Commons, Enclosures, Complexity: Prolegomena to a “Critical ABM”

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***Introduction***

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; David O’Sullivan, 2008; David 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 sub-fields 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; David O’Sullivan, 2004, 2018). Given the promise of ABM to aid our understanding of many different kinds of non-linear environmental and social systems where experimental science is difficult or impossible (O’Sullivan & Perry, 2013, page 16), this absence invites explanation. The present thesis will offer some explanation, as well as a modest contribution towards filling the void: an heuristic ABM based on the emergence of capitalist relations among the Lauje highlanders of Sulawesi, Indonesia, at the turn of the twenty-first century, as recounted by Tania Murray Li (Li, 2014).

The thesis is organized into five sections. We begin with a general discussion of ABM, “Agent-Based Modeling: Background and Methodological Issues.” This includes background on complexity science, which, 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 (David O’Sullivan et al., 2012). These kinds of processes had resisted conventional scientific understanding until recent decades. 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).

The second section, “Placing ABM in Critical Quantitative Methods,” is an historical discussion of the dualism between critical social theory 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; David O’Sullivan, 2018; David O’Sullivan et al., 2018).

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.

A fourth section, “Commons and Enclosures,” is a discussion of the application of ABM to the emergence of capitalist relations in the case of Li's work on the Lauje. This is a theoretical reflection on the potentials and difficulties of this kind of application. It outlines the current limitations and what might be done moving forward. The ABM itself is available online [footnote: LINK], and detailed explanation and documentation of it is in an appendix. The present model is a modest, abstract and heuristic contribution towards the use of ABM in critical geography.

The fifth section, “Complexity Evolving: Norms, Games, Space,” contains an extended discussion of some 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 the former, and punctuated equilibria in the latter. Investigating the potential for geographers to engage with these developments is therefore an extension of Stallins' (2012) call for geographers to engage with "new organism-environment interaction" in cutting edge biology. These new developments rooted in complexity science have pertinent implications for the specific topic of this thesis, as well as for critical geographers and political ecologists in general.

Finally, there is a concluding section.

***Agent-Based Modeling: Background and Methodological Issues***

Uri Wilensky, creator of the computer language, NetLogo, which is used for ABM, defines 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. 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 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 predicting extinction rates with the ABM method. This is because it avoids the “nano wolf problem,” where fractional populations can make a come-back, 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,[[1]](#footnote-1) to which ABM is closely tied[[2]](#footnote-2). 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 theproperties of the elements, but rather, also arise from interactions of the elements. Suchemergent structures are system properties yet they often feedback to the very individualelements 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 to grasp, including scientists (Wilensky & Resnick, 1999). This difficulty persists despite the ubiquity of emergent structures, as has been demonstrated, in part using ABM, in such phenomena as murmuration flying patterns of birds and optimization of food gathering by ants, as well as many other examples from physical and social sciences, as well as thought experiments like “cellular automata” (David O’Sullivan & Perry, 2013, page 18).

Unlike an ABM of predator-prey relations, or gas molecules colliding, for many phenomena the rules of behavior are not well known. We may have some idea of the target global behavior for which we are trying to find rules, though. 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, where 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 phenomena to see if it yields insights into that phenomena. 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 (David O’Sullivan, 2004, page 290). It is worth admitting frankly, as Sterman argues, that “all models are wrong,” though some are useful (discussed in David O’Sullivan, 2004, page 290; Sterman, 2002). 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” (David O’Sullivan, 2004, page 291).

The deductive-nomological model of explanation that dominated 20th century science, where universal laws are 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; David O’Sullivan, 2004).

So, we admit that all models are wrong, that validation of ABM is not possible. Even more, following O’Sullivan (David O’Sullivan, 2004, page 288), we can say 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 we 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 capitalism 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, 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 be much more skeptical and critical of computer-simulated representations—even though it can be demonstrated the latter are more rigorous, in the Lotka-Volterra case[[3]](#footnote-3).

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 (David 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 it is at least widely recognized that what ABM is best suited for is modeling complex systems with interacting, heterogenous actors, which are 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, critical social theory more generally, or 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”—encompassing feminist, Marxian, post-structuralist, post-colonial, green, disabled, queer and other approaches aimed at emancipation and changing the object of their 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). It would be interesting to empirically investigate to what extent these claims are true, and if so, whether any of this has changed over the last fifty years or so[[4]](#footnote-4).

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, which bolstered the status quo (Handelman, 2019).

When Geographic Information Systems was ascendent in the 1990s, there was much attack from human geography, eventually leading to more nuanced debate, and the emergence of the whole subfield of “critical GIS,” with a large body of literature and practices (Kwan & Schwanen, 2009; David O’Sullivan, 2006; Sheppard, 1995; J. Thatcher et al., 2016; J. E. Thatcher et al., 2018), which has gone some distance towards bridging the “critical / quantitative” divide in geography.

