Epistemic Commitments Have No "Off" Button: On the Embodiment of Commitments by Way of Model Formulation

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Abstract:

The current paper examines how a commitment to a principle, adhered to by an individual agent, becomes an accepted standard of an epistemic community. Addressing this question requires three steps: first, to define the terms used throughout the paper, and especially the characteristics of commitments to a principle. The second step is to find a mechanism through which such epistemic commitments are introduced to an epistemic community and in certain cases are adopted as the standard by the community. While there could be several such mechanisms, the current paper focuses on the practice of model formulation. The third step is to demonstrate the analytical framework developed in the first two steps in a case study. The case study chosen for this paper is the unique approach to feedback analysis adopted by the ecologist and population geneticist Richard Levins. In what follows I will show that part of the features that made Levins' approach unique was his Marxist commitments, and his attempt to embody those commitments in feedback analysis by formally representing them as modeling assumptions.

Introduction:

The current paper examines how commitments to principles, that an individual agent adheres to, are introduced to an epistemic community such that the community adopts those commitments as general epistemic standards. Specifically, my paper focuses on the process of standardization from the side of the individual agent aiming to introduce her commitments to the community.

In order to cope with this question, I will address the literature concerning epistemic commitments and will argue that these commitments are *thick* in the sense that they contain both evaluative and non-evaluative aspects. I will argue that epistemic commitments have a compelling power over those who adhere to them; once an agent adheres to a commitment, it is not clear whether uncommitting is possible. Moreover, these commitments (while in effect) turn certain ways of interpreting the world to appear more self-evident and thus to require less degree of justification. These issues will be the focal point of the first two section of the paper.

While there could be several mechanisms through which commitments of an agent are introduced to and accepted by an epistemic community, my work concentrates on one specific mechanism, namely, model formulation. An important distinction in the context of the present paper is between "model" as a completed artefact that can be used in various ways for various ends, and the process of formulating that artefact. Model formulation is a common practice, but it is the artefact - the model itself - and not the practice of formulating it, that must meet certain epistemic standards so that the output of the model be rendered as "knowledge". Put differently, model formulation can introduce new commitments to a research community, whereas the mechanism of that introduction is not scrutinized.

The paper will demonstrate how committed agents work to standardize their commitments in the scientific community relevant to them. Accordingly, the fourth section will examine the modeling approach for complex ecological phenomena advocated by the population geneticist and ecologist Richard Levins. I will elaborate on Levins' commitments and see how they come into play in his modeling approach. The final part will examine what general lessons can be learned from the case study.

1. The characteristics of fundamental epistemic commitments:

The question that the current paper addresses is how a commitment of an individual agent becomes accepted standard of an epistemic community. Some distinctions are required to clarify this question. The first distinction is between commitment to a principle in general, and commitment to epistemic principle (or commitment) which is a narrower category. The second distinction relates to different levels of analysis: I consider commitments as pertaining to an individual agent. While communities also have commitments, the attributes of collective commitments may be radically different from those of a single agent. For that reason, when discussing an epistemic community, I will focus throughout the paper on the epistemic standards to which the community adheres. The current section means to define both ingredients of the term "epistemic commitment". Regarding commitments to principle, Michael Lynch offers the following definition (Lynch, 2014, p. 83):

> To commit to a principle is to adopt the policy of relying on it as true, of using it as a premise in our deliberations over what to think or do. Commitment is distinct from belief. It is different from belief because one can commit to a principle without believing, like the scientist who commits to a theory, and so relies on it in subsequent reasoning, while remaining agnostic about its ultimate truth. You can also believe without commitment – you can find yourself feeling that something is true without thinking it wise to adopt a policy of acting on that feeling. Commitment is also not mere assumption. To really commit to a principle means commitment to its truth, such that one no longer plans further inquiry into the matter. One takes the issue to be closed unless and until further considerations force you to reconsider. [Q1.1]

Thus, committing to a principle involves relying on it when making a decision that should be seen as "informed". It should be noted that [Q1.1] is not clear regarding the truth value ascribed to the principle. On the one hand commitment entails relying on the principle as true; on the other hand, the quote accepts the possibility of a scientist who relies on a principle *while remaining agnostic about its ultimate truth*. One way of reconciling these positions involves highlighting that Lynch's emphasis that the principle is being relied on "as true" indicates the instrumental approach toward such principles (as opposed to *really commit to a principle*). In this sense, the principle is seen as true within a given frame of reference, such as a scientific theory.

