stretched back countless generations (Li, 2014, 15), enabled a remarkable degree of autonomy. Li (2014, Pg. 6) sums up that situation and the enclosures that followed:

> All highlanders who wanted to farm had free access to land, and they could survive on the food they grew for themselves. Their autonomy was only slightly modified by state rule and taxation, which were light and incomplete in this rather

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[16] "it wasn't easy [for merchants] to bind [the Lauje highlanders] into extractive relations. So long as highlanders had abundant land to grow their own food, merchants had no reliable means to compel them to sell their labor or their products. Nor could the small chiefdoms that formed on the coast in the period before Dutch colonial rule compel highlanders to pay tribute. When the Dutch colonial administration attempted to impose its control in the Lauje area around 1910, it too lacked the capacity to force highlanders to pay taxes or obey commands. A century later, some highlanders were still not incorporated in the desa administrative system, and desa maps and neighborhood lists were incomplete..." (Li, 2014, 32).

> remote region. The big shift occurred when they started to plant tree crops, which had the effect of making their land into individual property. Initial landownership was unequal and over time, efficient farmers were able to accumulate land and capital, and pay workers to expand their farms and profits. Farmers who failed to compete lost control of their land and were compelled to sell their labor—if they could find someone who wanted to buy it.

Li both builds upon—but also challenges important aspects of—earlier explanations of the mechanisms involved in this kind of transition. For Marx, Brenner, and Wood (discussed in Li, 2014, Pg. 6; Wood, 1998) a shift from markets experienced as an opportunity to markets-as-coercion is the “critical diagnostic” of capitalist relations. Li accepts this, but pushes back against the Marxist emphasis on coercion being also the key initiating mechanism involved—as in the theory of “primitive accumulation” or “accumulation by dispossession” (D. Harvey, 2003). Instead, Li emphasizes the importance of Foucauldian themes of power forming desires and identities (Li, 2014, Pg. 18). She emphasizes that the Lauje had “long been familiar with markets,” selling food, cash crops, and their labor—but they did this only occasionally, when the terms suited them:

> The powers shaping the enclosure of land included threats and coercion, but also desire: a desire to prosper, and to see kin and neighbors prosper as well. Spurred on by this desire, Lauje highlanders did not invoke custom to prevent enclosure or to manage and limit it. E. P. Thompson reports a similar process in England during the eighteenth century. Although many villagers opposed enclosure of common lands, some supported it because they hoped to claim some land for themselves. In so doing, they thought they

> could share in the wealth made possible by more productive land uses. Hope turned to despair when they later lost the few hectares they gained as prices turned against them, a process with echoes in the Lauje highlands (Li, 2014, Pg. 114).

Theoretical Resources Towards Building an ABM of Capitalist Relations Emerging

I find Li's argument compelling and feel that there are important lessons to be learned here. But to be clear, I am *not* claiming that accumulation by dispossession is not of primary importance in the emergence of capitalist relations overall. Nevertheless, if enclosure *can* occur in a self-initiated way without significant coercion involved (and there are shades of self-initiation of enclosures even in more typical, coercive instances), then we need different theoretical tools to comprehend the dynamics. For this, I turned to Elinor Ostrom's conceptual framework to supplement Li's essentially Marxian account.

Using Elinor Ostrom's game theoretic terms,[17] the Lauje highlanders can be said to have achieved a non-tragic equilibrium in a modified prisoner's dilemma game (Ostrom, 1990, 7), with the high level of cooperation in their traditional system. The dynamics of this cooperation forms a "self-governed commons" (Janssen, 2020, 216; Ostrom, 1990), where people have been able to use a common pool resource for multiple generations sustainably. The highlanders thus avoided a "tragedy of the commons" (Blaikie, 1985, 130; Hardin, 1968). And the high payoff of

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[17] Many geographers may be unfamiliar with game theory or hold outdated stereotypes of it (Gintis, 2014, 212). Although a thorough introduction is beyond the scope of this thesis, a discussion of it is presented starting on page 50, below. It will be argued there that game theory warrants our attention. Meanwhile, the reader should keep in mind that in terms of questions of human cooperation, the classic prisoners' dilemma problem got inverted. Instead of proving that cooperation is impossible among self-interested rational actors, as with classical game theory's cold war strategy fame, cultural evolutionary game theory now asks, since cooperation is evident, how does it evolve (Bowles & Gintis, 2011)?

selling cacao can be seen as a temptation to “cheat” on this egalitarian equilibrium, fatally disrupting their control of collective land ownership. This is another way of looking at what Li sees in Marxian terms as capitalist relations taking root, with social life now dominated by market exchange and competition. Ostrom’s game theoretic framework helps explain why this could happen in a way that does not fit the classical Marxian narrative of “primitive accumulation” (Li, 2014, 3). In other words, Ostrom’s framework would not fit very well for analyzing the emergence of capitalist relations where it has been induced with the prodigious amounts of coercion typical in colonial contexts, or in the original enclosures with European peasants (Patel & Moore, 2017). And in general, Ostrom and colleagues do not see their work as intended to explain the emergence of capitalist relations. Ostrom’s framework was designed to address the tragedy of the commons and the averting of tragedy with self-governed commons. But in this anomalous case of the Lauje highlanders, I argue that the emergence of capitalist relations are synonymous with an inverted tragedy of the commons: similar to the latter in terms of environmental degradation, it was a tragedy of privatization.[18]

Using Ostrom’s framework gives us an opportunity to examine some of the different facets of this process, both to make sense of what has happened, and to ask “what if” questions. It enables us to take a fresh look at some of the forces at play when self-governed commons come unraveled, versus staying resilient. For example, what if the Lauje had had greater awareness of the threat the cacao boom posed to their way of life? Might they have

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[18] This does invert Hardin's (1968) original analysis of the tragedy of the commons. For him there are two solutions to the tragedy: the state and the market (private property). For Hardin the emergence of capitalist relations is a response and solution to the tragedy, not the tragedy itself. Note that Hardin later retracted his argument to an extent, however, saying that it should have been called “The Tragedy of the ‘Unmanaged’ Commons” (Fairlie, 2009; Hardin, 1991).

been able to resist the destruction of their self-governed commons, or at least significantly postpone it? Is there any way that what has been lost could even be restored?

Description of the Model

My ABM is based on the “Governing the Commons” model in Marco Janssen’s book, *Introduction to Agent-Based Modeling: with Applications to Social, Ecological, and Social-Ecological Systems* (Janssen, 2020, page 214).[19] Janssen’s model gave a basic ABM of both Hardin’s tragedy of the commons (Hardin, 1968) and how non-tragic outcomes can be found through self-government, if cooperative norms are enforced. What the current model adds to that is an investigation of geographically specific, market-related threats to self-governed commons, and how these threats can be combatted.

The main agents in my model represent groups of highlanders.[20] There is a grid of patches which is initialized upon starting each model run, each cell of which has different amounts of resources which regrow at a logistic rate. Each patch is 3 hectares, roughly the size of a highlander group’s clearing (Li, 2014, 114).The agents move about the patches harvesting resources. Each “tick” represents a few years (the typical amount of time highlanders would stay in a clearing before letting it fallow) (Li, 2014, 88).

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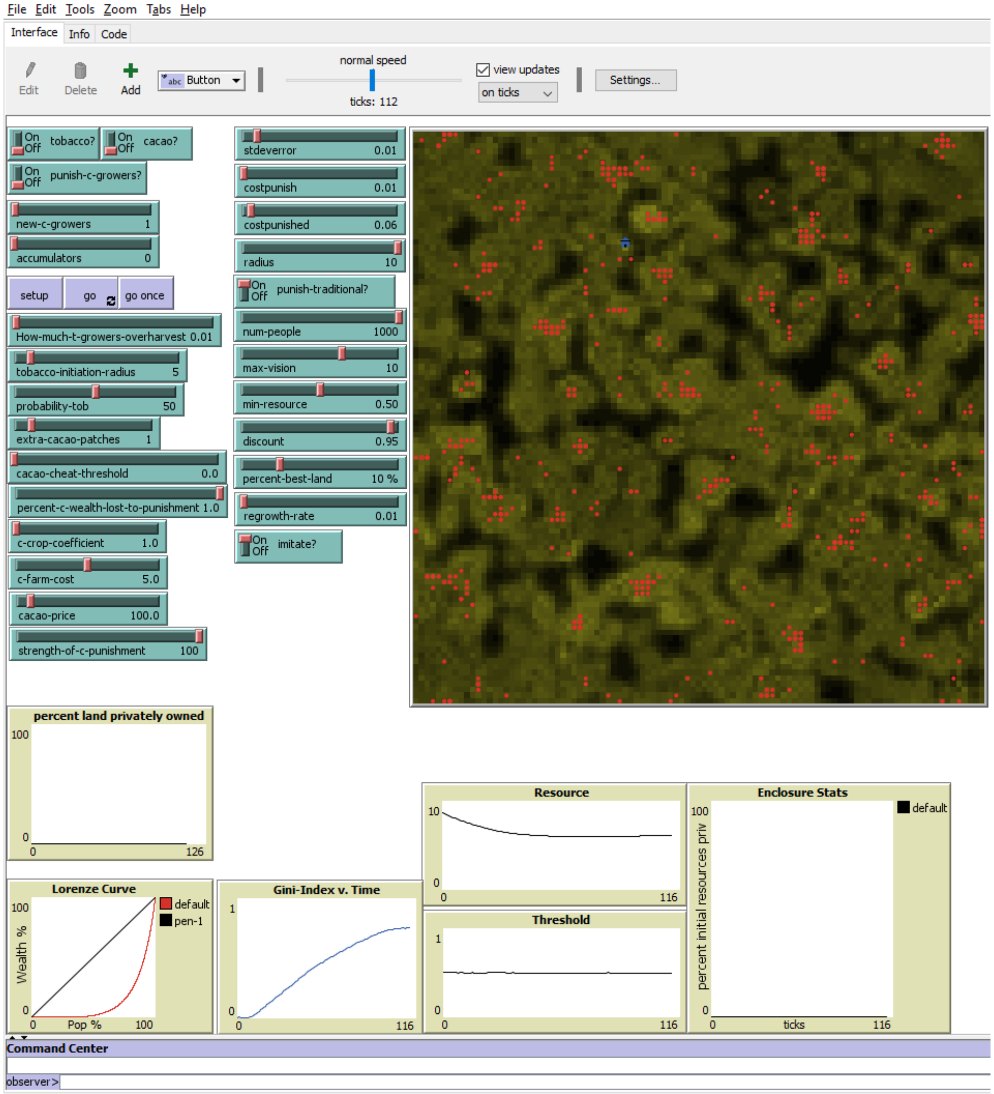
[19] Janssen’s model is in turn based upon the “Wealth Distribution” model by Uri Wilensky (1998), which in turn is built on Sugarscape (Epstein & Axtell, 1996).

[20] Highlanders lived "in transient clusters ranging in size from two to twenty households. Each household, which was generally based on a nuclear family of parents and unmarried children, built a small bamboo house in its current farm plot and shifted its residence when the plot was fallowed after two to five years. The typical pattern was for a few households of close kin to build houses near one another, with some distance—up to a kilometer—between their cluster and the next" (Li, 2014, 23).



normal speed

ticks: 112

☑ view updates

on ticks

On Off tobacco?

On Off cacao?

On Off punish-c-growers?

new-c-growers 1

accumulators 0

How-much-t-growers-overharvest 0.01

tobacco-initiation-radius 5

probability-tob 50

extra-cacao-patches 1

cacao-cheat-threshold 0.0

percent-c-wealth-lost-to-punishment 1.0

c-crop-coefficient 1.0

c-farm-cost 5.0

cacao-price 100.0

strength-of-c-punishment 100

stdeverror 0.01

costpunish 0.01

costpunished 0.06

radius 10

On Off punish-traditional?

num-people 1000

max-vision 10

min-resource 0.50

discount 0.95

percent-best-land 10 %

regrowth-rate 0.01

On Off imitate?

Figure 1--Commons And Enclosures ABM. Highlander groups are represented by red dots. The more resources on a patch, the more yellow it is. There are no tobacco or cacao-growers yet here. This represents a non-market self-governed commons.

There are five important optional parameters that will be highlighted here: the switches labeled “cacao?”, “tobacco?”, “punish-traditional?”, “punish-c-growers?” and a slider, labeled “accumulators”. Tobacco represents a crop that highlanders grew in recent centuries with an intermediate amount of production for market. Cacao-growing, which only started in the 1980s, was produced solely for market.[21]

When the model is run with the default parameters, and with all switches "off" and the "accumulators" slider set to zero, what we see is the "tragedy of the commons" (Hardin, 1968). The resource collapses quickly and the wealth level of highlander groups plummets. By turning "punish-traditional?" on, we have enforcement of norms against overharvesting, such that agents who are found to be harvesting from a patch that is at less than half its maximum resource value are punished by others who can afford the cost of punishing. In this way, resource collapse can be averted in the model for the equivalent of millennia. This represents a self-governed commons (M. Janssen, 2020, 214; Ostrom, 1990). See figure 1.

The behavior of the model described thus far is equivalent to what is shown by Janssen's model (Janssen, 2020, page 214). If "tobacco?" is now switched on, those within a certain radius of a randomly placed "market" are tempted by higher payoffs to grow tobacco, which is represented in the model by overharvesting.[22] Some of this mid-level commodification, when localized near the market, can coexist with a self-governed commons for the equivalent of many centuries. This matches what appears to have happened with Lauje highlanders (Li, 2014, 25). See figure 2.

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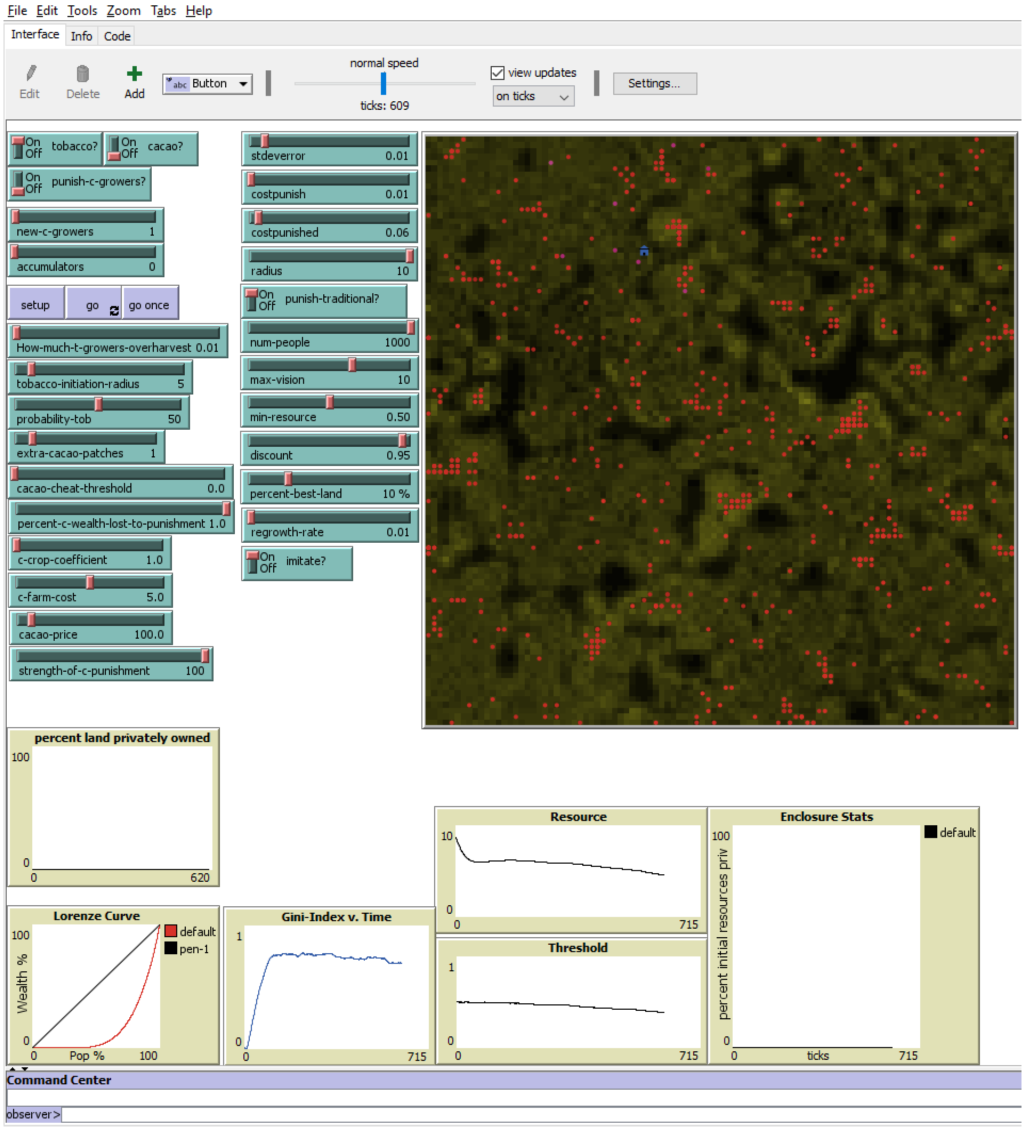
[21] Highlanders in fact never used cacao and did not understand its utility, besides being a commodity for sale (Li, 2014, 123).