Indeed, critical GIS—overlapping and connected with “critical quantitative geographies,” (Kwan & Schwanen, 2009; David 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” at the AAG conference 2021 (Crampton, 2020). The new sub-discipline of critical physical geography is also very much aligned philosophically with this bridging of the critical / quantitative divide (Lave et al., 2018).

It is here that I envision a “critical ABM” fitting in with currents of other scholarship. Critical ABM seems like such an obvious and natural extension to critical GIS[[5]](#footnote-5), which has been fairly widely discussed, and ABM is an exploding, closely allied field to GIS, literally connected and synergizing together (David O’Sullivan & Perry, 2013; Wilensky & Rand, 2015). David O’Sullivan, a prolific proponent of critical GIS and critical-quantification[[6]](#footnote-6) (O’Sullivan & Manson, 2015; David 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; David O’Sullivan et al., 2012; 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 heard of such a thing (David O’Sullivan, 2021).

Part of the impetus of critical GIS has been to critique the political economy of GIS itself—and to interrogate 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[[7]](#footnote-7). There is certainly parallel work to be done with ABM. 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 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; D. O’Sullivan & Haklay, 2000; O’Sullivan & Manson, 2015; David O’Sullivan, 2002, 2004, 2008; David O’Sullivan et al., 2012; David 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, critical social theory more generally, or 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 so much 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 the family of “Big Data” computational approaches, taught alongside 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 (David O’Sullivan, 2018, page 28)[[8]](#footnote-8). 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 (David 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 (David O’Sullivan, 2004, 2018). O’Sullivan 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.” (David O’Sullivan, 2018).

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 (Torrens, 2010) wrote the last major survey of ABM in geography. But this paper is quite skewed towards the “spatial sciences” and did not consider much human geography. Torrens did not even cite O’Sullivan’s 2008 paper, “Geographical Information Science: Agent-Based Models.” Torrens only has one mention of Wilensky, who wrote the popular computer language used by hundreds of thousands of people to build ABMs, NetLogo (Wilensky & Rand, 2015). It is difficult to say much with confidence at this time, but I have a strong hunch 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 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 it 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 (Torrens, 2010), David O’Sullivan and George Perry wrote the only available textbook on ABM specific to geography (David O’Sullivan & Perry, 2013), 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; David O’Sullivan, 2004, 2008; Sullivan, 2009). Note only O’Sullivan papers that might have been commented upon in Torrens 2010 review were included in that 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; David O’Sullivan et al., 2012; David O’Sullivan, 2018). Also, maybe I’m making too much of this omission by Torrens? Two papers O’Sullivan authored or co-authored were discussed in Torren’s review (M. Haklay et al., 2001; David O’Sullivan, 2002).

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; 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. This author both appreciates the sentiment and sees how it could put off those who take themselves seriously— or 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 worlds 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. But there are a lot of people who apparently believe in these visions. And it’s not for nothing because ABM does hold out some interesting promises.

***Commons and Enclosures***

Now that we have introduced the methodological issues around ABM, the problem of critical / quantitative dualism, new efforts to bridge that divide with critical GIS and critical quantitative geographies, and contextualized the split strands of ABM research in geography, we 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. As an abstract and heuristic ABM that makes little claim to represent the real world in a way that can be easily validated, it is difficult to evaluate what has been achieved. It remains mostly on the level of a thought experiment, but also a suggestion of what else might be accomplished with greater skills and more time.

*Background on the Lauje Highlanders*

The Lauje highlanders remained somewhat insulated in a remote area of Sulawesi, and retained non-capitalist ways of living until the 1990s. Their system of shared access to common land, which stretched back countless generations (Li, 2014, 15), enabled a remarkable degree of autonomy:

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 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, 2014, Pg. 6).

Li both builds upon—but also challenges important aspects of—earlier explanations of the mechanisms involved in this 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 does not deny this, but pushes back to an extent, emphasizing 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 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).

*Building an ABM of Capitalist Relations Emerging*

The reader may wish to download and familiarize themselves with the model and its attending, detailed documentation in the appendix before reading the discussion that follows.

Using Elinor Ostrom’s game theoretic terms[[9]](#footnote-9), 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. 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).

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 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. What if the Lauje had had greater awareness of the threat the cacao boom posed to their way of life, for example? Might they have been able to resist the destruction of their self-governed commons, and for how many decades or centuries more? Is there any way that what has been lost could even be restored?

The model here 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), which is in turn based upon the “Wealth Distribution” model by Uri Wilensky (1998), which in turn is built on Sugarscape (Epstein & Axtell, 1996). 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.

Although this model is loosely based on Li’s work on the Lauje (Li, 2014), there is no attempt at any great deal of realism or detail in representing that case. There is not enough reference data to do justice to that. Also, there is always a trade-off between realism, detail, and generality, when it comes to modeling (O’Sullivan & Perry, 2013, page 23). Instead, Li’s work and the case of the Lauje focalizes the dynamics of the model. But it must be seen as a rough approximation that can hopefully 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) [FIND REF TO CITE IN BLAIKIE].