Epistemic commitments are a subclass of commitments that is very important for present purposes. According to Lynch, an epistemic principle is *a principle to the effect that some method for forming beliefs is trustworthy* (Lynch, 2013, p. 344). Put differently, epistemic commitments relate to ways of forming beliefs and entail value judgements about belief-formation. An appropriate method of belief-forming is supposed to result in a trustworthy belief, while the trustworthiness of a belief that was formed in an inappropriate way is questionable. Committing to such a principle means trusting the method it recommends, usually in a tacit way such that the committed agent is mostly unaware of the commitment.

In a similar vein to epistemic commitments, epistemic standards are the standards that an epistemic community considers as the appropriate procedures to base its beliefs on. In the professional assessment of such a community, a belief must meet those standards in order to be considered as "knowledge". In a sense, the introduction of an individual's commitment to the community and the adoption of a commitment as a standard by a community refer to the same process, seen from different vantage point. The term "introduction" prioritizes the individual agent, whereas the term "adoption" prioritizes the community to which that agent relates.

One subclass of epistemic commitments is fundamental epistemic commitments [henceforth: FECs]; these commitments are fundamental in that they cannot themselves be deduced from prior propositions. An example to a FEC is *deductive inference from true premises is reliable* (Lynch, 2013, p. 345). While other epistemic commitments could be justified by reverting to FECs, the same does not apply for the FECs themselves; thus, a central feature of FECs is that attempting to justify them produces epistemic circularity (Lynch, 2013, p. 344):

> The hallmark of a fundamental epistemic principle, and thus any fundamental epistemic commitment, is that the relevant principle can't be shown to be true without employing the method that the relevant principle endorses as reliable. Arguments in favor of such principles are therefore subject to what is sometimes called "epistemic circularity". [Q1.2]

Lynch follows Michael Bergmann in the definition of epistemic circularity as arising when the reliability of one's sources of belief comes into question (Bergmann, 2004, p. 710). The circularity derives from the inability to justify a FEC without referring to itself. Showing that a FEC is true entails *employing the method that the relevant principle endorses as reliable*. The problem arises when the principle itself is called into question. Such circularity poses a genuine problem when an agent is called to justify her FECs, and Bergmann and Lynch adopt very different approaches to handling this problem. Lynch is concerned with the way that commitments to different epistemic principles break the epistemic peerhood between interlocutors such that their ability to communicate across their respective commitments is hindered; accordingly, he considers it an imperative to be able to justify one's commitments, FECs included, and rejects some arguments that mean to circumvent the need to justify epistemic circularity.

To summarize, FECs are theoretically relied-upon and practically used methods and judgements (hence the aspect of commitment) that pertain to belief-formation (and are thus epistemic). They are fundamental in that they cannot be deduced from prior proposition, and thus their justification leads to circular argumentation. While concerns over the breakdown of epistemic peerhood and its effects on public discourse are important, they are not related to the introduction of commitments to epistemic communities. The reason that the two are not related is that when an agent is trying to introduce her commitment to her relevant community, she is working (in Lynch's terminology) to establish peerhood with her interlocutors. In these situations, the concern over the absence of peerhood is avoided. Thus, I move now to address some features of epistemic commitments that, due to differences of interest, are not being treated by Lynch.

2. Other features of FECs:

The frame of reference developed in the current paper highlights several features of FECs that Lynch does not thoroughly address. The first feature is that commitments are thick. The thickness of FECs is similar to the notion of thick ethical concepts, that originated in (Williams, 1986). The character that makes a concept "thick" is that it expresses evaluative as well as non-evaluative content.[1] The applicability of thick concepts to epistemology is a debated subject that exceeds the scope of the current paper; opposing views on the matter could be found in (Kyle, 2013; Väyrynen, 2008). It should be noted that the view that epistemic commitments are thick entails the assumption that the term "thick concept" indeed applies to epistemology.