[22] The radius from the market, the rate at which agents are tempted to become tobacco-growers, and the amount they overharvest when doing so, can all be adjusted by sliders.



On Off tobacco?
On Off cacao?
On Off punish-c-growers?
new-c-growers 1
accumulators 0
How-much-t-growers-overharvest 0.01
tobacco-initiation-radius 5
probability-tob 50
extra-cacao-patches 1
cacao-cheat-threshold 0.0
percent-c-wealth-lost-to-punishment 1.0
c-crop-coefficient 1.0
c-farm-cost 5.0
cacao-price 100.0
strength-of-c-punishment 100
stdeverror 0.01
costpunish 0.01
costpunished 0.06
radius 10
On Off punish-traditional?
num-people 1000
max-vision 10
min-resource 0.50
discount 0.95
percent-best-land 10 %
regrowth-rate 0.01
On Off imitate?

Figure 2--Tobacco-growers (purple) are near "market" (blue). They over-use resources but do not convert patches into permanent private property. This can coexist with self-governed commons for centuries.

When “cacao?” is switched on, highlander groups are tempted by much higher payoffs to start growing cacao. The closer they are to the market, the higher the likelihood of them starting to grow it. When they do grow cacao, they take a group of patches as private property and cease to circulate or share their patches with others.[23] Figures 3 and 4. With

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[23] The number of patches they start growing cacao on is adjustable—it is set by adding the number on the “extra-cacao-patches” slider to a default initial patch.

"accumulators" set to zero, we see equitable development of cacao farming. With that slider set higher, that slider-number of highlanders per tick will accumulate more groups of patches for cacao farms. This can leave most highlanders without any resources. The former scenario could be seen as a cacao-promoting NGO's dream, and the latter scenario is more like realism in the case of the Lauje highlanders' plight (Li, 2014, 139).

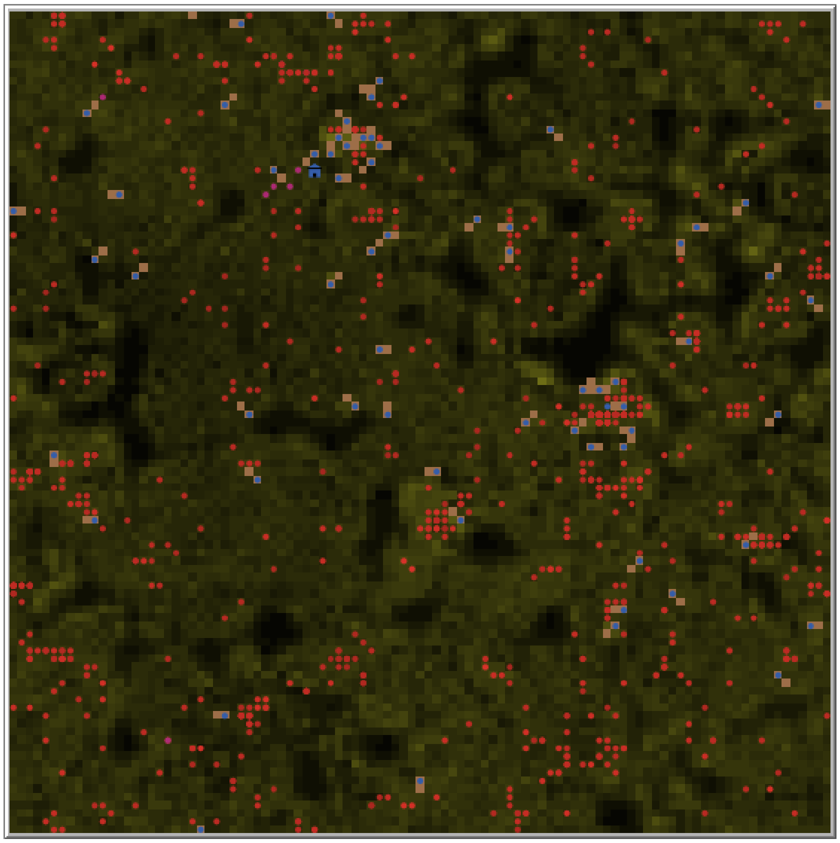

Figure 3--Detail of the ABM "View" with cacao farms "brown" starting to appear. Cacao-farmers are colored blue.

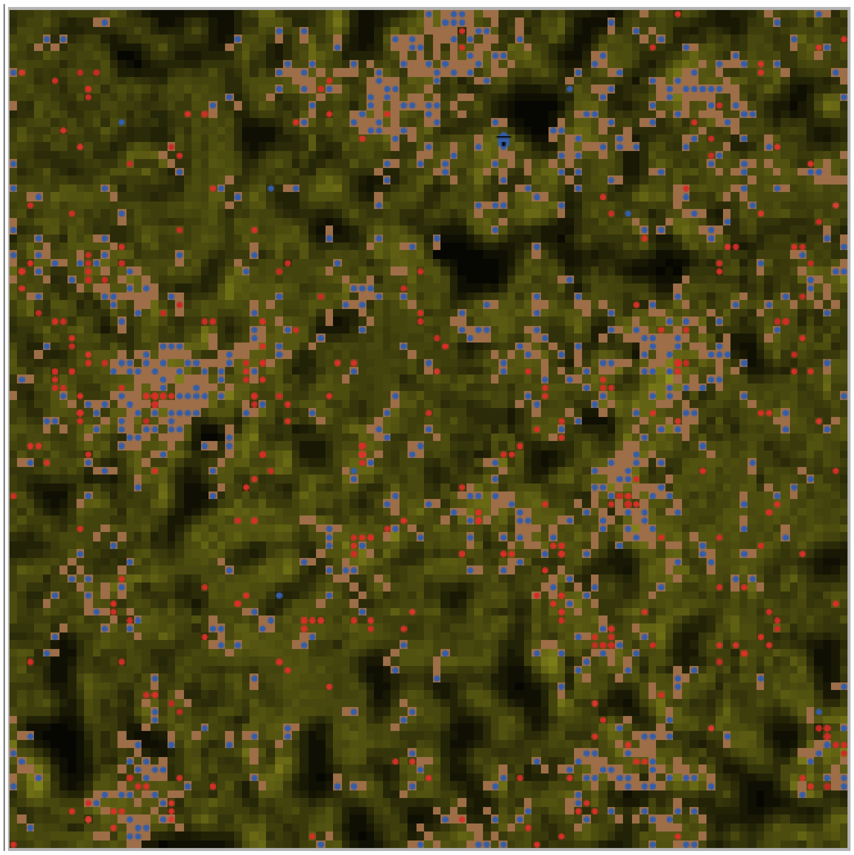

Figure 4--Detail of "View" with many cacao farms. Enclosure of private property induces emergence of capitalist relations, as highlanders are deprived of free access to land on which to grow food.

Lastly, with “punish-c-growers?” turned on, agents can punish cacao-growers.[24] To an extent, this can represent the “weapons of the weak” (Li, 2014, 155; Scott, 1985), such as gossip, theft, arson and slander, that highlanders did actually use against cacao-growers. In the model, however, if we wish we can turn up the dial on that punishment and adjust it until cacao-growing ceases completely. This can represent some of the “what if?” questions, alluded to earlier. When highlanders have greater awareness of the threat the cacao boom poses to

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[24] The amount of punishment, how sensitive to punishment cacao-growers are, and the propensity to punish, are set by the “strength-of-c-punishment,” “percent-c-wealth-lost-to-punishment,” and “cacao-cheat-threshold” sliders, respectively.

their way of life, they can resist the destruction of their self-governed commons. This represents a counterfactual, hypothetical historical scenario.

Evaluating the Model

Li's highly nuanced account is caricatured by the model's treatment. This is not necessarily a shortcoming of my model; ABMs will never be a replacement for nuanced interpretive methods like ethnography. However, the reliance on Janssen's ABM only very crudely approximates the dynamics of Lauje highlander swidden agriculture, for example. According to Li, these dynamics involved reciprocation of festive work parties (Li, 2014, 63), for example, which were more or less difficult to pull off, and disappeared later as the social fabric unraveled (Li, 2014, 123). Various more sophisticated schemes to represent those dynamics game-theoretically suggest themselves but were beyond the scope of this project. As discussed in Chapter Two, the game-theoretic treatment of social norms is a very active area of research with no consensus on best practices.[25]

The ABM does not approach the level of realism, for example, of Lansing and Kremer's model discussed above. If it did, there is a whole battery of error analysis, sensitivity analysis, uncertainty analysis, and structural uncertainty analysis tests that could and should be done on it.[26]

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[25] I have since learned of efforts in progress to model swidden agriculture socioecological dynamics among Q'eqchi' Maya in Belize that have many similarities to those of the Lauje highlanders, including this kind of labor reciprocity (Correa & Downey, n.d.). See also Downey et al. (2020).

[26] See chapter seven in O'Sullivan and Perry (2013). Such a technical tour de force would be a silly exercise with my ABM.

Frankly, I don’t think my model advances our understanding of the emergence of capitalist relations much (with the Lauje or elsewhere). Downey provided an exemplary instance of the use of simulation modeling in conjunction with ethnography by modeling a simulation of Paul Willis’ classic ethnography, “Learning to Labor.” As Downey (2005, 1) has written, “simulation modeling can be used constructively to expose important questions that were not asked while in the field, and identify important relationships that might not be evident.” It cannot be claimed that my ABM succeeds in probing Li’s theory in this kind of way.

It is tempting to say that my ABM uses Li’s (2014) work to focalize the dynamics of the model, and that it can be seen as a rough approximation that can be useful in an abstract, heuristic way. There are, after all, many diverse cases of common property systems that have struggled to survive in the face of market pressures—a common theme in the literature of political ecology (Robbins, 2012, 45, 51). However, I am not sure that my ABM succeeds in being generalizable at all. Nevertheless, it could prove useful as a pathbreaking baby step for other researchers who seek to open similar paths.

It might be interesting to reorient the present model to be focused on the question of the importance of low-cost, local conflict resolution, in terms of the resilience of self-governed commons. This is one of Ostrom’s (2000, 152) eight “design principles” that she sees as key to such resilience.[27] In the case of the Lauje highlanders, it appears that they must have lost much

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[27] The eight principles are (1) local users "define who has rights to withdraw from the resource"; (2) effective assignment of costs that are proportionate to benefits; (3) "the users of a resource design their own rules"; (4) those rules are "enforced by local users or accountable to them"; (5) this enforcement uses graduated sanctions; (6) "access to rapid, low-cost, local arenas to resolve conflict among users or between users and officials"; (7) "minimal recognition of the right to organize by a national or local government"; (8) the presence of governance activities organized in multiple layers of nested enterprises" (Ostrom, 2000, 151–152).

of their indigenous conflict resolution processes. At least, they believed that their whole system of dealing with conflict was given to them by the Dutch colonists. Highlanders "welcomed their inclusion in the [modern] system of rule through headmen situated on the coast that had been in place since Dutch colonial times because they found it helpful in keeping the peace, and resolving disputes" (Li, 2014, 10). And although they did have their own unique ways of dealing with conflict locally at low cost, it was not too infrequent that they depended on modern authorities on the coast to adjudicate when they could not work things out on their own.[28]

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[28] Emily T. Yeh's objections to both ABM and Ostrom's socio-ecological systems (SES) framework need to be considered here. Working with an interdisciplinary climate change research team on the Tibetan Plateau (Klein et al., 2014), Yeh (2016, 37) reports that the project was "funded only when we included a coupled ecological and agent-based household decision-making model as the primary 'integration' piece of the project". According to Yeh (2016, 37), methodological individualism is "at the core" of ABM. (Methodological individualism is a doctrine, attributed to Ludwig von Mises (Gintis, 2014, 171) and generally held by economists, among others, claiming that all social behavior can be explained by strategic interactions among individuals (Gintis, 2014, 245). Thus "nothing beyond the characteristics of individuals is needed, or even permitted, in modeling social behavior" (Gintis, 2014, 171)). Yeh (2016, 37) writes, "[ABM] elide[s] power relations as manifested in the dynamics of capitalism and class differentiation, racial formations and gender ideologies. Simple household decision rules stand in for what are in fact complicated decisions made recursively through the interplay of cultural politics and political-economic pressures, emotions as well as facts."

Extending this critique to Ostrom's socio-ecological systems (SES), Yeh (2016, 37) continues, "a number of [the] characteristics [of methodological individualism] can also be found in the... predominantly mathematical, economic and rational choice approaches...worked into a broader framework of [SES]…The drawing of a bounded system, and the way in which the representational form of SES privileges proximate rather than underlying structural and historical processes, has some major shortcomings. Relational power, interest, multiple social identities, and the interplay of structure and agency, are difficult to represent and thus easy to lose sight of. As a result, normative questions are ignored, but this elision itself has normative consequences: the tendency toward a conservative approach to social change, and a propensity for diagnoses and solutions that are either top-down or laissez-faire…The dynamics of capitalist accumulation and class struggle are...strikingly absent from SES framework studies of resilience and adaptation…So too is colonialism... The framing of social-ecological systems in the here-and-now leaves no space for an investigation of colonialism's ongoing effects."

These are biting criticisms and definitive answers to them will not be attempted here. It is worth noting, however, that in similar criticism of methodological individualism within ABM—by geographers who know it well—there is a key difference with Yeh's view. O'Sullivan and Haklay (2000, 1413) argued that "ABMs often tend strongly towards methodological individualism…not that methodological individualism is unavoidable, but that it is a frequently unacknowledged assumption of models of this kind." Rather than being intrinsic to ABM, they find the roots of individualism in its origins in computer science (O'Sullivan & Haklay, 2000, 1416-17) and in various intellectual trends in social science (O'Sullivan & Haklay, 2000, 1417-18). Herbert Gintis, an avid proponent of ABM (Gintis, 2007; 2014, 196), is also one of the most vociferous critics of methodological individualism (Gintis, 2014, 171; Gintis et al., 2019, 27).

For O'Sullivan and Haklay, it makes a difference if the agents in a model represent individuals. The fact that they almost invariably do (O'Sullivan & Haklay, 2000, 1413) does not mean that they have to. In the present model, for example, agents primarily represent groups of people, or in one case a market center. It could be that Yeh would disagree with this reasoning, but it is not clear what she means by methodological individualism being "at the core" (Yeh, 2016, 37) of ABM.

As for Yeh's claim that ABM elides power relations, her critique is very welcome, and it is not surprising that this may hit the nail on the head in most cases of ABM. But again, it is questionable whether this elision is intrinsic to ABM, or a historical artifact of its use. In fact, although Yeh (2016, 37) appears to imply that this particular shortcoming of ABM is absolutely intrinsic to it, this is not explicitly clear in her own account. While it is difficult to evaluate the effectiveness of the current ABM in this regard, the intention for it was certainly to eschew the elision of "power relations as manifested in the dynamics of capitalism and class differentiation" (Yeh, 2016, 37).

Yeh's broader criticism of the ahistoricism in SES will have to wait for someone more knowledgeable than I for an incisive response. To my knowledge, Yeh is the only political ecologist to have collaborated with agent-based modelers. Could it be that she is also the only political ecologist to have collaborated with scholars in SES? SES and political ecology appear to this author to be closely allied fields. (Political ecology even comes up as an allied field in the Wikipedia page for SES (Crowd-sourced, 2022)). The lack of dialogue between the two could be a symptom of the general critical/quantitative divide discussed above. In any case, it is hoped that Yeh's criticism of SES can be welcomed and responded to by people who are more firmly rooted in it.

It may be true, as Yeh claims, that "normative questions are ignored" in SES (Yeh, 2016, 37). But social norms are certainly not. The question of the evolution of social norms is at the heart of Ostrom's (2000) approach. As I discussed above, the reliance of the present ABM – with its use of the "punish-traditional?" switch, which simply turns on the enforcement of norms against overharvesting – in Janssen's (2020, 214) model, makes for a very crude approximation of the dynamics of Lauje highlander swidden agriculture. This is primarily because the treatment of social norms there is very minimal. However, it can be argued that Janssen and I should not be criticized too harshly for making a simple presentation of this topic. The game theoretic modeling of social norms is in fact an overwhelmingly active area of research, with no consensus on a best approach at present (see discussion in Chapter Two). This connects to a range of debates that warrant geographers' attention, to which I will turn shortly.

Chapter Two—Complexity Evolving: Norms, Games, Space

Introduction to Chapter Two

Biology has recently seen a debate raging about the nature of social cooperation (Abbot & 136 co-authors, 2011; M. A. Nowak et al., 2010)—a "clash of the titans," as Gintis (2012)[29] has put it, rarely seen in the sciences. In short, the "selfish gene" paradigm (Dawkins, 2006), which convinced a generation of biologists that cooperative behavior could be explained solely in terms of organisms helping others that share their genes, has come under attack. In its place, a new paradigm has been proposed which recognizes that there is a complex evolutionary dynamic within the genome itself, where genes affect each other, making for more than single loci additive dynamics assumed by the influential "Hamilton's Rule"[30] (Gintis, 2013; Hamilton, 1963). A revised theory of group selection—once considered discredited—has been formulated, which recognizes that the legitimacy of Hamilton's Rule is limited, and when it comes to social species this rule needs to be understood in a broader context of multilevel selection (Traulsen & Nowak, 2006). This new biology of sociality—in particular the theory of gene-culture coevolution, also known as dual inheritance theory (DIT)—is helping to spawn a new transdisciplinary "behavioral sciences" paradigm (Gintis, 2014, 194) with striking implications for all the social sciences, and deep resonances with an important forerunner of critical geography, Pyotr Kropotkin (Bowles & Gintis, 2011, 7; Kropotkin, 1885).