As described in more detail in the appendix, the agents in the present model represent groups of highlanders. There is a grid of patches which is initialized upon starting each model run, which have different amounts of resources which regrow at a logistic rate. The agents move about the patches harvesting resources every “tick,” which represents a few years, the typical amount of time highlanders would stay in a clearing before letting it fallow (Li, 2014, 88).

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[[10]](#footnote-10).

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 fairly 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).

The behavior of the model 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[[11]](#footnote-11). 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).

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[[12]](#footnote-12). With “accumulators” set to zero, we see equitable development of cacao farming. With that slider set higher, some accumulate many patches for cacao farms and other agents are left without any. 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).

Lastly, with “punish-c-growers?” turned on, agents can punish cacao-growers[[13]](#footnote-13). 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, we can turn up the dial on that punishment and adjust it until cacao-growing ceases, if we please. This can represent some of the “what if?” questions, alluded to earlier. What if the highlanders 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, and for how many decades or centuries more? With the model we can adjust this variously to represent alternate, hypothetical historical scenarios.

*Evaluating the Model*

Certain shortcomings of the model must be frankly admitted. Li’s highly nuanced account is caricatured by the model’s treatment. 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[[14]](#footnote-14).

Representation of space in the model is very inadequate. It is simply a grid that is toroidally wrapped, with a diffusion of resources[[15]](#footnote-15) that regrow at a logistic rate[[16]](#footnote-16). Turning it into an island shape—such as in the model of moa extinction rates due to early Maori colonization of New Zealand by O’Sullivan and Perry (2013, 230)—could be considered for future iterations of the present model. But if realism were seriously to be attempted, the rugged topography of central Sulawesi would need to be incorporated, along with considerations of what areas have different levels of precipitation[[17]](#footnote-17).

However, if shooting for realistic detail, one might consider adding all kinds of factors from Li’s account, like levels of taxation (Li, 2014, 47), levels of male gambling behavior (Li, 2014, 52), levels of intra-kin feuds (Li, 2014, 10) that were upsetting to people and made them rely on officials at the coast in some instances, to resolve disputes (Li, 2014, 37).

There was also a secondary type of highlanders’ commons, that of primary forest, believed to be protected by feared spirits, and loosely presided over by ritual specialists (Li, 2014, 87). The present model does not touch that fact, and many others in Li’s nuanced account.

But the question remains, would adding more details such as these help the model to illuminate anything? Or would it just clutter, leading to the kind of scenario written about by McElreath and Boyd (2007, page 8): “we had a world we didn’t understand and now we have added a model we don’t understand.”

More generally, to address some common questions:

1. Is this model predictive in any sense?

The answer is no.

1. Do model forecasts correlate with subsequently observed reality?

In this case, a “forecast” is really a “hindcaste,” and “subsequently observed” would mean looking back at information about what happened in the past, with an ecological and social system that no longer exists. It can be argued, as above, that certain scenarios roughly approximate what happened, according to Li’s account: accumulation, where a small number of highlander groups became relatively wealthy, and most became destitute, with the loss of traditional usufruct property rights and thus free access to land (Li, 2014, 139).

1. Does the ABM offer coherent results that do not actually touch reality?

If by “reality” we mean what we know to have happened, then yes it is possible to argue that the model *can* give results that contradict that, and are thus “unreal”. Parameters for punishment of cacao-growers, as discussed above, can be adjusted in such a way that cacao-growing never booms, or is beaten back. But doesn’t this beg the question of what “reality” is, when speaking of complex historical processes? It can be argued that these alternative, hypothetical historical scenarios are just as “real”. If, for example, in 1990, when the cacao boom was just starting, highlanders became determined to commit arson to any cacao plantation, would this contingency not have altered the course of history?

Note that more realist ABMs are certainly possible[[18]](#footnote-18). The current high bar—in terms of realism with ABMs that are at least somewhat akin to what has been attempted with the present model—has been set by 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). Over at least a thousand year period, a system of elaborate rituals were developed by rice farmers in Bali, in which local, direct-democratic assemblies, organized as water temples, coordinate planting and fallow periods in such a way to both manage pests and equitably distribute 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. By the 1980s, many rice farmers wanted to return to their traditional 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 Kramer built an ABM 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 back off.

Lansing and colleagues (Lansing, 2006; Lansing & Cox, 2019) have argued that there are many examples of sustainable, self-organizing, complex socio-ecological systems like the water temples system—that are invisible to moderns and are being lost at an alarming rate. The present thesis and ABM attempts to argue that the traditional system of the Lauje highlanders is one such example. Unfortunately, without much more reference data, it would be difficult to make this case with more realism.

The situation of the Lauje highlanders is not the kind of thing that we’re ever going to be able to test experimentally. Data on it is ethnographic and historical. As for other accounts of the emergence of capitalist relations, the data is almost exclusively historical and mostly from the relatively distant past. So validation of the model, or any model of this kind at different scales or for different cases, in terms of it representing reality, is bound to be difficult and limited. There’s no repeatable or easily testable experiment that’s going to be possible. But like studying extinction rates of the moa, in relation to different hypothetical, early Māori hunting behaviors (O’Sullivan & Perry, 2013, 230), even though the ecological and cultural system studied here no longer exists, hindcasting (Torrens, 2010, 428) is still possible to the extent that corroborating data can be found.