Lynch makes a point to differentiate between relative fundamentality and absolute fundamentality. As for absolute fundamentality, Lynch writes that *a principle is absolutely fundamental if it is relatively fundamental to every system of beliefs* (Lynch, 2013, p. 345) and offers a procedure for the identification of absolutely fundamental epistemic principles.[2] The offered procedure evaluates the level of fundamentality of epistemic commitments inversely to their content, such that the more content-free an epistemic commitment is, the higher the chances it will be interpreted as absolutely fundamental. That means that absolutely fundamental epistemic commitments are also the closest to being "thin" commitments. However, by Lynch's own admission, even absolutely FECs are values in the sense that instructing an agent about the commitments she ought to adhere to has an intrinsic normative dimension (Lynch, 2014, p. 95). Under this view, even the most content free

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[1] A central question in the literature concerning thick concept is whether it is possible to decompose a thick concept into two or more thin ones. Despite its importance, this question exceeds the scope of the current paper.
[2] The procedure Lynch offers is a thought experiment he calls the "method game". This thought experiment aims to decide which FECs deserve a privileged position based on the reliability of the knowledge that they produce about an entirely unfamiliar environment (parallel earth). Essentially, Lynch suggests a Rawlsian "veil of ignorance" as a method to dispassionately prioritize different FECs.

level of epistemic commitments is still populated with thick commitments, which means that other commitments that are not as content free (i.e., relatively fundamental) must also be regarded as thick.

While it may seem obvious that non-epistemic principles are thick, it should be stressed that epistemic commitments also have evaluative content in the sense that they intend to instruct an agent about the appropriate way to understand the world; in itself, this feature is a value judgement that renders epistemic commitments as thick. Moreover, epistemic commitments could also be prescriptive, as illustrated by the term "rationality": when the aptitude of an action depends, among other considerations, on the appropriate way to understand the world, epistemic commitments are prescriptive in the sense that they have a claim about appropriate behavior.

This prescriptive nature leads me to another relevant characteristic of FECs, namely, that they can have a compelling power over their adherents. This compelling power implies several meanings. The first meaning is definitional to FECs; as discussed above, trying to justify FECs can lead to epistemic circularity since justifying a FEC entails evoking the FEC being justified. Put differently, these commitments are the benchmark against which epistemic commitments are evaluated. This gives FECs compelling power since they provide the standards of their own evaluation. Second, FECs are a default position; being a default position implies (a) that the agent may not be aware that she adheres to them, (b) that they are taken for granted to be correct, and (c) that the burden of proof always rests upon the side that objects to them.[3]

Accordingly, once an agent adheres to an epistemic commitment, it is questionable whether uncommitting is possible. For example, it is not possible to opt out of an epistemic commitment one is not aware of adhering to. Moreover, even if opting out of a FEC is theoretically possible, these commitments (while in effect) turn certain ways of interpreting the world to appear more self evident and thus to require less degree of justification, such that the adherents cannot simply decide to understand the world differently. In a sense, these commitments are the lens through which the agent sees the world; and since they are adopted tacitly (such that the committed agent is mostly unaware of the commitment), the agent cannot uncommit to them before she becomes aware of the existence of a commitment (this is a necessary condition, but not sufficient). Moreover, if the FEC is prescriptive with regard to ways of operating within the world, the committed agent can only understand and operate within the world in ways prescribed by her commitment. While the

[3] While accepting or adhering to a FEC can be done passively, rejecting it can only be accomplished actively.

compelling force of a commitment is merely an option, the combination of features results in the following postulation: disputes over epistemic commitments cannot be solved on purely epistemic grounds.