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[29] Gintis plays a role of diplomatic conflict mediation in this debate. He purports to seek a compromise with his own results (Gintis, 2013). However, it has been argued (Yee, 2014) that he actually makes a more effective case that the "inclusive fitness" of Hamilton's Rule (see below) is not all-explanatory, compared to the original, incendiary critiques of Hamilton's Rule (Nowak et al., 2010). Also note that Gintis has stated openly that he is closely allied with the authors of those critiques, E.O. Wilson and Martin Nowak (Carrol & Gintis, 2021, at 1:01:20 in video).

[30] "Hamilton's rule asserts that a trait is favored by natural selection if the benefit to others, B, multiplied by relatedness, R, exceeds the cost to self, C. Specifically, Hamilton's rule states that the change in average trait value in a population is proportional to BR−C" (Nowak et al., 2017, 5665)

In an independent but related development, the ascent of punctuated equilibria theory (PET) (Eldredge & Gould, 1972) in paleontology over the last five decades has dramatically overturned gradualism in evolutionary biology, replacing it with a view of vastly long periods of stasis punctuated by rapid speciation events—and this too has spurred striking developments in social science. Namely, the "dissipative social systems evolution" approach started by David L. Harvey and Michael Reed (1997, 1994) takes PET and ideas about self-organizing systems from Prigogine (Prigogine & Stengers, 1984) to form a mutually-referencing framework that integrates well with ideas from Marx.

One ramification of PET—because it entailed questions about biases in the births and deaths of species (Eldredge, 2016, 14)—has been the revival of a hierarchical perspective in evolutionary theory,[31] in which biological entities are nested parts within larger wholes (Eldredge, 2016, 1); species, larger-scale monophyletic taxa, and ecological communities are seen as real and consequential historical entities that can be construed as individuals at various scales (Eldredge, 1985, 5). In the hierarchical framework that Eldredge and colleagues (1985, 1986, 1989, 1995, 2016; Eldredge & Grene, 1992; Eldredge & Gould, 1972) have spearheaded, all nonsocial organisms are part of two separate hierarchically nested systems that, at larger scales, are distinctly either genealogical (reproductive) or ecological (pertaining to matter-energy transfer processes of growth and maintenance of the body, also referred to as "economic").[32] The recognition that these dual hierarchies have little to do with each other at

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[31] According to Eldredge (2016, 12), it is a revival because hierarchical thinking was important in the thought of Darwin's precursor, Giambattista Brocchi, and indeed to Darwin himself, in his early years, but this remained unknown for a long time.

[32] My discussion of Eldredge's dual hierarchy theory is necessarily very condensed but I will elaborate a bit here. It has subtle but profound implications for how we view life and social systems: All organisms embody a dual

higher levels, but interact consequentially, enables a (more traditional Darwinian) view of natural selection as a *passive* filter—in contrast with the dominant neo-Darwinian view in which evolution boils down to an *active* dynamic of competitively maximizing reproduction, of which all else is an epiphenomenon. From Eldredge and colleagues' perspective, social systems are entities arising from a *reintegration* of these diverging organismic economic (ecological) and reproductive dimensions of activity (Eldredge & Grene, 1992, 3). This revised ontology applies to all social species, but in interestingly very different ways, and has profound implications for how we think about human sociality. Harvey and Reed (1994) also build upon this foundation of Eldredge's hierarchical framework with their synthesis of Prigogine and Marx.

DIT and Eldredge and colleagues' evolutionary hierarchy theory can be seen as two competing alternatives to the dominant sociobiological paradigm (Wilson, 1975), both chipping away at the latter's genetic determinism. Meanwhile, as J. Anthony Stallins (2012) has written,

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character with both "economic" and "genealogical" sets of processes—neither set of processes is more important and they are ultimately inseparable. Yet "these sets of organismic processes have actually gone a long way towards separation as the phylogenetic history of life over the past 3.5 billion years has proceeded" (Eldredge & Grene, 1992, 68). In prokaryotes there is very little of such separation. With the advent of eukaryotes, distinct morphologies and loci of these classes of biological activities begin to develop, and with multicellular organisms this separation is taken much further with specific organ systems. This correlates with the idea of "two general classes of phenotypic features [and] hence adaptations" (Eldredge & Grene, 1992, 76) which was recognized by Darwin with his distinction between natural selection and sexual selection, but this has been nearly lost in modern versions of Darwinism. At larger scales, "Two distinctly different—and increasingly divorced—sets of associations of organisms arise as an automatic consequence of these two distinct sets of behaviors, physiologies, and morphologies housed within nearly every single organism…Species and monophyletic taxa are natural outgrowths of simple reproductive behavior; local and regional ecosystems, in contrast, necessarily arise as a simple consequence of economic activities of organisms" (Eldredge & Grene, 1992, 76-77). Social systems are *reintegrations* of these sets of processes which have undergone this long trend of separation (and further division of labor) in the history of life, at the scale of aggregates of organisms. The nature of these social systems is quite different, however, between colonial marine invertebrates like corrals, insects like wasps, and vertebrates generally, including humans. And human social systems display further unique oddities among animals in that we are "culture dwelling animals" requiring a "secondarily altricial" stage of maturation—in our first year of life it is as if we are in a secondary "social uterus," immersing us in language and culture; and our brains grow up to four times their natal size at maturity (Eldredge & Grene, 1992, 179-182). All this has important implications for debates about sociobiology, discussed below.

many different developments in biology of the last twenty years (including genomics, epigenomics, transcriptomics, proteomics, interactomics, metabolomics, and microbiomics) have converged to produce a new paradigm of complex organism-environment interaction that has definitively undermined reductionism and genetic determinism in the discipline of biology as a whole. Stallins has called for geographers to engage with this new paradigm in biology as a way to contribute to key debates in geography about scale and environment-mediated causality. I argue that both Gintis's work and Harvey and Reed's work resonate strongly with this call and invite expansion of it. However, there are tensions between Gintis's work and Harvey and Reed's work—as there are between the DIT and PET/evolutionary hierarchy theory the two new paradigms draw from, respectively. I explore these tensions—some would say incompatibilities—and argue for the possibility of a discerning reconciliation.

Before exploring the contours of all these developments in earnest, however, it is necessary to attend to the fraught historical relationship between biology and geography.

The Biology-Geography Disciplinary Nexus

It is no wonder that geographers have been wary of engaging with biology for the last hundred years. Virtually every major figure in geography around the turn of the twentieth century—from Sir Thomas H. Holdich and Siegfried Passarge to Isaiah Bowman and various French counterparts—justified their views with Darwinian-sounding language[33] (Livingstone, 1993, 216-218, 244, 249). And with only one exception, they did so to rationalize racism and

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[33] The only exceptions are Élisée Reclus and George Perkins Marsh (Livingstone, 1993).

serve empire. Kropotkin is the only one who was distinctly *against* empire or colonialism (or the state in general) (Piotr Kropotkin, 2021; Livingstone, 1993, 254). What Livingstone fails to point out is that Kropotkin is also the only one of these geographers who is taken seriously by biologists to this day, and who is considered to have made enduring, significant contributions to the field of evolutionary biology itself (Boucher, 1985, 17; Gould, 1997, 12-21; Kropotkin, 2021 [1902]).

According to Stallins, “evolution helped North American academic geography unify in its formative years [circa 1900] around the study of human–environment interactions (Stoddart, 1966, 1986 [cited in Stallins]). Geographers reworked these evolutionary outlooks to construct their academic niche, but backed away from them when their incompleteness, their social implications, and their blatant falseness became visible” (Stallins, 2012, 427). Ironically, this may have been productive, in that “geography’s subsequent self-imposed exile from biology and evolutionary thought set in motion geography’s ongoing pluralization of ideas about how environment, scale, and causality are intertwined” (Stallins, 2012, 428).

There was a parallel self-imposed exile from biology in the social sciences in general, and sociology in particular, according to David L. Harvey and Michael Reed (D. L. Harvey & Reed, 1994, 371). “At one point the idea of social evolution seemed to have been ceded to anthropologists and antiquarians for safe keeping, as sociologists and political scientists turned en masse to a strangely ahistorical Cold War science” (D. L. Harvey & Reed, 1994, 371). Talcott Parsons opened the work that launched his monumental career by declaring evolutionary thought in sociology to be dead (D. L. Harvey & Reed, 1994, 372; Parsons, 1949 [1937], 3). But this parallel self-exile had its own idiosyncrasies in sociology. Almost thirty years later, after the consolidation of the neo-Darwinian modern synthesis in the biology of the 1940s, Parsons returned to embrace and assimilate evolutionary theory (D. L. Harvey & Reed, 1994, 372; Parsons, 1966). Harvey and Reed interpret Parsons's turnaround as reflecting a special affinity for the conservative, glacial rates of change prescribed by the modern synthesis, with its genetic reductionism. "Parsons found in the neo-Darwinian synthesis a theory wholly consonant with his conservative interpretations of society as a homeostatic, order-seeking system" (D. L. Harvey & Reed, 1994, 372). Unfortunately for Parsons, this view would be refuted in biology in the 1980s by Gould and Eldridge's (1977) theory of punctuated equilibria. (D. L. Harvey & Reed, 1994, 372).

The theory of punctuated equilibria holds that the evolution of new species occurs quite rapidly for a geological time scale (5,000-50,000 years), emerging from marginal populations that become geographically isolated due to environmental change, notably catastrophic mass extinction events (Eldredge & Gould, 1972; S. J. Gould & Eldridge, 1977; Harvey & Reed, 1994, 392). But "stasis is the rule, not the exception" (Eldredge, 1995, 94). Thus PET makes a decisive break with the view of uniform, smooth, gradual change that had been virtual doctrine in evolutionary biology since Darwin. Much as Stallins (2012) has called for geographers to engage with the new ideas about organism-environment interaction in biology, as an opportunity to rethink (human) geographical theory, Harvey and Reed (1994, 373) took the ascent of punctuated equilibria to call for a revision of social theories grounded in the neo-Darwinian synthesis, with its historical bias towards uniform evolutionary processes and gradualism. The new paradigm has a much easier time making sense of forms of social change that come in dramatic leaps.

Stallins's (2012, 332) focus here is mostly on the very small scale processes of genomics, epigenomics, transcriptomics, proteomics, interactomics, metabolomics, and microbiomics. All of these are fields of study that have forced molecular biologists to relinquish genetic determinism in the last twenty years. "The Central Dogma of DNA, where one gene codes a single protein that in turn can direct and regulate physiological processes, has been replaced by a view of DNA that foregrounds the role of space and place. This new spatial view recognizes a far greater role for the contingencies of the environment to modify DNA expression" (Stallins, 2012, 432).[34] Proteins can interact with each other or bind to regulatory DNA with consequences for DNA expression. Environmental conditions can change expression of genetics through epigenetic mechanisms, without changing DNA sequence. All these "omics" represent new initiatives to come to grips with complex evolving systems, now widely recognized to operate on many levels. Echoing Wilensky and Rand (2015, 11) on emergent structures and murmuration patterns, discussed above, proteomics is now needed, for example, because "no centralized cellular mechanism guides [protein] folding. Rather, the subtle chemical push and pull between constituent amino acids self-assembles proteins into their three-dimensional shapes" (Stallins, 2012, 432).

Zooming out from the molecular to the organismal and ecological scales, it's worth quoting Stallins at length:

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[34] When this author was completing his undergraduate degree in Integrative Biology at UC Berkeley, 2016-2018, it was evident that biologists were still reeling from the disorientation of having the rug of the central dogma (Crick, 1958) pulled out from under them. It was a topic of conversation among PhD students with much conflicting information.

> When biologists describe the genotype-phenotype-environment linkage spanned by the omics, they are invoking a spatial dynamism that is geographical, perhaps more so in the tradition of recent human geographic scholarship on scale and environmental causality. An organism is an ongoing, contextual outcome of a causality that propagates among molecular, cellular, organismal, and environmental scales. Its interaction with the environment is Lamarckian in that causality is spatially distributed and heredity is not strictly genetic. The inheritance of environmental effects can be induced not only by conditions internal and external to an organism, but also by the predictability in their larger environment as it can be shaped by the organisms within it (Stallins, 2012, 432).

The theme of organisms shaping the predictability of their environment will bring us back to the new behavioral sciences paradigm of Gintis and collaborators. It is now recognized that many species create environmental constructs and transmit them across generations, and that this is a form of epigenetic transmission (Gintis, 2014, 199; Stallins, 2012, 443). This is known as niche construction, and includes beaver dams (Dawkins, 2016; Gintis, 2014, 199), beehives, and social structures (Odling-Smee et al., 2003; Stallins, 2012; Gintis 2014). “Niche construction gives rise to what might be called a gene-environment coevolutionary process since a genetically induced environmental regularity becomes the basis for genetic selection, and genetic mutations that give rise to mutant niches will survive if they are fitness-enhancing for their constructors” (Gintis, 2014, 199). When this gene-environment coevolution takes form with human social structures, it is known as gene-culture coevolution (Check Hayden, 2009; Gintis, 2014, 200; Laland et al., 2010; Richerson et al., 2010; Stallins, 2012, 434). Gene-culture coevolution is foundational for the transdisciplinary behavioral sciences approach (Gintis, 2014, 195), to which I will now turn.

A Proposed Unification of the Social Sciences

There are five conceptual parts of Gintis's (2014, 195) framework: (a) gene-culture coevolution; (b) the sociopsychological theory of norms; (c) game theory; (d) the rational actor model; and (e) complexity theory. I will now briefly flesh out a discussion of these.

Gene-culture coevolution, introduced above, is worthy of relatively detailed discussion here. There are two dramatic examples of products of gene-culture coevolution with concrete physical evidence and many more that are more elusive. One concrete example is the anatomical features that permit human speech. This includes the larynx being low in the throat (Relethford, 2009), known only from *Homo heidelbergensis* (800,000-100,000 years ago), Neanderthals and modern humans (Gintis, 2014, 201); the short oral cavity needed for the production of most consonants; the hyoid bone to which the tongue muscle is attached; and the large size of the hypoglossal canal, which enables connection of the nerve needed for effective control of the tongue. The latter is fixed in the fossil record, and is much bigger in Neanderthals and humans than any other known relatives (Campbell & Loy, 2000; Gintis, 2014, 201). According to the theory of gene-culture coevolution, as the capacity for speech began to evolve, the genetics necessary for these anatomical features were selected for by the cultural environment. That is, as our ancestors first began to be able to speak, cultural values dictated that those who were not as good at speaking did not have as many children. Over a 100,000 year period (or more) of positive feedback between culture and genetics, our physiology was dramatically transformed.

A second concrete example is the anatomy of complex facial expressions.[35] In mammals:

> mimetic musculature attaches to skin in the face, thus permitting the subtle and accurate facial communication of such emotions as fear, surprise, disgust, and anger. In most mammals, however, a few wide sheet-like muscles are involved, rendering fine informational differentiation impossible. In primates, by contrast, this musculature divides into many independent muscles with distinct points of attachment to the epidermis and distinct innervation, thus permitting higher bandwidth facial communication. Humans have the most highly developed facial musculature among vertebrates by far, with a degree of involvement of lips and eyes that is not present in any other species (Gintis, 2014, 202).

Again, presumably the genetic basis for these anatomical features also were selected for by the cultural environment:

> when a form of human communication became prevalent among hunter-gatherers, this new cultural form became the new environment within which new genetic mutations were evaluated for their fitness effects. Humans thus underwent massive physiological changes to facilitate speaking, understanding speech, and communicating with facial expressions (Gintis, 2014, 201).

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[35] Darwin (2009 [1890]) wrote a book about this subject, but it was ignored by biologists until recently (Burrows, 2008; Gintis, 2014, 194, 201).

A key proposition of Gintis is that, while not leaving fossil traces, “There is little doubt but that other human traits, such as empathy, shame, pride, embarrassment, reciprocity, and vengeance, traits without which social cooperation would be impossible, are the product of gene-culture coevolution” (Gintis, 2014, 202). This is where the sociopsychological theory of socialization fits into Gintis’s framework. Norms and their psychological/cognitive correlates or prerequisites are seen by Gintis (2014, 195) as emergent properties of all human societies. They cannot be deduced from society’s parts. There is a great diversity of social norms among different cultures, but all cultures socialize their members to conform to their social norms. All the human traits Gintis lists above are universally present in one form or another in different cultures, making human cooperation possible.

This implies the refutation of large bodies of contemporary theory in economics (Gintis, 2005, 2014, 200) and biology (Boyd & Richerson, 1988; Gintis et al., 2009; Richerson & Boyd, 2004) that have insisted on trying to show that strictly self-regarding rational individuals can bring about cooperation, without need for the emergent property of norms.