It might be interesting, for example, 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[[19]](#footnote-19). In the case of the Lauje highlanders, it appears that they must have lost much 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.

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[[20]](#footnote-20) is “at the core” of ABM.

[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 (Yeh, 2016, 37).

Extending this critique to Ostrom’s SES:

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 socio-ecological systems (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 (Yeh, 2016, 37).

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 (D. O’Sullivan & Haklay, 2000, 1416-17) and in various intellectual trends in social science (D. 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 (D. 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 a majority of 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[[21]](#footnote-21). 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[[22]](#footnote-22). This connects to a range of debates that warrant geographers’ attention, to which we will turn shortly.

***Complexity Evolving: Norms, Games, Space***

Biology has recently seen a debate raging about the nature of social cooperation (Abbot et al., 2011; M. A. Nowak et al., 2010)—a “clash of the titans,” (Gintis, 2012)[[23]](#footnote-23) 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 effect each other, making for more than single loci additive dynamics assumed by the influential Hamilton’s Rule (Gintis, 2013a; Hamilton, 1963). A revised theory of group selection—once considered discredited—has been formulated, which recognizes the legitimacy of Hamilton’s Rule is limited, and when it comes to social species needs to be understood in a broader context of multilevel selection (Traulsen & Nowak, 2006). This new sociobiology 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). In this final section we will explore the contours of these and related, new complexity science-infused developments. But first, 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[[24]](#footnote-24) (Livingstone, 1993, 216-218, 244, 249). And with only one exception, they did so to rationalize racism and serve empire. Kropotkin is the only one that was distinctly *against* empire, colonialism, and the state (Livingstone, 1993, page 254). What Livingstone fails to point out is that Kropotkin is also the only one of these geographers who is taken seriously by biologists, and 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 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 (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” (Harvey & Reed, 1994, 371). Talcott Parsons opened the work that launched his monumental career by declaring evolutionary thought in sociology to be dead (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 (Harvey & Reed, 1994, 372; Parsons, 1966). Harvey and Reed interpret Parsons’ 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” (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, on a geological time scale, emerging from marginal populations that become geographically isolated due to catastrophes, such as mass extinction events (S. J. Gould & Eldridge, 1977; Harvey & Reed, 1994, 392). Much as Stallins (2012) has called for geographers to engage with the new ideas about organism-environment interaction in biology, as an opportunity to re-think (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.

Much of Stallins’ (2012, 332) focus is 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). 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, 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)[[25]](#footnote-25).

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

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 we will now turn.

*A Proposed Unification of the Social Sciences*

There are five conceptual parts of Gintis’ (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. We 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 examples is the anatomical features that permit human speech. This includes the larynx 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 most consonants production; the hyoid bone to which the tongue muscle attaches; 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 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.

A second concrete example is the anatomy of complex facial expressions, something about which Darwin (2009 [1890]) wrote a book, but which was ignored by biologists until recently (Burrows, 2008; Gintis, 2014, 194, 201). 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 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).

A key proposition of Gintis’ framework 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 the framework. Norms and their psychological correlates or prerequisites are seen by Gintis (2014, 195) as emergent properties of all human societies. They cannot be deduced from its parts. This implies the refutation of a large bodies of contemporary theory in economics (Gintis, 2005, 2014, 200) and biology (Boyd & Richerson, 1988; Gintis et al., 2009; Richerson & Boyd, 2004) which have insisted on trying to show that self-regarding rational individuals can effect cooperation.

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* in which the behavior of agents is derived by assuming that each is choosing a *best response* 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)[[26]](#footnote-26). 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, 2014). But he is also a formidable critic. He attacks the reigning culture in game theory 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 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 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)[[27]](#footnote-27).

The rational actor model—which Gintis prefers to call the “beliefs, preferences, and constraints” model (BPC)—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 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 (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])”. (Darwin, 2009; Gintis, 2014, 207; Grafen, 1999, 2000, 2002)

“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 (Real 1991; Alcock 1993; Real and 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" (Alcock, 1993; Gintis, 2014, 208; Real, 1991; Real & Caraco, 1986).

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 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’ 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* 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).

Similar barbs are aimed at economists. For 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,” which Gintis (2014, 217) believes to be impossible (Gintis, 2007b, 2013b).

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 (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; Smith & Szathmáry, 1997).