Rooted in the discipline of economics, Gintis arrives at the need for the sociopsychological theory of norms, in part, through an immanent mathematical critique of economic theory. This requires a discussion of game theory. What is game theory and why is it important?

Game theory is a logically demanding, mathematical modeling framework for strategic interaction, using algebra, calculus, and probability theory. “Game theory is a logical extension of evolutionary theory” (Gintis, 2014, 212):

> The analysis of living systems includes one concept that is not analytically represented in the natural sciences: *strategic interaction* [emphasis in original] in which the behavior of agents is derived by assuming that each is choosing a *best response* [emphasis in original] to the actions of other agents. The study of systems in which agents choose best responses and in which such responses evolve dynamically is called evolutionary game theory (Gintis, 2014, 202).

Game theory itself has evolved into four related disciplines: classical, behavioral, epistemic, and evolutionary game theory (Gintis, 2014, 195). Classical game theory originated from strategic concerns in World War II (Von Neumann & Morgenstern, 1944). To the surprise of some of its early innovators (Nash, 1950), it then became a key conceptual apparatus in mainstream economics. In the 1970s, it was transformed by biologists (Maynard Smith & Price, 1973) and became the standard way to model the behavior of nonhuman organisms. Gintis has championed the new field of behavioral game theory—the “application of game theory to the experimental study of human behavior" (Gintis, 2014, 2)—which has provided empirical evidence against the canonical model of self-interest (Henrich et al., 2005).[36] Epistemic game theory, a more sophisticated alternative to classical game theory when it comes to modeling human reasoning, attempts to construct a social epistemology, showing under what conditions shared mental constructs effect behavior (Gintis, 2014, xiii, 142).

Gintis is a prolific proponent of the use of game theory in the social sciences (Gintis, 2009a, 2014b). But he is also a formidable critic. He attacks the reigning culture in game theory

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[36] This is a position that has put him into sometimes heated debate with other economists (Binmore, 2010; Gintis, 2011b).

as being guided by the prejudice that “game theory is, insofar as human beings are rational, sufficient to explain all of human social existence” (Gintis, 2014, xi). This allows game theorists in economics to do theory with no regard for facts or for the contributions of other social sciences.

Gintis (2014, xii) demonstrates mathematically that game theorists have fundamentally failed to provide a theory for the conditions of shared mental constructs. By incorporating the sociopsychological theory of norms into epistemic game theory, he believes he has solved the problem. He argues that social norms can be modeled game theoretically as correlated equilibria (Gintis, 2010b). In short, correlated equilibria occur in epistemic games—strategic games where a signaling (or “correlating”) device, which Gintis calls a choreographer, coordinates the best moves for all players. “Social norms act not only as choreographer, but also supply the epistemic conditions for common” beliefs (Gintis, 2014, 143). For a full, mathematical exposition of this argument, see chapters seven and eight in Gintis (2014).[37]

The rational actor model—which Gintis prefers to call the “beliefs, preferences, and constraints” (BPC) model—holds that individuals can be mathematically modeled as the maximizers of preferences, under certain conditions, namely that the preferences are consistent and the decisions are “routine” rather than “deliberative” (Gintis, 2014, 4, 209). “A

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[37] Note that the game theoretic treatment of social norms is an extremely active area of research (Aktipis et al., 2011; Bendor & Swistak, 2001; Brennan, 2013; Carpenter et al., 2009; Conte & Castelfranchi, 1999; Gavrilets & Richerson, 2017; Gintis et al., 2009; Gulesci, Selim, 2021; Hauert, 2002; Liu, 2021, 2020; Morsky & Akcay, 2019; M. Nowak & May, 1993; Opp, 2002; Paternotte & Grose, 2013; Rifki & Ono, 2021; Sterelny et al., 2013; Szabo & Szolnoki, 2012; Taylor, 1992; Wilczynski & Brosnan, 2021; Young, 1998, 2015; Zareen et al., 2016). Also note that there is no consensus on best approaches. While Gintis believes he has built upon the work of other leading theorists, particularly Bicchieri and Binmore, they have disagreed (Bicchieri, 2010; Binmore, 2010). Furthermore, others reviewing the field have concluded that the approaches of Bicchieri, Binmore, and Gintis are all incompatible with each other (Paternotte & Grose, 2013).

rational actor need not be selfish. Indeed, if rationality implied selfishness, the only rational individuals would be sociopaths" (Gintis, 2014, 1). Beliefs—which are products of social processes, shared among individuals, and need not be correct or welfare-enhancing for the model to work (Gintis, 2014, 208)—stand between choices and payoffs. It is because of the importance of beliefs, and confusion around the term "rational," that Gintis (2014, 1) prefers the term, BPC. Gintis believes that many social scientists have rejected the BPC model because of extravagant claims made by some of its adherents, but that this rejection handicaps those who reject it, including psychologists and sociologists, for example (Gintis, 2014, xi):

> For every constellation of sensory inputs, each decision taken by an organism generates a probability distribution over outcomes, the expected value of which is the fitness associated with that decision. Since fitness is a scalar variable, for each constellation of sensory inputs, each possible action the organism might take has a specific fitness value, and organisms whose decision mechanisms are optimized for this environment choose the available action that maximizes this value. This argument was presented verbally by Darwin (2009 [1998] [cited in Gintis]) and is implicit in the standard notion of "survival of the fittest," but formal proof is recent (Grafen, 1999, 2000, 2002 [cited in Gintis]) (Gintis, 2014, 207).
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> The rational actor model is the cornerstone of contemporary economic theory and in the past few decades has become the heart of the biological modeling of animal behavior (Alcock, 1993; Real, 1991; Real & Caraco, 1986 [cited in Gintis]). Economic and biological theory thus have a natural affinity: the choice consistency on which the rational actor model of economic theory depends is rendered plausible by evolutionary

> theory, and the optimization techniques pioneered in economics are routinely applied and extended by biologists in modeling the behavior of nonhuman organisms (Gintis, 2014, 208).

Finally, about the interrelationship between the BPC, gene-culture coevolution, and game theory:

> The rational actor model is the most important analytical construct in the behavioral sciences operating at the level of the individual. While gene-culture coevolutionary theory is a form of ultimate explanation that does not predict, the rational actor model provides a proximate description of behavior that can be tested in the laboratory and in real life and is the basis of the explanatory success of economic theory. Classical, epistemic, and behavioral game theory make no sense without the rational actor model (Gintis, 2014, 195).

A few points for geographers should be clear from the above. First, if we follow Gintis and want to engage seriously with biology, it would be helpful to understand game theory. Secondly, that also entails understanding the BPC.

The fifth and final conceptual unit of Gintis's proposed transdisciplinary paradigm is complexity theory. It is instructive to see how he deploys the theme of emergent properties, highlighting the necessity of interpretive, historical, ethnographic methods, and ABM—all of which he places in the same category, in relation to understanding these properties. Complexity theory is needed for social science because:

> human society is a complex adaptive system with *emergent properties* [emphasis in original] that cannot now be, and perhaps never will be, fully explained starting with more basic units of analysis. The hypothetico-deductive methods of game theory and the rational actor model, and even gene-culture coevolutionary theory, must therefore be complemented by the work of behavioral scientists who deal with society in more macrolevel, interpretive terms, and develop insightful schemas that shed light where analytical models cannot penetrate. Anthropological and historical studies fall into this category, as well as macroeconomic policy and comparative economic systems. Agent-based modeling of complex dynamical systems is also useful in dealing with emergent properties of complex adaptive systems (Gintis, 2014, 196).

Gintis has strong challenges, specifically in relation to complexity theory, for those who think game theory can explain everything: "human society is a system with emergent properties, including social norms, that can no more be analytically derived from a model of interacting agents than the chemical and biological properties of matter can be analytically derived from our knowledge of the properties of fundamental particles" (Gintis, 2014, xii).

These ideas form barbs that Gintis aims especially at his fellow economists: if the methodological individualism they generally accept were correct, “gene-culture coevolution would be unnecessary, complexity theory would be irrelevant, and the sociopsychological theory of norms could be derived from game theory” (Gintis, 2014, 217). Moreover, most economists specifically “reject the idea of society as a complex adaptive system, on grounds that we may yet be able to tweak the Walrasian general equilibrium framework, suitably fortified by sophisticated mathematical methods, so as to explain macroeconomic activity." Gintis (2007b, 2013b, 2014, 217) believes such a tweak to be impossible.

This view of society as a complex adaptive system nested in other complex systems, includes what has elsewhere been called "the evolution of evolution" (D. L. Harvey & Reed, 1994, 388):

> We learn from modern complexity theory that there are many levels of physical existence on earth, from elementary particles to human beings, each level solidly grounded in the interaction of entities at a lower level, yet having emergent properties that are ineluctably associated with the dynamic interaction of its lower-level constituents, yet are incapable of being explained on a lower level. The panoramic history of life synthesis of biologists Maynard Smith and Szathmáry (Smith & Szathmáry, 1997 [cited in Gintis]) elaborates this theme that every major transition in evolution has taken the form of a higher level of biological organization exhibiting properties that cannot be deduced from its constituent parts (Gintis, 2014, 172).

To sum up, in Gintis's view there are four conflicting models of decision-making and strategic interaction across the behavioral sciences of economics, anthropology, sociology, psychology, political science, and biology insofar as the latter concerns human and animal behavior.[38] These models he specifies as the psychological, the sociological, the biological, and the economic (Gintis, 2014, 194). If they were just different models, that would be fine and to

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[38] Note that Gintis never addresses geography specifically, but due to the overlap of our discipline with all the above, it should be clear that his challenge applies to geographers, variously, as well. By the same token, it is an open question for me whether some geographers may have models of decision-making and strategic interaction that are distinct from those of the above disciplines.

be expected. But they are incompatible—"each makes assertions concerning choice behavior that are denied by the others” (Gintis, 2014, 194)—implying at least three out of four of them are incorrect. Gintis argues that all four are seriously flawed, but each have important insights that can be preserved by modifying and reconfiguring them into a unified framework.

One example of putting this new paradigm to work is the monumental book, *A Cooperative Species: Human Reciprocity and its Evolution,*[39] by Bowles and Gintis (2011), which considerably fleshes out a gene-culture co-evolutionary theory of human cooperation. Space permits only a few brief points to be made here, but it must be said that what emerges is a richly argued, wide-ranging, provocative, and illuminating new view of *Homo sapiens*.

For reasons that are rooted in a million-plus years of ancestral environments—both natural and socially constructed—humans developed cognitive, linguistic, and neurological capacities for a predisposition to uphold ethical norms and closely cooperate with like-minded groups much larger than close kin. This is far from being a rosy view: the invention of lethal projectile weapons plays an important role in their account—including its elevation of group-level competition into an evolutionary force with greater power. The capacity for racism and xenophobia and all the worst human attributes developed hand in hand with the capacity for concern for others, aversion to tyranny, and all the best in us. But ultimately, Bowles and Gintis’s account resonates strongly with Kropotkin’s (1885), enabling a vision of freedom and justice for all (Bowles & Gintis, 2011, 7; Kropotkin, 2021).[40] From their view, the evidence

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[39] For a synopsis, see “A Cooperative Species: Précis” (Gintis, 2010a).

[40] As mentioned above, Kropotkin was a forerunner of critical geography (1885). His essays on cooperation in evolution, collected as *Mutual Aid: a Factor in Evolution* (Kropotkin, 2021 [1902]), are to this day seen as an enduring contribution to evolutionary biology (Gould, 1997). His contributions to ecology and ethnography

accumulated by Ostrom (1990) and others (M. A. Janssen & Anderies, 2013) for the capacity to avert the tragedy of the commons is not a terrible puzzle, because the greater puzzle of how we came to have such capacities is closer to being solved (Bowles & Gintis, 2011, 6).

Bowles and Gintis scaffolded this work on top of an impressive host of recent scholarship that they have undertaken with an array of collaborators (Gintis, 2005b; Gintis et al., 2009; Henrich et al., 2004, 2005). It is notable, in terms of the present thesis, that they make substantial use of ABM—along with marshaling evidence from a gamut of sources, including archeological, ethnographic, historical, neuroscientific, behavioral game theoretic, sociological, and political-economic. Furthermore, Gintis and others continue to advance similar ideas into new areas, deploying the most up to date evidence available (Gintis et al., 2019).[41]

I conclude the discussion of Gintis's transdisciplinary paradigm with some reflections on the implications for geographers in general and the present thesis specifically.

First of all, Gintis's treatment of the biology of sociality[42] and gene-culture coevolution strongly resonates with and extends Stallins's (2012) take on new developments in biology, discussed above. Stallins emphasizes the organism-environment interaction of niche-construction but only touches upon gene-culture coevolution (Stallins, 2012, 434). Gintis (2014,

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resonated strongly with his communist anarchist philosophy (Kropotkin, 1995). He was known worldwide for his fierce opposition to domination and oppression in all forms (Robbins, 2012, 25).

[41] It would be interesting to compare and contrast this newest investigation with Graeber and Wengrow's (2021) *Dawn of Everything*. The latter has a hundred thousand year time-line, whereas Gintis et al.'s "Zoon Politikon" (Gintis et al., 2019) has a million year time-line—which requires the latter to deal with different material, e.g. how the invention of lethal weapons effected cultural evolution. But there are many connecting themes, e.g. Graeber and Wengrow also seize on Aristotle's term, "Zoon Politikon," meaning "political animal," to express the human capacity for aversion to tyranny (Graeber & Wengrow, 2021, 86).

[42] Note that I use the term "biology of sociality" rather than sociobiology here. This may be controversial as I discuss, below.

200) elucidates the fact that the latter is a special case of the former. There will be more to say about this, below, once we have presented the related, but competing, ideas of Eldredge and colleagues.

Of course, this massive project of unifying all the sciences of behavior will offend many geographers, for whom the idea of having to adjudicate differences even between all of geography's subdisciplines is anathema. In Gintis's defense, I would hasten to point out that there is room for a plurality of very different forms of knowledge in his proposal—just not forms that totally contradict each other. Perhaps there are currents of geographical scholarship that reflect what Gintis sees as the psychological, the sociological, and the economic models, and engaging with his proposed paradigm can help these conflicting currents communicate with each other?

What might Gintis's transdisciplinary paradigm tell us about the emergence of capitalist relations among the Lauje highlanders? Let us recall Li's understanding of capitalist relations—defined as having taken root where participation in markets is experienced by the vast majority of a population as a coercive necessity of survival rather than an opportunity, and this experience drives dynamics of competition and productivity. From this perspective, the emergence of capitalist relations among the Lauje constituted the loss of a sophisticated system of cooperation—which can also be seen, I have argued, from Ostrom's framework as a succumbing to a tragedy of the commons. The concept of cooperation in Bowles and Gintis (2011) is so broad it includes warfare, mafia activities and cartels, as well as all of our more benign capacities, such as those reflected in the traditional Lauje highlander form of life. So there is no easy or immediate answer as to what Gintis's paradigm or various works within it tell us about this question.

Nevertheless, I would expect practitioners of Gintis's paradigm to take a keen interest in the Lauje and their plight. The team that Gintis brought together and funded to conduct behavioral game theoretic studies of small scale societies around the world had in fact intended to go to highland Sulawesi (Henrich et al., 2004, x), if the community they had relations with had not just recently been destroyed.[43] I would think they would be especially interested because what happened with the Lauje highlanders appears to contradict some of their main results: they found that "the more market integration there is, the more egalitarian people are" (Gintis, 2009b, 28:20 in video). It is possible that this is true to some extent but that the relationship is not linear. Perhaps up to a certain point the correlation holds true, but at some threshold—say, where capitalist relations take root, in Li's (2014) sense—more egalitarian social structures abruptly disintegrate? Certainly, it seems hard to believe that Gintis's results could be argued to extend beyond small scale societies to the states of the modern world, where inequality tends to be sky high in comparison.

As noted above, I have just learned of efforts to create ABM of swidden agriculture socioecological dynamics among Q'eqchi' Maya in Belize, where there are many similarities to those of the Lauje highlanders (Correa & Downey, n.d.). Downey et al. (2020) have also been conducting behavioral game theoretic experiments very much along the lines of what Gintis has advocated.

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[43] It is not clear to me exactly where Hentrich et al. (2004, x) refer to, but given the timeline, it could in fact be one of the exact same communities Li (2014) studied.

Considering the above, Gintis's paradigm could be of great help in formulating a research program around the Lauje highlanders and other communities like them.

What might Gintis's paradigm tell us about capitalist relations more generally? Gintis is most well-known for his early work when he was Marxist (Bowles & Gintis, 1976). However, in recent years he has tried to make it emphatically clear that he has repudiated his youthful love of Marx (Gintis, 2009b, 28:45 in video).[44] One wonders, however, if Gintis's ideas might be more subversive than he himself realizes? While he insists that his work supports the neoclassical economics paradigm on some occasions (Gintis, 2011a, 4), on others he pronounces that "empirical evidence challenges the very foundations of both classical game theory and neoclassical economics" (Gintis, 2014, xiv). And, as noted above, Gintis does not shy away from identifying his ideas with (at least some of) those of the (non-Marxist) communist Kropotkin (Bowles & Gintis, 2011, 7). It seems to me, at least, that Gintis's ideas are open to Kropotkinian and non-orthodox Marxian interpretations, probably despite Gintis's own intentions. More on this topic below.

Chaos and Capitalist Relations

There are explicitly Marxian versions of complexity theory (Byrne, 1998; Byrne & Callaghan, 2014; D. L. Harvey, 2001; D. L. Harvey & Reed, 1997, 1994; Williams, 2020). The

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[44] Bowles and Gintis's (1976) *Schooling in Capitalist America* has 16852 citations according to Google Scholar, as of this writing. That is well over three times as many citations for any of Gintis's non-Marxist works. It is little wonder that his economics colleagues still get confused by this and attack him for being anti-capitalist. (See Binmore (2010) and Gintis (2011, 4)). Nevertheless, after his twenty-year career as a Marxist, Gintis moved on some decades ago, and is outspoken about his disillusionment with Marxism.

seminal contributions are from Harvey and Reed (1997, 1994). They propose an ambitious rethinking of social evolution, based on Prigogine's theory of dissipative structures (Prigogine, 1981), punctuated equilibria theory (Eldredge & Gould, 1972, 1977) and the concommittant triadic hierarchy model of evolution (Eldredge, 1995, 2003, 2016). One of their main foci is a reformulation of Julian Steward's cultural ecology—foundational for political ecology (Robbins, 2012, 37)—within a framework that integrates Marx and Prigogine. Space does not permit a thorough review of Harvey and Reed's ideas here, but I hope to give a sense of the main contours, whet the appetite, and outline the debates and tensions within complexity theory that are entailed.

Ilya Prigogine—who won a Nobel Prize in chemistry in 1977 for work on dissipative structures—is a key figure for Harvey and Reed. Dissipative structures (Prigogine & Stengers, 1984, 142-143) are central in Harvey and Reed's theory. In brief, dissipative structures—also referred to as dissipative systems—are "natural thermodynamic entities capable of evolutionary behavior" (D. L. Harvey & Reed, 1994, 377). Two properties set them apart: (1) the capacity to import energy from their environment and transform it into complex internal structure; (2) as they accumulate random disorder—and all thermodynamically ordered systems do—dissipative systems can export that internal disorder to their environment. By dint of the first property they can be said to be "information preserving" and "information accumulating" (D. L. Harvey & Reed, 1994, 377-378). The capacity to evolve to increasingly improbable states of order and complexity internally is called negentropy, or negative entropy (D. L. Harvey & Reed, 1994, 383). Dissipative structures depend upon continuous energy flowing from their environments—they are far-from-equilibrium configurations, which "mark the move from chemistry towards biology, with chains of catalytic loops working to generate nonlinear cascades of reactions, similar to metabolic reactions in living beings" (Williams, 2020, 26).[45]

A central claim of Harvey and Reed (1994, 385, 389) is that social processes and their antecedent evolutionary processes in biology are special classes of dissipative systems.[46] Furthermore, they see chaos theory, dissipative systems theory, and punctuated equilibria as forming a mutually-referencing conceptual framework (D. L. Harvey & Reed, 1994, 395). Human history must be situated vis-à-vis natural history.[47] Like the punctuated equilibria view of natural history in Gould's *Wonderful Life*, human history is part of a "cosmological unfolding," the essence of which is contingency (Gould, 2000; D. L. Harvey & Reed, 1994, 388-389). "Gould's idea of history as contingency captures the essence of deterministic chaos and how it might be assimilated into the study of social systems" (D. L. Harvey & Reed, 1994, 389). Contingency, to be clear, is different from randomness—it is path-dependence, where a final result depends on what happened in every step in a process which preceded it (Byrne, 1998, 40).

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[45] For the purposes of this discussion, I will assume the mathematics and physics involved here are valid. Frankly, I am not qualified to evaluate them. Admittedly, neither are Harvey and Reed—although they do present much more of that material (D. L. Harvey & Reed, 1994). For now, I am mostly interested in giving a sense for how they use the metaphors involved and build a social scientific hypothesis.

[46] For a discussion of critics of Harvey and Reed's claim that social systems are a special class of dissipative systems, within sociology, see Williams (2020, 60-61).

[47] In fact, Harvey and Reed draw heavily upon Roy Bhaskar's philosophy of critical realism, which insists that the social sciences can be "natural sciences"—though not at all in a mechanistic or reductionist sense (Bhaskar, 1998; D. L. Harvey & Reed, 1994, 408). Bhaskar's philosophy elucidates the emergent properties of social structures, and has been important to human geographers (David O'Sullivan, 2004, 287). For the full "wedding" of Bhaskar's critical realist ontology and epistemology to their dissipative systems paradigm, see Harvey and Reed (1997, 296, 298-301) and Byrne (1998, 35, 37).

As introduced above, another important component of Harvey and Reed's (1994, 393) proposed paradigm is the evolutionary hierarchy theory of Niles Eldredge and Colleagues (Eldredge, 1985, 1986, 1989, 1995, 2016; Eldredge & Gould, 1972; Eldredge & Grene, 1992). We will now delve a bit deeper into this hierarchy theory, via the use made of it by Harvey and Reed (1994, 395-407), also referred to by them as the canonical triadic structure of evolution.

In Eldredge's original triad, organisms mediate between two evolutionary hierarchies: (1) a genealogical or "information" hierarchy and (2) an ecological or "economic" hierarchy.[48] The diagram (see figure 5) of this triad shows that the two hierarchies converge in the organism, because it is both a propagator of genetic information and in interaction with ecosystems:

> "evolution is the outcome of interaction between biological entities involved in the two great classes of biological processes—(a) matter/energy transfer, and (b) the maintenance, transmission, and modification of genetically based information.. . . Evolution is a result either of (a) changes in the information system itself (e.g., mutation seen as simplistic copying error) or of (b) input from the economic sphere that biases information transfer—as in the classical notion of Darwinian natural selection" (Eldredge, 1986, 3521; D. L. Harvey & Reed, 1994, 393).

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[48] Note that in this context "economic" and "ecological" are interchangeable terms. Eldredge's point is to distinguish two classes of activity of all organisms: one pertaining to matter-energy transfer processes (the economic/ecological), and the other pertaining to reproduction (information/genealogical) (Eldridge, 1996, 194).

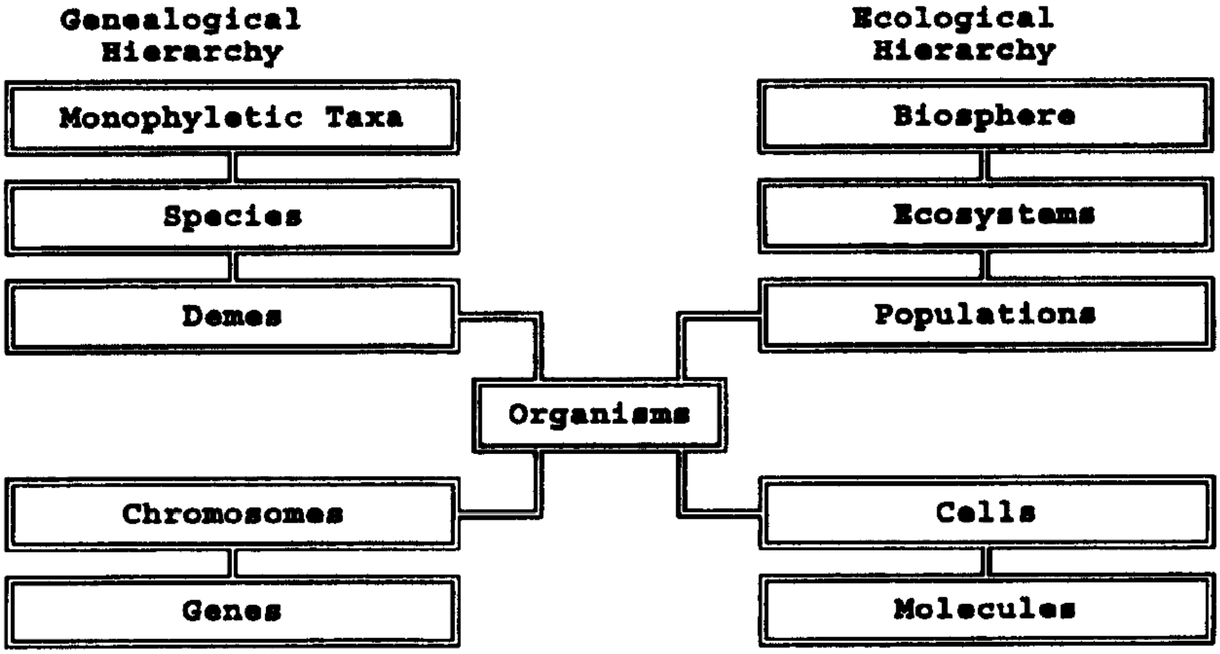

Figure 5 Eldredge's Dual Hierarchy of Evolutionary Processes (D. L. Harvey & Reed, 1994, 394)

Eldredge had already suggested that this triad is *canonical* and structures both biological and social systems: "all social systems emerge as reintegrations of the reproductive and economic activities of organisms" (quoted in D. L. Harvey & Reed, 1994, 396; Eldredge, 1989, 180-181). Harvey and Reed respond to this suggestion by treating the triad metaphorically as a fractal or "iterative mechanism generating at different ontological levels a series of self-similar patterns...under the constraints of an increasingly complex phase space" (D. L. Harvey & Reed, 1994, 396). Thus, figure 6 shows, three further levels are added to Eldredge's triadic hierarchy of biological evolution, each with components that are functionally homologous to the levels below them: (1) emergent human evolution; (2) multilinear social evolution; and (3) dynamic structures of dissipative social systems per se. Note that all levels of this pyramid diagram are predicated upon—and contain elements that are transformed from—the levels below them. In addition to the three levels Harvey and Reed add above the biological level, they put the biological level upon a foundation of the domain of thermodynamically organized dissipative systems, as a necessary foundation for the emergence of life (D. L. Harvey & Reed, 1994, 398).

Each level is distinct in that they have different evolutionary content; they are located differently on a chain of emergent structures that is irreversible; and each is a product of transformations of the rules of evolution—a “symmetry-breaking” “evolution of evolution” itself. Upward arrows signify a “hierarchy of conditioning”—a process whereby the function of the level above is produced and determined by that below it. Downward arrows signify a “hierarchy of control”—cybernetic controls that an emergent level exercises on all those below it (D. L. Harvey & Reed, 1994, 398).

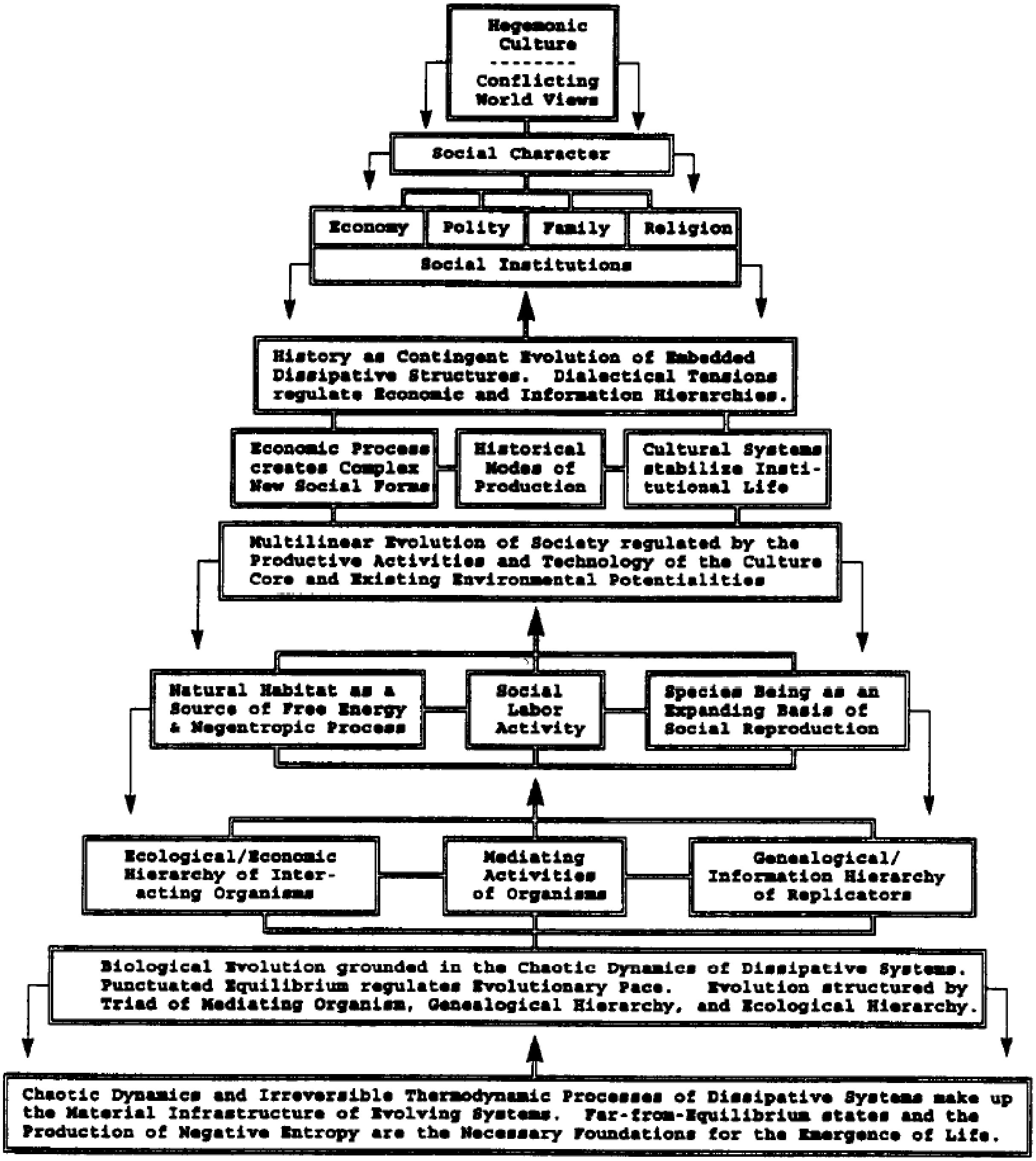

Figure 6 Harvey and Reed's additions to Eldredge's Hierarchy of Evolutionary Processes (D. L. Harvey & Reed, 1994, 397)

The first new iteration taking the place of the mediating organism in the hierarchy is now social labor activity. Leaning on Marxist anthropology, with its conception of nature transformed by human labor as the necessary condition for society's existence:

> The organism has now been replaced by a collective agency, communally-based social labor. The genealogical/information hierarchy, in like fashion, has been translated into the species of human being—i.e., that combination of human plasticity and open biogenetic character that sets humans apart from other life forms. Likewise, the ecological/economic hierarchy in turn now assumes the form of "nature," the necessary object of labor's transformational activities (D. L. Harvey & Reed, 1994, 399).

In building upon Eldridge's triad in this way, Harvey and Reed draw on the idea of "symmetry breaking" from the physicist Philip W. Anderson (1972)—another Nobel laureate and luminary of complexity science. They quote him at length to the effect that symmetry-breaking is at the heart of the structure of the physical universe—"a process by which new levels of reality emerge which require new non-reductionist forms of explanation" (D. L. Harvey & Reed, 1994, 386-387).

In the present context, cooperative human labor is a symmetry-breaking act that changes the terms of evolution (D. L. Harvey & Reed, 1994, 400). In other words, the self-transforming possibilities of social labor—encompassing the potential for rationality and foresight—extends evolution onto a new, cultural course. And social labor is the link connecting the uniquely human cultural reproductive processes to its source of energy: nature. "Tools and technology…become the instruments by which ever-more elaborate environmental adaptations and negentropic-based organizational complexity emerge” (D. L. Harvey & Reed, 1994, 400).

To round out this anthropological conception of social labor, a reinterpreted form of Julian Steward’s cultural ecology is plugged in. Harvey and Reed write:

> we need a theory of social evolution compatible with dissipative systems theory, chaos theory, and the productivist ontology of Marxist theory, but one that can simultaneously allow for the empirical diversity that social evolution has taken historically…Julian Steward’s (1973 [1955], 1977 [cited in Harvey and Reed]) cultural ecology…and the theory of multilinear evolution associated with it fit the bill on all three counts (D. L. Harvey & Reed, 1994, 397).

Steward’s primary thesis of a “culture core”—“material practices and normative commitments pertaining to everyday sustenance activities” (D. L. Harvey & Reed, 1994, 401)—is, for Harvey and Reed, a more sophisticated equivalent to Marx’s “mode of production.” Changes in core practices of material production eventuate changes in the other, normative areas of society. Importantly, however, in Steward’s theory of multilinear evolution, there is no series of fixed stages that all cultures go through, as in vulgar versions of historical materialism.[49] “A culture core configured in one way, and perturbed at one point in history, will take one evolutionary path; while quite similar configurations, when perturbed at a different

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[49] There is abundant evidence that Marx himself did not hold the belief in these fixed stages, *at least* in the last decade of his life (Shanin, 2018). But this has been largely ignored by Marxists.

point in time and space, may be sent in a radically different direction" (D. L. Harvey & Reed, 1994, 401).