To sum up, in Gintis’ 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 it concerns human and animal behavior. [[28]](#footnote-28) 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 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,[[29]](#footnote-29)* by Bowles and Gintis(2011) considerably flesh 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. Far from 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’ account resonates strongly with Kropotkin’s (1885), enabling a vision of freedom and justice for all (Kropotkin, 2021). From their view, the evidence 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 (Gintis et al., 2019).[[30]](#footnote-30)

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

First of all, Gintis’ treatment of sociobiology and gene-culture coevolution generally strongly resonates with and extends the Stallins’ (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, 200) elucidates the fact that the latter is a special case of the former. Note that there are some thorny issues here when it comes to engaging with sociobiology. At least, that discipline has stirred up much controversy, critique and charges of racism over E.O. Wilson’s early work, for example (Allen et al., 1975; Gould, 1996). Arguably, sociobiology has considerably transformed with Wilson and others’ more recent advocacy for multilevel selection (M. A. Nowak et al., 2010), and the development of gene-culture coevolution, aligned with departures from genetic determinism, discussed above. But there may well be some points of contention here. [[31]](#footnote-31)

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 between all of our subdisciplines is anathema. In Gintis’ defense, we 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’ 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 a coercive necessity of survival rather than an opportunity, the experience of which 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, we 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 our more benign capacities, such as the traditional Lauje highland form of life. So there is no easy or immediate answer as to what Gintis’ paradigm or various works within it tell us about this question.

Neither is there an easy or immediate answer as to what Gintis’ paradigm tells 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). [[32]](#footnote-32) This will lead us into discussions of very different forms of complexity theory, below.

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There are Marxian versions of complexity theory (Byrne, 1998; Byrne & Callaghan, 2014; D. L. Harvey, 2001; D. L. Harvey & Reed, 1997, 1994; Williams, 2020). The 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) and Gould and Eldridge’s theory of punctuated equilibrium (Gould & Eldridge, 1977). 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 we 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 for work on dissipative structures, complex systems, and irreversibility—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 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—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 non-linear cascades of reactions, similar to metabolic reactions in living beings” (Williams, 2020, 26).[[33]](#footnote-33)

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. 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.[[34]](#footnote-34) 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).

Another important component of Harvey and Reed’s (1994, 393) proposed paradigm is the canonical triadic structure of evolution, proposed by the co-founder of punctuated equilibria, Niles Eldredge (1986, 1989). In his original triad, organisms mediate between two evolutionary hierarchies: (1) a genealogical or “information” hierarchy and (2) an ecological or “economic” hierarchy. The two converge in the organism, which is both a propagator of genetic information and in interaction with ecosystems (see figure 1).

“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).

Diagram

Description automatically generated

**Figure 1 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 2 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 rest that 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).

Diagram

Description automatically generated

**Figure 2 Harvey and Reed's additions to Eldredge's** **Hierarchy of Evolutionary Processes**

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:

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 (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; Julian Hanynes Steward, 1973 [1955]; Julian Haynes Steward, 1977).

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, with 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.[[35]](#footnote-35) “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 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).

This presentation of Harvey and Reed’s main theses has been quick and surface-scratching due to space constraints. But we hope the gist of it is clear. We 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; Prigogine, 1997; Prigogine & Stengers, 1984)

Elsewhere, Prigogine—or chaos theory (Douglas Kiel & Elliott, 1997), with which he is closely associated—is considered a “spiritual ancestor” (David 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, 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)[[36]](#footnote-36).

However, the style 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, on a geological time scale, emerging from marginal populations that become geographically isolated (S. J. Gould & Eldridge, 1977; Harvey & Reed, 1994, 392). 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, 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 sociobiology, and Gintis is veritably on the attack on this point.[[37]](#footnote-37) But what about the need to situate human history vis-à-vis natural history, on a grand paleontological scale? And must biology 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’ paradigm in a way that is quite consonant.[[38]](#footnote-38) And what of Harvey and Reeds 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 we have missed something crucial that makes all this incompatible with Gintis. But from what we 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.[[39]](#footnote-39)

And what of Harvey and Reed’s extension to Eldredge’s triadic hierarchy of evolution? Here we 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 we also suspect that the very non-orthodox use of Marx in Harvey and Reed may be unfamiliar to Gintis. What Gintis really seems to object to is what he sees as the mechanistic approach of Marx [FIND CITATION WHERE GINTIS SAYS THIS], 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? It seems that this theme has not been much taken up by the small group of “complexity Marxist” school scholars (Williams, 2020, 60),[[40]](#footnote-40) 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” (Moore, 2015; Patel & Moore, 2017), or other schools of Marxian theory, to think about the emergence of capitalism on various scales. As for the case of the Lauje highlanders, no one ever claimed that it was generalizable—it is more of an anomalous case that appears to hold important lessons, in terms of the possibility for capitalist relations to emerge even *without* much coercion, though this is probably rarely how things work. The case of the Lauje highlanders is also anomalous in that racism was not involved (Li, 2014, 10), at least in the local context of the highlanders and the more modern, coastal people in that area of Sulawesi, who also see themselves as ethnically Lauje. This would be contrary to at least some interpretations of Cedric Robinson’s theory of Racial Capitalism, a key conclusion of which is that racism is fundamental to capitalism (Robinson & Kelley, 2000, 2).