At the next level of the diagram—social evolution—Eldredge's framework is further synthesized with that of Marx. "As elements of human history and its unfolding, the two evolutionary hierarchies become the internal institutionalized anchorings of society. The mode of production now mediates between the contradictory requirements of society's replicative moment, on the one hand, and its dynamic adaptive demands, on the other" (D. L. Harvey & Reed, 1994, 403). The economic part of the hierarchy has one foot in natural ecology and one foot in institutional order, and the domain of cultural replication has a relative autonomy, enabling people to sometimes act in conflict with ruling factions.

At the highest level of the pyramid, Eldredge's triad reappears once again, but this time disguised as integral complexes of the institutions of economy, polity, family, and religion (or ideology). The first two are functionally homologous to Eldredge's ecological hierarchy, and the last two are homologous to the geneological hierarchy. Social character and hegemonic structures of culture are further expressions of evolution at this level (D. L. Harvey & Reed, 1994, 404). Social character—or personhood—plays the mediating role here and is divided into two aspects, following the formulation of George Herbert Mead (Mead, 1972 [1962]): (1) an "objective moment of the 'Self' as an 'intersection' of institutional roles manifesting themselves in the unifying pragmatics of individual or group problem solving"; and (2) an "indeterminant aspect of the Self…free, spontaneous, and morally autonomous...above all else, the social system's wild card—society's own interior source of symmetry-breaking innovation....an ever present threat to the status quo ante, and at the same time a wellspring of community renewal and adaptive refinements” (D. L. Harvey & Reed, 1994, 404-405). At the apex of the pyramid is culture—always a conflicted process of defining and reproducing a totalizing system of beliefs, values and ideas from one generation to the next. It requires the qualifier “hegemonic” in the case of modern societies because of their unique dynamics of class struggle and structural complexities (D. L. Harvey & Reed, 1994, 406).

Tensions Within Complexity Science

This presentation of Harvey and Reed’s main theses has been quick and surface-scratching due to space constraints. But I hope the gist of it is clear. I now turn to discuss the implications of their paradigm for various aspects of the present thesis, and for some broad debates and tensions within complexity science.

Ilya Prigogine is dear to Harvey and Reed as a luminary of complexity science. Many others share this, including geographer, Doreen Massey:

> The assumption that non-simple aspects of the world were in principle reducible to simple systems (or, in terms of knowledge-production, would need to be if 'scientific' knowledge were to be gained from them), that they were really simple systems with too much 'noise' in them, prevented them from being addressed in their own right as complex systems. As is now being ever more frequently argued in a range of fields, the move from an assumption of simplicity to a recognition of complexity (with openness, feedback, non linearity and a move away from simple equilibrium) can change the picture entirely, to the point of thoroughly undermining many of the conclusions arrived

> at through the analysis of simple systems alone. Prigogine and Stengers (1984 [cited in Massey]) and Prigogine (1997 [cited in Massey]) argue this point at some length, expanding it to make the wider observation that an overconcentration on simple systems might, at least on occasions, have led us thoroughly astray (Massey, 1999, 265).

Elsewhere, Prigogine—or chaos theory (Douglas Kiel & Elliott, 1997), with which he is closely associated—is considered a “spiritual ancestor” (O’Sullivan, 2004, 283) of complexity science. However, in Harvey’s (2001) account, there are many in the Santa Fe Institute (SFI)—plausibly the most influential hub of complexity science in recent decades—who would rather that complexity science not be associated with Prigogine, chaos theory, or dissipative structures at all.

Harvey perceives two approaches to complexity with profound differences (which he calls Chaos Theory and Complexity Theory) but which must not be separated:

> Chaos Theory and the Complexity Theory both have a common ontological field of investigation: nonlinear systems and their evolutionary elaboration over time. They differ in that the Complexity Theory of the Santa Fe Institute is currently concentrating its energies on mathematically modeling the inner structuration or internal subsystem of complex systems, while Chaos Theory as articulated by Ilya Prigogine and the Brussels School have used models from statistical, far-from-equilibrium thermodynamics to study the external system of complex systems. To the extent this characterization holds, the two can be seen as holding complementary positions (D. L. Harvey, 2001).[50]

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[50] This is an online publication without page numbers, but one can search for the quotes.

However, the styles of scientific praxis and the social agendas that these two approaches to complexity pursue can be quite distinct. According to Harvey:

> those currently leading [complex adaptive systems (CAS)] research [through SFI] are still sunk in methodological individualism and a reactive conception of agency, human or otherwise. No matter how many emergent levels CAS modeling efforts have been able to simulate…this perspective has yet to effectively produce a systemic model in which a whole/part interaction based upon either negative feedback or positive feedback (or preferably both) can generate a holisitc conception of a self-regulating or far-from equilibrium system capable of saltational possibilities (D. L. Harvey, 2001).

For Harvey, “the assumptions and modeling methods employed by CAS researchers may be severely limited when it comes to studying human agency, society, and history” (D. L. Harvey, 2001).

But if CAS and chaos theory are “antipodes” to Harvey, he seeks their reconciliation. “In that complex dynamic systems are functionally and structurally differentiated into internally replicative and externally oriented adaptive subsystems, both chaotic and complexity moments of the New Science are required” (D. L. Harvey, 2001). And the path to reconciliation Harvey recommends is through the punctuated equilibria framework for evolutionary processes, which “fully accommodates the internal/external division of dissipative systems theory and has shown its capacity to be applied heuristically and metaphorically to large-scale social and cultural systems” (D. L. Harvey, 2001).

As stated earlier, the theory of punctuated equilibria holds that the evolution of new species occurs quite rapidly for a geological time scale (5,000-50,000 years) (Eldredge, 1995, 99), emerging from marginal populations that become geographically isolated (Eldredge & Gould, 1972; S. J. Gould & Eldridge, 1977; Harvey & Reed, 1994, 392). “Punctuated equilibria is simply the notion of speciation applied as the explanation for evolutionary change interrupting vastly longer periods of monotonous stasis” (Eldredge, 1995, 97). Quite controversial in the 1970s and 1980s—including a brief firestorm of accusations within paleontology that it was Marxist propaganda (Eldredge, 1995, 101-102)—punctuated equilibria has been vindicated by the empirical paleontological record, and is now standard theory in biology. Prigogine embraced punctuated equilibria theory, declaring it “in complete accord with the results of far-from-equilibrium thermodynamics” (Prigogine, 1997, 162). And Prigogine goes further, inviting us to understand biology and even society as manifesting and amplifying fundamental properties of far-from-equilibrium thermodynamics:

> Irreversibility, and therefore the flow of time, starts at the dynamical level. It is amplified at the macroscopic level, then at the level of life, and finally at the level of human activity. What drove these transitions from one level to the next remains largely unknown, but at least we have achieved a noncontradictory description of nature rooted in dynamical instability. The descriptions of nature as presented by biology and physics now begin to converge (Prigogine, 1997)(quoted in D. L. Harvey, 2001; Prigogine, 1997, 162).

Gintis—a professor of the SFI—fiercely argues that human behavior must be situated vis-à-vis our biology—which, insofar as it concerns human and animal behavior, is one of the six behavioral sciences that Gintis insists needs to be integrated. This integration of biology into a general behavioral sciences framework must overcome immense obstacles posed by many in a variety of social sciences denying the validity of biological approaches to sociality, and Gintis is veritably on the attack on this point.[51] But what about the need to situate human history vis-à-vis natural history, on a grand paleontological scale? And must biology itself be situated vis-à-vis dissipative systems, as Harvey, Reed, and Prigogine claim? It seems plausible that these central ideas of Harvey and Reed can be integrated into Gintis's paradigm in a way that is quite consonant.[52] And what of Harvey and Reed's nesting, in the same way, of the domain of dissipative systems in relation to the domain of deterministic chaos more generally? It is entirely possible that I have missed something crucial that makes all this incompatible with Gintis. But from what I can gather, the silence from Gintis and many others affiliated with the SFI on dissipative systems, chaos, and punctuated equilibria may stem more from intellectual fashions and aesthetics than from a lack of validity of the math or physics involved.[53]

And what of Harvey and Reed's extension to Eldredge's triadic hierarchy of evolution? Here I suspect Gintis may object to Harvey and Reed's Marxism. As discussed above, Gintis is most well-known for his early work when he was Marxist , but he tries to make it emphatically clear that he has repudiated his youthful love of Marx (Gintis, 2009b, 28:45 in video). But I also suspect that the very non-orthodox use of Marx in Harvey and Reed may be unfamiliar to

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[51] See Gintis (2009b).

[52] But see the discussion on sociobiology below.

[53] Note that Harvey portrays a mixed reception at an SFI conference in 1994, where some prominent voices spoke highly of punctuated equilibria theory, but for the most part "the idea was usually pooh-poohed and the exchange politely diverted to more commodious subjects" (D. L. Harvey, 2001). However, for whatever it may be worth, the SFI did publish a book a couple of years later with Eldredge's (2003) contribution as the opening chapter.

Gintis. What Gintis really seems to object to is what he sees as the *mechanistic* approach of Marx, but this is what Harvey and Reed call "the more hidebound readings of evolutionary theory that have plagued past interpretations of historical materialism" (D. L. Harvey & Reed, 1994, 391). These, for Harvey and Reed, can and must be shorn.

What does Harvey and Reed's paradigm tell us about the emergence of capitalist relations? At the least, their use of Steward's multilinear evolution concept clearly implies that there is no such thing as inexorable progress in one direction or another. As far as what happened with the Lauje highlanders, Harvey and Reed's adaptation of punctuated equilibria, suggesting that social change can come in swift leaps, does seem to fit the Lauje's case. In general, it seems that this theme of emergence of capitalist relations has not been much taken up by the small group of "complexity Marxist" scholars (Williams, 2020, 60),[54] who are more oriented to using Harvey and Reed's paradigm to understand current trends and thinking strategically about contemporary political interventions. There are interesting takes on neoliberalism as a form of complexity here, for example, in Williams (2020, 195). Further work might explore using Harvey and Reed in connection with Moore and Patel et al.'s "World Ecology"[55] (Moore, 2015; Patel & Moore, 2017), or other schools of Marxian theory, to think about the emergence of capitalism on various scales.

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[54] For Williams, this school of thought includes Byrne (1998), Byrne and Callaghan (2014), Williams (2020) himself, and Harvey and Reed (1997, 1994). Paul Prew (2019) should also possibly be added—he is unique in that his "sociopoesis" concept emphasizes and builds upon another complexity concept I have not had time to discuss—autopoesis, derived from Varela's (1981) thought on the self-sustaining nature of organisms.

[55] Moore's (2015) framework of "World Ecology" is based upon "World-systems theory" (I. Wallerstein, 1974)—but seeks to synthesize political economy and ecology—for whom Immanuel Wallerstein is an important figure. According to Prew (2019), Wallerstein was an early adaptor of Prigogine's ideas. See also *Open the Social Sciences: Report of the Gulbenkian Commission on the Restructuring of the Social Sciences*, co-authored by Wallerstein, Prigogine, and others (Immanual Wallerstein et al., 1996).

It is notable that Harvey (2001), at least, has criticized the SFI's emphasis on simulation, and he and Reed seem to have no interest in it. Some of the other complexity Marxist scholars seem to differ, though. Byrne (1998, 62, 81), e.g., sees it as promising. I see no reason why simulation methods would be inherently incompatible with Harvey and Reed's paradigm.

Returning to the comparison of Gintis's paradigm with that of Harvey and Reed, what would the latter make of Gintis's transdisciplinary behavioral sciences paradigm? This requires a closer examination of the relationship between the various threads of developments in biology that are utilized by these two paradigms. Upon first encountering DIT, PET, Eldredge's hierarchical evolution theory, the clash of E.O. Wilson, Nowak, and Tarnita (2010) against Hamilton's Rule, and all the new developments that Stallins (2012) delineates as bringing the recent demise of genetic determinism and ushering in a new organism-environment biology, one might think that all the above would be converging harmoniously all along. However, as mentioned earlier, DIT and Eldredge and colleagues' evolutionary hierarchy theory—both developed about the same time—can be seen as competing with each other as alternatives to the dominant sociobiological paradigm (Wilson, 1975). Note that Eldredge would probably disagree with me on this point: whereas in their seminal book, *Culture and the Evolutionary Process,* DIT theorists Boyd and Richerson (1985, 14) saw their work as distinct from, and in fact eclipsing the sociobiology of E.O. Wilson and others, Eldredge and Grene (1992, 186) dismissed DIT as "sociobiology with a new wrinkle, nothing more." While Eldredge and colleagues (Tëmkin & Eldredge, 2007) later criticized DIT in a somewhat more constructive vein, the two camps seem to barely maintain diplomatic relations and as far as I can tell have never fully hashed out their differences.[56] [57]

Stephen J. Gould (whose ideas are central to Harvey and Reed) was famously at odds with the founder of sociobiology, E.O. Wilson (whose ideas are arguably central to Gintis). However, note that the debates in biology about social systems that Gintis intervened in (Gintis, 2012; M. A. Nowak et al., 2010; Yee, 2014) did not even start raging until 2010. And what made these debates so dramatic was that with that 2010 paper, Wilson (M. A. Nowak et al., 2010) made an about-face from having been, in his own words, a “convert” (Zimmer, 2021) to Hamilton’s Rule since 1963. Not only was he a convert for nearly fifty years, Wilson grew to be like the pope of Hamilton’s Rule. Then he became its most famous apostate.

To return to our question of what would Harvey and Reed would make of Gintis’s transdisciplinary behavioral sciences paradigm, it may come down to a question of whether any kind of reconciliation is possible between Eldredge and Gould’s ideas and those of Wilson’s. It would be very interesting to know if Eldredge has been moved by Wilson’s reversal. As far as I know, he has not commented on it publicly.

As discussed above, practitioners of DIT initially also framed themselves as critics of Wilson’s sociobiology as well. But if Gintis’s joint work on behalf of DIT and at least the later,

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[56] The closest thing I have found to a systematic critical examination of DIT by scholars who *might* be presumed to be in some kind of a “camp” with Eldredge (because both Lewontin and Eldredge collaborated with Gould, and all three are famous for opposing genetic determinism in disputes with Wilson’s sociobiology) is a paper by Joseph Fracchia and Richard Lewontin (1999). It is a rather hostile and provocative attack on DIT, but again, it is hard to know how much it is representative of Eldredge and colleagues’ critiques of DIT.

[57] I have only been able to find one (dismissive) mention of Eldredge in Boyd and Richerson’s works (2005, 291-292).

post-Hamilton's Rule, Wilson, is any indication, it appears DIT and Wilson have made their peace. Indeed it would seem that Wilson, by rejecting Hamilton's Rule, may have conceded to the critics of genetic determinism. Because Eldredge was an early champion of many of the developments, such as epigenetics and proteomics, that Stallins (2012) writes of undermining that old paradigm in the decade after the Human Genome Project began, within the framework of the "Extended Evolutionary Synthesis" (Brooks, 2011; Eldredge, 1985, 1995), perhaps it is reasonable to expect that a reconciliation between Eldredge's and Wilson's ideas could be possible.[58]

Whether or not Harvey and Reed would accept Gintis's insistence on the importance of game theory and the rational actor model is another matter. Consider this moment of self-deprecating humor where Harvey takes a jab at SFI founder Gell-Mann's approach to mathematics:

> [Gell-Mann said] 'we need to pay much more attention to society, to social science, to try to find those few social scientists who don't suffer from crippling math phobia but are, nevertheless, not the kinds who trivialize social problems by mathematizing them,'….As a bona fide mathophobe myself (I attend weekly meetings of Numbers Anonymous) there is a measure of truth in his statement. At the same time, by taking this Procrustean stance, Gell-Mann finds a convenient way to ignore the possibility that

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[58] There may, however, remain deeper problems that Eldredge and colleagues have yet to voice about DIT and Wilson (post-Hamilton's Rule). Wilson's earlier sociobiology work stirred up such controversy, critiques, and charges of racism (Allen et al., 1975; Gould, 1996) that it would not be surprising if points of contention remain, whether personal or substantive. Furthermore, occasional elements in Gintis's work, e.g. his discussion of a "likely genetic predisposition underlying sociopathy" (Gintis, 2014, 200), harking back to incendiary sociobiological statements, will no doubt raise some eyebrows—these sorts of arguments having a long history of justifying disproportionate rates of incarceration by race, etc.

> the assumptions and modeling methods employed by CAS researchers may be severely limited when it comes to studying human agency, society, and history (D. L. Harvey, 2001).

My interpretation is that Harvey is striking out at what he sees as a kind of math-supremacist orientation in the SFI. Harvey is not really a “mathophobe”—Harvey and Reed make extensive use of mathematics in their presentation of deterministic chaos (D. L. Harvey & Reed, 1994). Harvey and Reed also affirm statistical and predictive modeling in their field of sociology, where appropriate (D. L. Harvey & Reed, 1997, 309). However, they do see areas where these methods are not appropriate, and defend iconological modeling— a kind of “pictorial method… [using] visual correspondences rather than deductive reasoning” (D. L. Harvey & Reed, 1997, 309)—to better understand chaos theory, for example.