Returning to the comparison of Gintis’ paradigm with that of Harvey and Reed, what would the latter make of Gintis’ transdisciplinary behavioral sciences paradigm? The debates in sociobiology that Gintis intervened in, and gathers steam from, did not even start raging until ten or fifteen years after Harvey and Reed were formulating their theses. All the new organism-environment interaction biology important to Stallins (2012) did not start gaining ground until about the same time, as well. Epigenetics and gene-culture coevolution may only now be seeing something closer to widespread acceptance. Because Eldredge was an early champion of many of these developments, within the framework of the “Extended Evolutionary Synthesis” (Brooks, 2011; Eldredge, 1985, 1995), we suspect Harvey and Reed would very much welcome Gintis’ treatment of gene-culture coevolution. Then again, Stephen J. Gould—whose ideas are central to Harvey and Reed—was famously at odds with E.O. Wilson—whose ideas are arguably central to Gintis (Allen et al., 1975; Gould, 1996). But as discussed above, sociobiology has considerably transformed since Wilson and others’ more recent defense of group selection / multilevel selection (M. A. Nowak et al., 2010). Now that Wilson and Gould are both gone, perhaps their ideas can be reconciled?

Whether or not Harvey and Reed would accept Gintis’ 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 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).

Our 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 in 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 'Mans 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’ view on the place of game theory and the rational actor model.

***Conclusions***

To sum up, we started with the premise that ABM is an important new methodology that has seen very little use in critical geography. We explained that 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. GIS was engineered to serve capital accumulation and the state. But we have seen it can be “hacked” for uses in critical social science, as well as for political interventions towards 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. A modest effort towards a hands-on contribution towards this project was made with the present ABM. While we do not claim it has reached a high bar of realism to which we might aspire—such as in the work of Lansing and Kremer (1993)—it did produce an abstract, heuristic model that can perhaps be of some use. It can help stir interest in its topic, the emergence of capitalist relations among the Lauje highlanders, and provoke thought about historical contingency, accumulation of wealth and inequality. As a first foray into using ABM in political ecology, its creation entailed an examination of the epistemology and politics of modeling presented in this thesis. The need to engage with ideas in complexity science followed from this examination.

Geographers’ engagement with developments in complexity science, especially in relation to new organism-environment interaction biology, promises to be productive. Gintis’ transdisciplinary behavioral sciences paradigm invites considerable extension of Stallin’s call for geographers to engage with this new biology, particularly in relation to gene-culture coevolution. Harvey and Reed’s dissipative social systems evolution paradigm holds out similar promise to geographers, in relation to the grand paleontological scale of natural history as understood with punctuated equilibria theory, and also Eldredge’s contributions to the “Extended evolutionary synthesis,” such as the triadic hierarchy model of evolution.

Gintis’ and Harvey and Reed’s paradigms remain segregated from each other. It is entirely possible that we could have missed something crucial that would make them incompatible—given the wide-ranging and complicated elements involved—but there seems considerable promise for powerful integrations.

**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. We 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 [LINK].

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.

1. *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:

1. their amount of “wealth”
2. their “norm-min-resource”—the minimum amount of resources on patch before they will harvest.
3. “cacao-grower?”—whether or not they grow cacao, entailing a change in land use and property relations.
4. “owned-patches”—a list of patches owned by cacao growers.
5. “market-distance”—their distance to a market.
6. “costpunished-c”—the losses suffered if punished for growing cacao.
7. “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:

1. “copied-norm”
2. “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:

1. “resource-here”: the current amount of resource on this patch.
2. “max-resource-here”: the maximum amount of resource this patch can hold.
3. “cacao-crop?”: if true, the patch is privately owned by cacao-growing group of highlanders, known as a “c-grower”, & others can't go there.
4. “owner”: c-growers that own cacao-crop patches. This state variable is set to "Nobody" if cacao is not grown there.
5. “initial-resource”: the amount of resource-here upon initialization, used for some calculations in the model machinery, discussed below.
6. *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.

Start

Initialize landscape / resources

Initialize highlander groups

Patches grow resources

New tobacco-growers recruited

New cacao-growers recruited

Non-cacao-growers move to neighboring patches with best available resources

Cacao-growers harvest, calculate profits

Wealth discounted

Overharvesting norm violators punished

Imitate norms of those doing better

Cacao-growers decide whether or not to abandon cacao-growing

Update plots of Lorenz Curver, Gini-index, average wealth and average resources on patches

*Figure 1: A flowchart showing 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 that 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[[41]](#footnote-41).

*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 fairly 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[[42]](#footnote-42). 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).

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[[43]](#footnote-43). With “accumulators” set to zero, we see equitable development of cacao farming. With that slider set higher, some accumulate many patches for cacao farms and other agents are left without any. 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*

Lastly, with “punish-c-growers?” turned on, agents can punish cacao-growers[[44]](#footnote-44). 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, we can turn up the dial on that punishment and adjust it until cacao-growing ceases, if we please. This can represent some of the “what if?” questions, alluded to earlier. What if the highlanders 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, and for how many decades or centuries more? With the model we can adjust this variously to represent alternate, hypothetical historical scenarios.

1. *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 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. What if the Lauje had had greater awareness of the threat the cacao boom posed to their way of life, for example? Might they have been able to resist the destruction of their self-governed commons, and for how many decades or centuries more? 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 form where the most wealthy accumulate more farms. The chart below [Lee note, 8-4-22: ALL THESE CHARTS SHOULD PROBABLY BE REDONE WITH 100 RUNS AVERAGED WHERE THERE ARE NOW JUST 1 “TYPICAL” RUN GIVEN…AND SHOULD BE MADE MORE PRETTY] gives an idea of the comparative behavior of these two forms, in terms of how long it takes for resource collapse.

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.

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:

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). One could 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 for, or eliminate, its effects.

1. *Initialization*
2. All state variables are cleared.
3. Maximum resource of patches is set to 50
4. An amount of the patches, set by the “percent-best-land” (default 10%), is randomly selected as best land and given maximum resource values.
5. These patches diffuse 0.25 of their resources, which is repeated five times.
6. Then all patches further diffuse 0.25 of their resources, again repeated five times.
7. Resource levels are rounded to whole numbers.
8. These initial resource values are saved for use in decision processes around abandoning cacao farms.
9. A number of highlanders are created, the number set by a slider, colored red, and randomly placed on the grid of patches.
10. If the “imitate?” switch is on, they set their “norm-min-resource” variable to 0.5; otherwise to the minimum resource value.
11. Their wealth starts at zero
12. Their “vision” state variable, is set to 1 plus a random number up to whatever is set with the “max-vision” slider (default = 10).
13. Their “cacao-profit” state variable is set to zero and their “cacao-grower?” and “t-grower?” variables set to false.
14. Their “market-distance” variable is set as the distance between themselves and the market.
15. Their “c-punished” variable is set to zero.
16. A “market” agent is placed randomly on one of the patches.
17. “ticks” is reset.
18. Input Data

There is no specific quantitative input data. Qualitative input data attempted to roughly approximate the 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.

1. *Submodels Details*
2. Submodel A

Highlanders harvest all resources much fast than they can regrow.

1. Submodel B
2. Non-cacao-growers have their wealth discounted according to an amount set by slider (default = 0.95)
3. 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”.
4. 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).
5. If highlanders have zero wealth, they randomly pick another and copy their norm-min-resource.
6. Movement: highlanders check every direction and choose the neighboring patch with the highest resource available, and move there.
7. Submodel C
8. Non-cacao-growing highlanders within a radius set by slider “tobacco-initiation-radius” (default = 5), 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.
9. Submodel D
10. New cacao-growers are selected by 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.
11. 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.
12. 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”, new patches are acquired and settings changed as in #b above.
13. 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.
14. 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
15. Submodel E
16. 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[[45]](#footnote-45). 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, we can turn up the dial on that punishment and adjust it until cacao-growing ceases, if we please. This can represent some of the “what if?” questions, alluded to earlier. What if the highlanders 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, and for how many decades or centuries more? With the model we can adjust this variously to represent alternate, hypothetical historical scenarios.