After the above jab at Gell-Mann, Harvey continues:

> One need merely peruse Prigogine and Stenger's (1984) landmark statement, *Order out of Chaos*…to recognize the difference between the CAS agenda and Chaos-based forms of inquiry. First, the subtitle of Prigogine and Stenger's volume, when translated into English is 'Man’s New Dialogue with Nature', a phrase signifying an expected sea-shift in how intellectual life generally, not just the physical sciences, would be done in the future (D. L. Harvey, 2001).

This bespeaks a view towards social transformation that entails a radical epistemological break with critical/quantitative dualism, to say the least—which may or may not be compatible with Gintis’s view on the place of game theory and the rational actor model.

Chapter Three—Conclusion

This thesis started out with the argument in Chapter One, that simulation can be a powerful supplement to interpretive methods like ethnography, helping us understand complex social and environmental systems. I focused on ABM and attempted to model the dynamics of swidden agriculture in a common property system, and how this system could give way to enclosures and the emergence of capitalist relations (or not). Specifically, the goal was to use ABM in connection with Li's (2014) ethnography of the Lauje highlanders.

From the beginning of this project, I have been as interested in debates about the philosophy, history, epistemology, and politics of modeling in geography as I have been interested in modeling itself. It was notable to me that ABM is an important new methodology that has seen very little use in critical geography. As I explained in Chapter One, there are reasons for this, rooted in a deep-seated, historical, critical/quantitative dualism that remains difficult to overcome. Critical GIS is perhaps the most exemplary case of an attempt to overcome critical/quantitative dualism. It is also exemplary as a project of sustained, critical, collective inquiry into a form of technology. This entails two things. First of all, it entails getting our hands dirty and putting a form of technology to different uses than it was intended for. GIS—like analytical computing paradigms generally—was largely engineered to serve capital accumulation and state power (J. Thatcher et al., 2016, 819). But we have seen it can be "hacked" for uses in critical social science, as well as for political interventions toward social justice. Secondly, such a sustained, critical, collective inquiry entails engaging in a political-economic and social historical analysis of the technology itself. The case was made that an anologous "Critical ABM" has been incipient in the works of a small group of scholars, and should be further cultivated.

While my ABM[59] itself cannot be seen as a groundbreaking success, it is a start at pathbreaking, and together with the theoretical foray that accompanies it, it may open up vistas of the terrain that can inform future work extending this or similar paths. That theoretical foray led me to discover the work of Lansing and Kremer (1993) and Downey and colleagues (S. S. Downey et al., 2020), for example. With works like these as sign posts and guides at the beginning of a trail, one could make it much further than my ABM was able to go.

More generally, that foray led me to discover many facets of complexity theory such as the work of Gintis (2014), Harvey and Reed (1997, 1994), and Stallins (2012). To my knowledge, no one has yet tried to hold any of these facets together to see how they may fit, or to propose adjudicating their differences. My attempt at this may prove to be the bigger contribution of this thesis, quite aside from arguments about simulation.

It is possible that I have become infected with a mania for synthesis from reading too much Gintis. My hunch, however, for which I have argued, is that gene-culture coevolution and Eldredge's hierarchical theory of evolution can be viewed as converging, despite their history of mutual antipathy. Let us assume for the moment that this argument is correct, a synthesis of the two is possible, and name such a hypothetical synthesis the hierarchical theory of gene-culture coevolution (HTGCC) for now. An HTGCC synthesis could replace the genetic-determinist paradigm of sociobiology which has dominated our understanding of the biological context of sociality since the 1970s. Such a development would align well with the myriad other developments—from genomics and proteomics to niche-construction, recounted by Stallins

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[59] Again, available here: https://github.com/FramL/Commons_Enclosures_Complexity

(2012)—which over the last twenty years have stormed genetic determinism in biology in general; not to mention the transformation of the disciplinary landscape effected by E.O. Wilson's dramatic repudiation of Hamilton's Rule.

To make clear the implications of this synthesis for geographers, I will repeat a quote from Stallins but then riff on it. "When biologists describe the genotype-phenotype-environment linkage spanned by the omics, they are invoking a spatial dynamism that is geographical, perhaps more so in the tradition of recent human geographic scholarship on scale and environmental causality. An organism is an ongoing, contextual outcome of a causality that propagates among molecular, cellular, organismal, and environmental scales" (Stallins, 2012, 432). Human organisms are ongoing contextual outcomes of such a causality, too, but culture must be factored into that environmental scale as well. That is what gene-culture coevolution offers to do by treating culture as an environmental niche that humans construct.

Meanwhile, Eldredge's theory of a dual evolutionary hierarchy, with ecological as well as genealogical dimensions, has long argued for a revised and expanded biological ontology, precisely to emphasize important aspects of the ecological dimension—which "represents dynamics of matter and energy exchange, and, generally, corresponds to the spatial dimension of life" (Eldredge, 2016, 13). In Eldredge's view, the dominant nonhierarchical ontology of orthodox "ultra-Darwinism" (Eldredge, 1995, x) is blind to this spatial dimension. With its genetic determinism, ultra-Darwinism is also more generally blind to the nested hierarchical structure of biological systems that makes them have this "causality that propagates among molecular, cellular, organismal, and environmental scales," to quote Stallins (2012, 432) one last time. Furthermore, with its understanding of the separation of the genealogical and ecological hierarchies, and their *reintegration* in social systems, Eldredge's framework illuminates the biological context of sociality in a way that arguably expands the vision of gene-culture coevolution.

If such an HTGCC synthesis is valid, that would remove a significant barrier to the synthesis of Gintis's (2014) transdisciplinary behavioral sciences paradigm and Harvey and Reed's (1994) dissipative social systems evolution approach. Most other elements such as concepts from Prigogine's dissipative structures (Prigogine & Stengers, 1984) and Eldredge and Gould's (1972) punctuated equilibria, while not currently fashionable with the SFI, are arguably not only compatible with its version of complexity theory but necessary to round it out (D. L. Harvey, 2001). All of this has been argued in more detail in Chapter Two.

A major sticking point remaining in the way of such a synthesis is the question of whether Gintis's (2014, 209, 212) arguments in favor of the rational actor model and game theory can be acceptable to people like Harvey and Reed. It would be a stunning conversion, most likely, for critical social scientists nursed in fields like sociology that tend to reject such ideas.[60] I myself remain undecided exactly what to think on this matter. Gintis (2014, xi) makes an intriguing defense of game theory and the rational actor model, however, combined with an attack on the limitations and the reigning culture of these fields, including blindness to the crucial contributions of other disciplines like sociology.

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[60] While David L. Harvey was a professor of sociology, Michael Reed was actually a professor of economics, trained in game theory and the rational actor model in the late 1960s. He was completely unconvinced that the rational actor model makes any sense, when I talked with him in September 2022 (Reed, 2022). However, he was not familiar with epistemic game theory or other such recent developments that are crucial for Gintis's (2014b) unusual interpretation on these matters.

What is to be gained by integrating Gintis's and Harvey and Reed's paradigms? If Harvey's (D. L. Harvey, 2001) argument is valid, there has been a polarization between two approaches to complexity: the Chaos Theory approach of Prigogine and the Brussels School (and I would add, Harvey and Reed themselves) and the Complexity Theory of the SFI (including that of the external professors of the SFI Gintis, Samuel Bowles, and Stephen J. Lansing). For Harvey, these approaches should be seen as inseparable and complementary, however. The latter focuses on "modeling the inner structuration or internal subsystem of complex systems, while Chaos Theory as articulated by Ilya Prigogine and the Brussels School have used models from statistical, far-from-equilibrium thermodynamics to study the external system of complex systems" (D. L. Harvey, 2001). See chapter two for more detail.

Despite Prigogine's Nobel laureate prestige, it seems the Chaos Theory approach has fallen out of fashion, or for whatever reason complexity science practitioners of the SFI like Gintis do not see it as valid, or simply ignore it. And despite Harvey's (D. L. Harvey, 2001) argument for complementarity, practitioners of Harvey and Reed's paradigm appear to be somewhat cut off from the mainstream of (SFI-style) complexity science. Hence their dissipative social systems evolution approach has remained marginal, with only a handful of adherents (Byrne & Callaghan, 2014; Williams, 2020). For them, there have been no million-dollar MacArthur Foundation grants funding a research agenda spanning the globe, such as Gintis and colleagues have been able to secure (Henrich et al., 2004). For these complexity Marxists, therefore, an integration with Gintis's paradigm could potentially open a world of possibilities of different techniques in the mainstream of complexity theory, giving them much wider exposure.

What does Gintis or his paradigm have to gain from such a synthesis with Harvey and Reed's? If Harvey (D. L. Harvey, 2001) is correct that Prigogine's dissipative structures (Prigogine & Stengers, 1984) and Eldredge and Gould's (1972) punctuated equilibria, while not currently fashionable with the SFI, are arguably not only compatible with its version of complexity theory but necessary to round it out, as dilineated in Chapter Two, then Gintis and other complexity theorists may have much to gain from this proposed synthesis. In apparent indication of the truth of this line of thought, Gintis (2014a, 24:10-16:00 in video) has acknowledged that non-equilibrium dynamics are both not possible for him to model currently, and ubiquitous in human social dynamics. And the dynamics non-equilibrium systems is precisely the forte of Prigogine.

I hope that with this project—by examining the epistemology, history, and politics of modeling in geography; exploring the potential integration of cutting-edge developments in complexity science; and making a first foray into using ABM in political ecology with an application of computer simulation of the dynamics of commons, enclosures, and capitalist relations—I have helped advance the incipient theory and practice of a "critical ABM," and perhaps more generally the theory and practice of simulation and complexity science in geography.

Appendix

This appendix gives details on the agent-based model (ABM) of the emergence of capitalist relations among the Lauje highlanders of Sulawesi, Indonesia, 1990-2010. I follow the "Overview, Design concepts and Details (ODD) protocol for describing Individual- and Agent-Based Models (ABMs)" (Grimm et al., 2020). There is some redundancy here, but for much more general information and background on the Lauje, see the section entitled "Commons and Enclosures" in this thesis. The model can be downloaded here: https://github.com/FramL/Commons_Enclosures_Complexity

1. *Purpose of the Model*

The model is based on the "Governing the Commons" model in Marco Janssen's book, *Introduction to Agent-Based Modeling: with Applications to Social, Ecological, and Social-Ecological Systems* (Janssen, 2020, page 214). Janssen's model gave a basic ABM of both Hardin's tragedy of the commons (Hardin, 1968) and how non-tragic outcomes can be found through self-government if cooperative norms are enforced. What the current model adds to that is an investigation of certain geographically specific, market-related threats to self-governed commons, and how these threats can be combatted.

2. *Entities, State Variables, and Scales*

Each "tick" represents a few years, the typical amount of time highlanders would stay in a clearing before letting it fallow (Li, 2014, 88). Each patch is 3 hectares, roughly the size of a highlander group's clearing (Li, 2014, 114).

The mobile agents—called highlanders—in the present model represent groups of Lauje highlanders, initially practicing swidden agriculture in a mostly non-market economy. Their main state variables of interest are:

A) their amount of "wealth"

B) their "norm-min-resource"—the minimum amount of resources on patch before they will harvest.

C) "cacao-grower?"—whether or not they grow cacao, entailing a change in land use and property relations.

D) "owned-patches"—a list of patches owned by cacao growers.

E) "market-distance"—their distance to a market.

F) "costpunished-c"—the losses suffered if punished for growing cacao.

G) "t-grower?"—whether or not they grow tobacco, a mid-level commodity crop which entails some amount of violation of social norms regarding over-harvesting, but not the fundamental property-relations change entailed by cacao-growing.

The following are highlanders variables used for calculations in the "machinery" of the model, discussed in the details section below:

H) "copied-norm"

I) "Harvestedlevel"

A non-mobile type of agent in the model that is nonetheless distinct from "patches"—in Netlogo parlance, a "breed" of "turtle"—is a "market." The market functions to specify geographically specific activities in the model but has no state variables of its own.

There is a grid of 101 X 101 patches which have the following state variables:

A) “resource-here”: the current amount of resource on this patch.

B) “max-resource-here”: the maximum amount of resource this patch can hold.

C) “cacao-crop?”: if true, the patch is privately owned by cacao-growing group of highlanders, known as a “c-grower”, and others can't go there.

D) “owner”: c-growers who own cacao-crop patches. This state variable is set to "Nobody" if cacao is not grown there.

E) “initial-resource”: the amount of resource-here upon initialization, used for some calculations in the model machinery, discussed below.

3. *Process Overview and Scheduling*

The grid of patches which is initialized upon starting each model run, which have different amounts of resources which regrow at a logistic rate. Highlanders are randomly placed upon the grid at initialization.

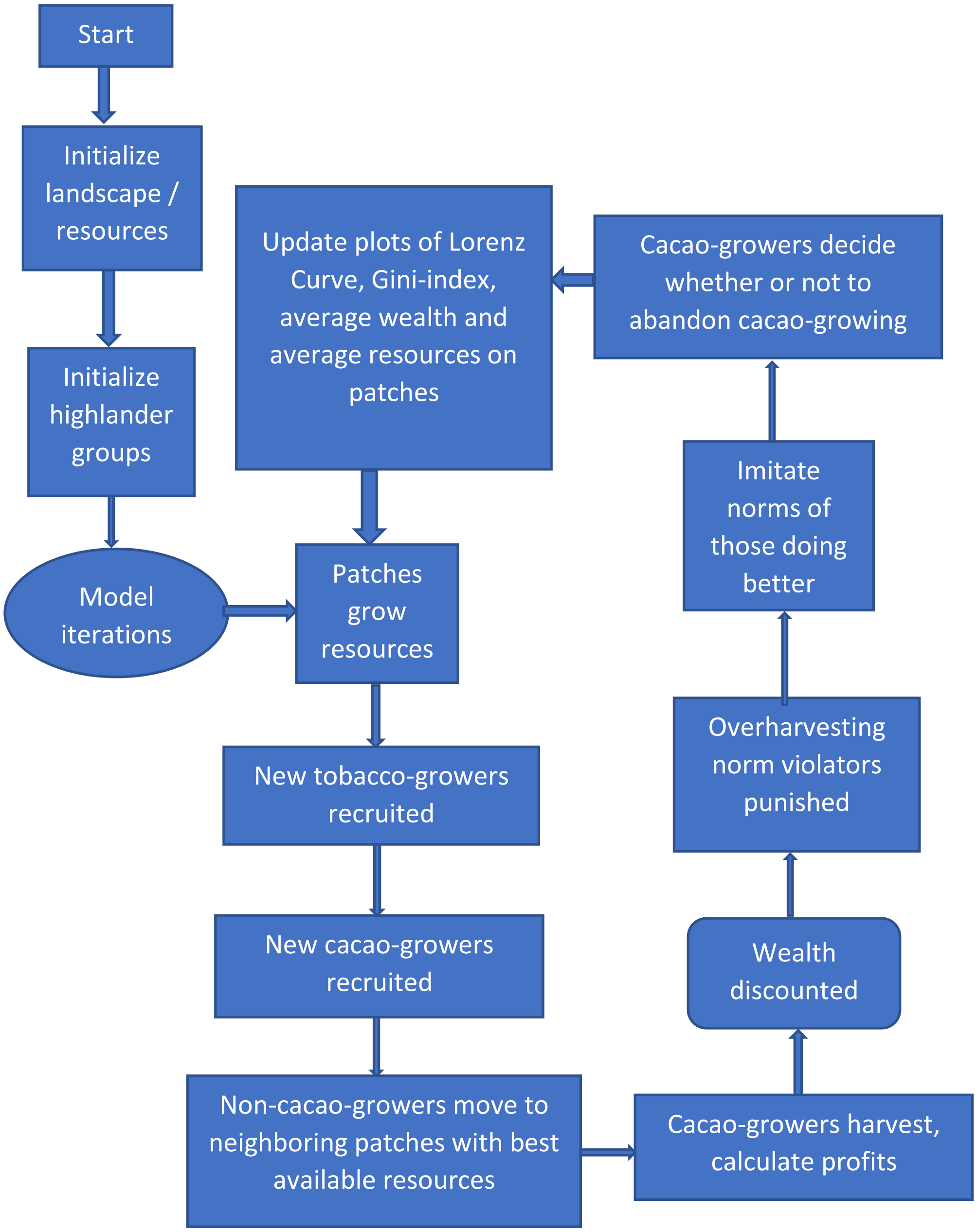

Figure A1—A flowchart showing an overview of the model. The left-hand shows model initialization. The right-hand shows the details for each model iteration.

Each tick begins with patches growing their resources at a logistic rate. New tobacco or cacao growers are then initiated if certain conditions are met. Newly accumulated farms by highlanders who are already growers are then acquired if certain conditions are met. Non-cacao growing highlanders then move to the neighboring patch with the highest resource available and harvest resources from that patch, which translates to wealth. Cacao-growers then move between their owned patches of cacao farms and harvest cacao. *Their* movement doesn't functionally matter in terms of what gets harvested but helps graphically to see who owns patches.

Non-cacao growing highlanders then go through a variety of processes of decisions and wealth processing: their wealth is discounted, according to a percentage set with a slider; they punish violators of social norms under certain conditions; they imitate the "norm-min-resource" of others under certain conditions; they change colors if they are tobacco growing; they punish cacao growers under certain conditions; tobacco growers decide if they are abandoning tobacco growing, a result of sufficient punishment to get them to revert to following social norms.