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1. Complex systems theory has gone by many names. I will refer to it mostly 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 theory, 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). Thrift 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 (Thrift, 1999, 48-50), 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] 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). [↑](#footnote-ref-1)
2. “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). [↑](#footnote-ref-2)
3. 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 solved, except numerically…For these reasons, we would prefer formal and simulation models be learned side by side.” [↑](#footnote-ref-3)
4. This author has laid out plans for such an historical sociology of quantitative methods in the discipline of geography (Fram, 2021). [↑](#footnote-ref-4)
5. For overviews, see especially (David O’Sullivan, 2006; J. Thatcher et al., 2016; J. E. Thatcher et al., 2018). [↑](#footnote-ref-5)
6. It is notable that O’Sullivan co-authored one of the main textbooks (Sullivan & Unwin, 2010) used in the graduate course in quantitative methods in geography, as well as the sole textbook on ABM in geography (David O’Sullivan & Perry, 2013). [↑](#footnote-ref-6)
7. 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; Wilson, 2012). For critical social history of culture and political-economy around computers more generally, see (Turner, 2006; Worden, 2012). [↑](#footnote-ref-7)
8. 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). [↑](#footnote-ref-8)
9. 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, it will be argued shortly that game theory warrants our attention. For now, 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)? [↑](#footnote-ref-9)
10. Highlanders in fact never used cacao and did not understand its utility, besides being a commodity for sale (Li, 2014, 123). [↑](#footnote-ref-10)
11. 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. [↑](#footnote-ref-11)
12. 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. [↑](#footnote-ref-12)
13. 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. [↑](#footnote-ref-13)
14. See footnote 24 for references to the wildly active area of research on game-theoretic treatment of social norms. [↑](#footnote-ref-14)
15. The initial amount of resources can be adjusted by the “percent-best-land” slider. [↑](#footnote-ref-15)
16. It can be unwrapped, which does change some of the behavior, leading to resource collapse faster in general. [↑](#footnote-ref-16)
17. It would not be too difficult to import GIS data from Central Sulawesi into the ABM. It could be interesting, but this level of detail and realism can also be inappropriate, entailing an unnecessary trade-off with generality for the purposes of the model (David O’Sullivan & Perry, 2013, 24). [↑](#footnote-ref-17)
18. And there are a whole battery of error analysis, sensitivity analysis, uncertainty analysis, and structural uncertainty analysis tests that could and should be done on an ABM that aspires to realistic representation. See chapter seven in O’Sullivan and Perry (2013).This might be worthwhile as an exercise with the present ABM but it seems silly to have such a technical tour de force for a model that is more of an abstract, heuristic, thought experiment than a realistic representation. [↑](#footnote-ref-18)
19. 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). [↑](#footnote-ref-19)
20. Methodological individualism is a doctrine, 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). [↑](#footnote-ref-20)
21. Political ecology even comes up as an allied field in the Wikipedia page for SES (Crowd-sourced, 2022). [↑](#footnote-ref-21)
22. See footnote 24, below. [↑](#footnote-ref-22)
23. Gintis plays a role of diplomatic conflict mediation in this debate. He purports to seek a compromise with his own results (Gintis, 2013a). However, it has been argued (Yee, 2014) that he actually makes a more effective argument that “inclusive fitness” is not all-explanatory, compared to the original, incendiary arguments (M. A. Nowak et al., 2010). Also note that Gintis has stated openly that he is closely allied with E.O. Wilson and Martin Nowak (Carrol & Gintis, 2021, at 1:01:20 in video). [↑](#footnote-ref-23)
24. The only exceptions are Élisée Reclus and George Perkins Marsh (Livingstone, 1993). [↑](#footnote-ref-24)
25. When this author was completing his undergraduate degree in Integrative Biology at UC Berkeley a few years ago, it was evident that biologists are still reeling from the disorientation of having the rug of Crick’s central dogma (Crick, 1958) pulled out from under them. Conflicting information abounded, and this was a topic of conversation among PhD students. Some clung to the genetic-determinist outlook and hoped to build their career on it, while others recognized the new complexities and uncertainties were a game-changer (Marck & Martinez, 2018). Others would ask, “Why did he call it a ‘dogma’ anyway? Isn’t that non-scientific?” [↑](#footnote-ref-25)
26. This is a position that has put him into sometimes heated debate with other economists (Binmore, 2010; Gintis, 2011). [↑](#footnote-ref-26)
27. Note that the game theoretic treatment of social norms is a wildly 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). And 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 all Bicchieri, Binmore, and Gintis are all incompatible with each other (Paternotte & Grose, 2013). [↑](#footnote-ref-27)
28. 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 as well. [↑](#footnote-ref-28)
29. For a synopsis, see “A Cooperative Species: Précis” (Gintis, 2010a). [↑](#footnote-ref-29)
30. 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. But there are many connecting themes. For example, Graeber and Wengrow also seize on Aristotle’s term, meaning “political animal,” to express the human capacity for aversion to tyranny (Graeber & Wengrow, 2021, 86). [↑](#footnote-ref-30)
31. Gintis’ discussion of a “likely genetic predisposition underlying sociopathy” (Gintis, 2014, 200) will no doubt raise some eyebrows, for example—these sorts of arguments having a long history justifying disproportionate rates of incarceration by race and so on (Allen et al., 1975; Gould, 1996). [↑](#footnote-ref-31)
32. Bowles and Gintis’ (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’ 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; Gintis, 2011, 4). Nevertheless, after his twenty-year career as a Marxist, Gintis has long moved on some decades ago, and is outspoken about his disillusionment with Marxism. [↑](#footnote-ref-32)
33. For the purposes of this discussion, we will assume the mathematics and physics here are valid. Frankly, we are 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, we are mostly interested in giving a sense for how they use the metaphors involved and build a social scientific hypothesis. [↑](#footnote-ref-33)
34. 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). [↑](#footnote-ref-34)
35. 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. [↑](#footnote-ref-35)
36. This is an online publication without page numbers, but one can search for the quotes. [↑](#footnote-ref-36)
37. See (Gintis, 2009b). [↑](#footnote-ref-37)
38. But see the discussion on sociobiology, Gould and Wilson below. [↑](#footnote-ref-38)
39. 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). [↑](#footnote-ref-39)
40. 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 be added—he is unique in that his “sociopoesis” concept emphasizes and builds upon another complexity concept we have not had time to discuss—autopoesis, derived from Varela’s (1981) thought on the self-sustaining nature of organisms. [↑](#footnote-ref-40)
41. Highlanders in fact never used cacao and did not understand its utility, besides being a commodity for sale (Li, 2014, 123). [↑](#footnote-ref-41)
42. 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. [↑](#footnote-ref-42)
43. 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. [↑](#footnote-ref-43)
44. 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. [↑](#footnote-ref-44)
45. 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. [↑](#footnote-ref-45)