Cacao growers decide if they will abandon cacao depending upon certain conditions.

Global variables used for calculating graphs of the mean wealth, average norm-min-resource and resource-here values of patches are updated, as are plots of the Lorenz curve and Gini-Index of wealth.

There are five important global parameters that will be highlighted here: the switches labeled "cacao?", "tobacco?", "punish-traditional?", "punish-c-growers?" and a slider, labeled "accumulators." Tobacco represents a crop that highlanders grew in recent centuries with an intermediate amount of production for market. Cacao-growing, which only started in the 1980s, was produced solely for market.[61]

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[61] Highlanders in fact never used cacao and did not understand its utility, besides being a commodity for sale (Li, 2014, 123).

Submodel A

When the model is run with the default parameters, and with all switches “off” and the “accumulators” slider set to zero, what we see is the “tragedy of the commons” (Hardin, 1968). The resource collapses quickly and the wealth level of highlander groups plummets.

Submodel B

By turning “punish-traditional?” on, we have enforcement of norms against overharvesting, such that agents who are found to be harvesting from a patch that is at less than half its maximum resource value are punished by others who can afford the cost of punishing. In this way, resource collapse can be averted in the model for the equivalent of millennia. This represents a self-governed commons (M. Janssen, 2020, 214; Ostrom, 1990).

Submodel C

If “tobacco?” is now switched on, those within a certain radius of a randomly placed “market” are tempted by higher payoffs to grow tobacco, which is represented in the model by overharvesting.[62] Some of this mid-level commodification, when localized near the market, can coexist with a self-governed commons for the equivalent of many centuries. This matches what appears to have happened with Lauje highlanders (Li, 2014, 25).

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[62] The radius from the market, the rate at which agents are tempted to become tobacco-growers, and the amount they overharvest when doing so, can all be adjusted by sliders.

If “tobacco?” is now switched on from the beginning of a model run, the process that starts tobacco growing still does not start until after 170 ticks, which is about the amount of time it takes for submodel B to settle into an equilibrium.

Submodel D

When “cacao?” is switched on, highlander groups are tempted by much higher payoffs to start growing cacao. The closer they are to the market, the higher the likelihood of them starting to grow it. When they do grow cacao, they take a group of patches as private property and cease to circulate or share their patches with others.[63] With “accumulators” set to zero, we see equitable development of cacao farming. With that slider set higher, that number of highlanders per tick will accumulate more groups of patches for cacao farms. This can leave most highlanders without any resources. The former scenario could be seen as a cacao-promoting NGO’s dream, and the latter scenario is more like realism in the case of the Lauje highlanders (Li, 2014, 139).

If “cacao?” is now switched on from the beginning of a model run, the process that starts tobacco growing still does not start until after 200 ticks, to give some time for tobacco growing to start before cacao growing begins.

Submodel E

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[63] The number of patches they start growing cacao on is adjustable—it is set by adding the number on the “extra-cacao-patches” slider to a default initial patch.

Lastly, with “punish-c-growers?” turned on, agents can punish cacao-growers.[64] To an extent, this can represent the “weapons of the weak” (Li, 2014, 155; Scott, 1985), such as gossip, theft, arson and slander, that highlanders did actually use against cacao-growers. In the model, however, if we wish, we can turn up the dial on that punishment and adjust it until cacao-growing ceases. This can represent some of the “what if?” questions, alluded to earlier. When highlanders have greater awareness of the threat the cacao boom poses to their way of life, they can resist the destruction of their self-governed commons. This represents a counterfactual, hypothetical historical scenario.

4. *Design Concepts*

Using Elinor Ostrom’s game theoretic terms, the Lauje highlanders can be said to have achieved a non-tragic equilibrium in a modified prisoner’s dilemma game (Ostrom, 1990, 7), with the high level of cooperation in their traditional system. The dynamics of this cooperation forms a “self-governed commons” (Janssen, 2020, 216; Ostrom, 1990), where people have been able to use a common pool resource for multiple generations sustainably. The highlanders thus avoided a "tragedy of the commons" (Blaikie, 1985, 130; Hardin, 1968). And the high payoff of selling cacao can be seen as a temptation to “cheat” on this egalitarian equilibrium, fatally disrupting their control of collective land ownership.

The ABM developed for this thesis uses Ostrom’s framework to give us an opportunity to examine some of the different facets of this process, both to make sense of what has

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[64] The amount of punishment, how sensitive to punishment cacao-growers are, and the propensity to punish, are set by the “strength-of-c-punishment,” “percent-c-wealth-lost-to-punishment,” and “cacao-cheat-threshold” sliders, respectively.

happened, and to ask "what if" questions. It enables us to take a fresh look at some of the forces at play when self-governed commons come unraveled, versus staying resilient. For example, what if the Lauje had had greater awareness of the threat the cacao boom posed to their way of life? Might they have been able to resist the destruction of their self-governed commons, or at least postpone it? Is there any way that what has been lost could even be restored?

Cacao-growing has two different possibilities: an egalitarian form of development where agents only own one farm, and a form where the most wealthy accumulate more farms. The chart below gives an idea of the comparative behavior of these two forms, in terms of how long it takes for resource collapse.

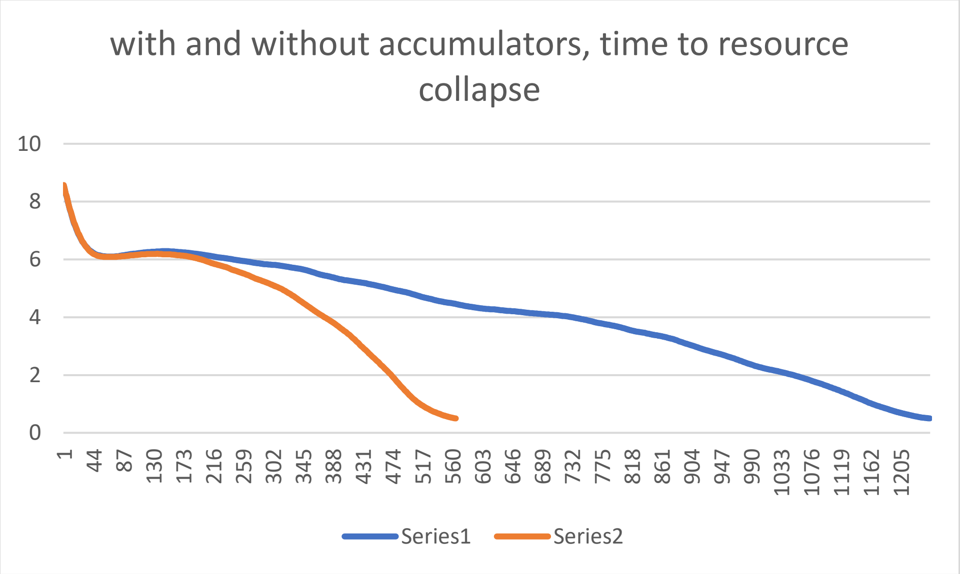

Figure A2—Series 1 is a run of the model without "accumulators" and series 2 is with them.

And the chart below gives an idea of the behavior when there is no cacao or tobacco, compared to when there is just tobacco.

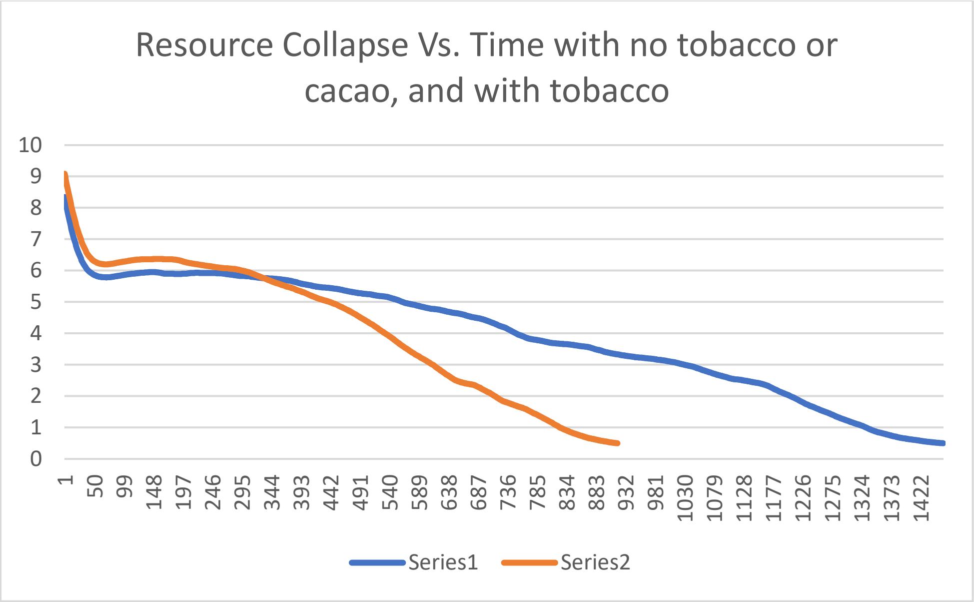

Figure A3—Series 1 is a run of the model without tobacco or cacao. Series 2 has tobacco, starting at 170 ticks.

A third chart shows comparisons of all crop options together:

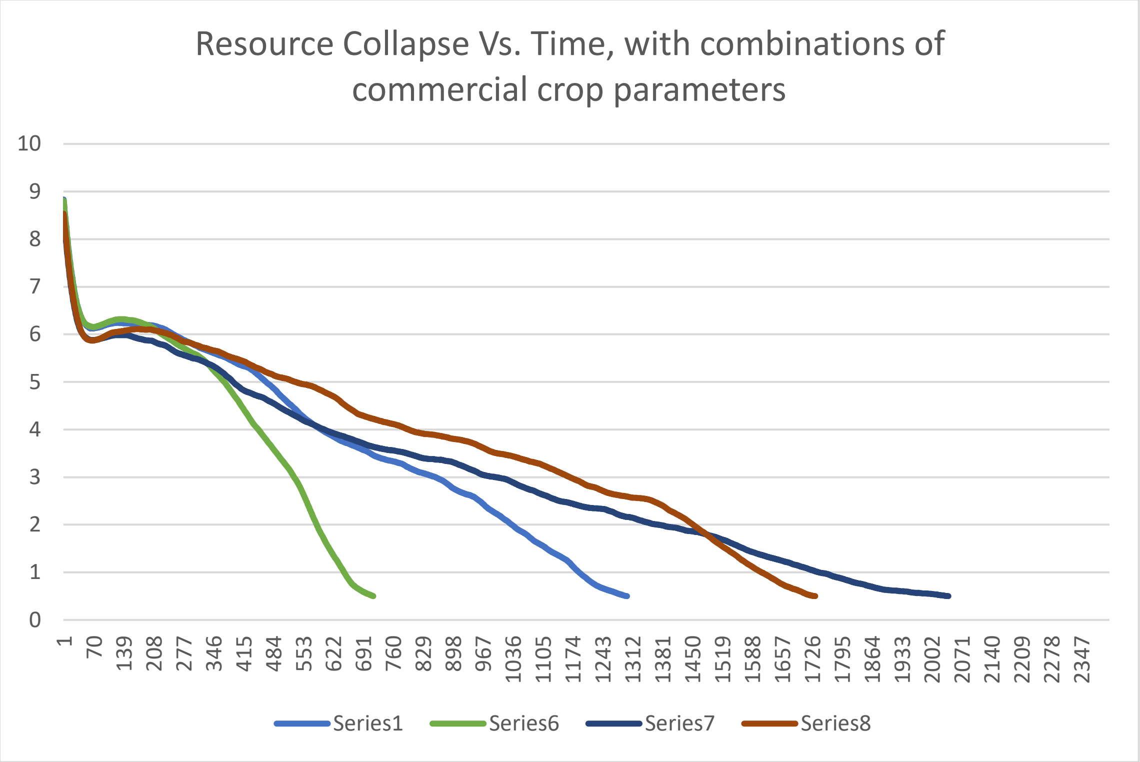

Figure A4—All crops compared. Series 1 now has no tobacco and no cacao. Series 6 has just tobacco. Series 7 has tobacco, cacao, but no accumulators. Series 8 has tobacco, cacao, and accumulators.

The last results may seem strange. No commercial crops does worse than if there is both tobacco and cacao (accumulators or not). Could one interpret this as cacao growers staying on their private farms having the paradoxical effect of freeing up other resources for others, giving the illusion of it not being a problem? But that might be a charitable interpretation, and it could be a problem with the way I modeled this.

Lastly, the model has a new component of punishment specific for the growers of cacao. This can be adjusted to mitigate or eliminate its effects.

5. *Initialization*

A) All state variables are cleared.

B) Maximum resource of patches is set to 50.

C) An amount of the patches, set by the “percent-best-land” (default 10%), is randomly selected as best land and given maximum resource values.

D) These patches diffuse 0.25 of their resources, which is repeated five times.

E) Then all patches further diffuse 0.25 of their resources, again repeated five times.

F) Resource levels are rounded to whole numbers.

G) These initial resource values are saved for use in decision processes around abandoning cacao farms.

H) A number of highlanders are created, the number set by a slider, colored red, and randomly placed on the grid of patches.

I) If the “imitate?” switch is on, they set their “norm-min-resource” variable to 0.5; otherwise to the minimum resource value.

J) Their wealth starts at zero.

K) Their “vision” state variable, is set to 1 plus a random number up to whatever is set with the “max-vision” slider (default = 10).

L) Their “cacao-profit” state variable is set to zero and their “cacao-grower?” and “t-grower?” variables set to false.

M) Their “market-distance” variable is set as the distance between themselves and the market.

N) Their “c-punished” variable is set to zero.

O) A “market” agent is placed randomly on one of the patches.

P) “ticks” is reset.

6. Input Data

There is no specific quantitative input data. Qualitative input data attempted to roughly approximate what was found in Tania Murray Li’s (2014) book, *Land’s End: Capitalist Relations on an Indigenous Frontier.* However, this must be seen as a mostly abstract, heuristic model that does not come close to representing Li’s account in any realistic detail.

7. *Submodels Details*

A) Submodel A

Highlanders harvest all resources much faster than they can regrow.

B) Submodel B

a) Non-cacao-growers have their wealth discounted according to an amount set by slider (default = 0.95)
b) If their amount of wealth is higher than “costpunish” (set by slider, default = 0.01), they will punish “cheaters”—those who have overharvested (their “harvestedlevel” > “norm-min-resource”) that they find within a “radius” set by slider (default = 10). Cheaters then lose “costpunished” (set in slider, default = 0.06) and punishers lose “costpunish.”
c) If highlanders run into another that has greater wealth, they copy the norm-min-resource of those doing better, with addition of a random normal number with a mean of zero and a standard deviation of “stdeverror” (set in slider, default = 0.01).
d) If highlanders have zero wealth, they randomly pick another and copy their norm-min-resource.
e) Movement: highlanders check every direction and choose the neighboring patch with the highest resource available, and move there.

C) Submodel C

a) If non-cacao-growing highlanders within a radius set by the “tobacco-initiation-radius” (default = 5) slider, and a random number from 1-100 is less than “probability-tob” (set by a slider, default = 5), they will become a tobacco-grower. This means they will deduct

"How-much-t-growers-overharvest" (set in slider, default = 0.01) from their norm-min-resource. This is shown graphically by a color change to fuchsia.

D) Submodel D
   a) New cacao-growers are selected at random, but weighted towards those closer to the market. The number in of them is set by the "new-c-growers" slider (default = 5). "cacao-grower?" is set to true.
   b) Becoming a cacao-grower means private property of patches is initiated. They are moved to a random available (not already a cacao farm) patch within the radius set by "radius-new-cfarm" (default = 5) and given extra neighboring available patches numbering "extra-cacao-patches" (set by slider, default = 1). All patches owned have their "cacao-crop?" set to true. And "owner" set to be the "self" of the grower.
   c) If "accumulators" slider is above zero, the number on it will select current cacao growers to accumulate new farms, randomly selected but weighted by wealth so that the rich get richer. Once an "accumulator" is present, new patches are acquired and settings changed as in #b above.
   d) Profits made from cacao farm patches are calculated as follows: "crops" * price - farm costs. "crops" are the product of the sum of each max-resource-here value of owned patches * ("c-crop-coefficient" + rnd normal, sd = half max)). "c-crop-coefficient" is a global variable set by slider, default = 1. "cacao-price" sets the price by slider, default = 100.
   e) Cacao growers will abandon cacao growing if their cacao-profit variable is less than 1, or if costpunished-c > wealth * percent-c-wealth-lost-to-punishment [set in slider, default = 1] + random-normal with a mean of costpunished and standard deviation = costpunished / 4

E) Submodel E

a) If a random number is less than "strength-of-c-punishment" (set by slider, default = 100), non-cacao-growing highlanders go through a process of deciding whether to punish cacao growers. A "cacao-cheat-threshold" set by slider (default = 0) is multiplied times the amount of wealth the highlander has, creating a "too-rich" bar. If any "too-rich" cacao growers are found within "radius" (set by slider, default = 10), they are punished and lose "percent-c-wealth-lost-to-punishment" * their wealth. The punisher loses costpunish * the number of those punished.

Lastly, with "punish-c-growers?" turned on, agents can punish cacao-growers, as discussed above.

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