Finally, weather there are FECs;[4] whether the fundamentality of epistemic principles comes in various degrees;[5] and whether the method game is an apt procedure to identify absolutely FECs are all beside the point, since relative or absolute fundamentality affect the adherent in the same general way. Put explicitly, the level of commitment to an epistemic principle does not follow the level of fundamentality of that principle. A historical actor may accept the differentiation between relative and absolute fundamentality, and she may accept (through the method game or any other determining procedure) that the FEPs to which she adheres are merely relative in the terminology that Lynch suggests. It does not follow from that acceptance that the degree of commitment of the historical actor in question to her epistemic principles will be any lesser. The important point here is that, once accepted, an epistemic principle has the potential to compel the agent. This compelling power could be demonstrated by modeling traditions.

3. **Modeling traditions**

The previous sections concentrated on features of commitments; the current section focuses on the ways such commitments are introduced to an epistemic community and become standardized. One particular mechanism of introduction is model formulation, and I will claim that modeling traditions play a central role in this process. To be clear, it is not my intention to argue that model formulation and epistemic commitments are one and the same thing; the deployment of models is a standard scientific practice, and (for the most part) the formulation of models is unrelated to the commitments of the modeler. However, and this is the central point of the current section, model formulation could be a mechanism for introduction of FECs to a community.

By the term “modeling traditions” I refer to a process whereby practitioners in a certain discipline adopt a standard approach to modeling. Within this process a modeling toolbox, which includes the use of certain mathematical areas and other modeling practices such as agent-based modeling, becomes routinized. My assumption is that modeling traditions are the rule, rather than the exception: it is more efficient, and potentially epistemically fruitful, to formulate a model within

[4] Under Lynch’s definition, I would argue that this is an empirical question, and the answer to it will be positive.

[5] If we accept the general scheme, I do not see why relative fundamentality does not come in degrees.

a standard approach than to start modeling *de novo*. Moreover, a common practice in model deployment is to explore a new target system by applying a pre-existing model; such practice could be seen as a stand-in for formulating a new model. (Gelfert, 2016) dedicates extensive efforts to studying how old structures (models) are applied to new target systems, and (Weisberg, 2013, p. 75) claims that this application is a vital part of modeling and explains how such implementation is possible.

Gelfert and Weisberg are correct to ascribe such importance to the deployment of existing models to new contexts, for several reasons: first, since such deployment seems to be a common practice, and it should be addressed as such. Second, the very phenomenon that meaningful results derive from a re-deployed model is not trivial and deserves explanation. Yet, such application raises a concern that I refer to as "the carry-over phenomenon"; when a model is used to explore a target system that is different from its original one, it carries with it ideas and modeling assumptions that were appropriate for the original context, but when applied to the new context they can easily become implicit, or simply unexplored.[6]

To understand the problem with assumptions becoming implicit, it is helpful to differentiate between two kinds of assumptions that are introduced to a model during its formulation; type-1 modeling assumptions are introduced to the model for pragmatic reasons (e.g., so that the resulting model would be solvable). These assumptions are used despite the awareness of the model maker (and users) that they are not realistic. Type-2 modeling assumptions, on the other hand, reflect a belief concerning some state of affairs in the target system. It stands to reason that modelers will at least be agnostic with regard to the truth value of type-1 assumptions, and as a result, describing these assumptions as "unrealistic" is not likely to evoke argument. The same cannot be said, however, with regard to type-2 assumptions. Pragmatic modeling decisions could be criticized based on their perceived effects on the results of a model; however, an argument concerns a modeling assumption that is supposed to be true outside the model indicates that the argument verges on what Michael Dietrich describes as a controversy of principle (Dietrich, 2020, pp. 339-340):

> *Controversies of principle* are in general much more difficult to resolve than controversies of fact or theory, because a controversy over methodological principles may have no clear or agreed upon

[6] While the question of model adequacy (or lack thereof) can be evaluated on different scales, it should be noted that a model can be, in certain respects, inadequate even to its original target system.

> means for adjudication. […] Controversies of this sort over scientific standards speak of methodological issues that are often foundational. [Q2.1]

The carry-over phenomenon demonstrates how an epistemic commitment may persist over time and spread between fields; since there are no agreed-upon methods (let alone strict rules) for model-formulation, this process is not limited to theory and data. Formulating a model is a practice; as such, it could be construed in terms of tacit, rather than explicit, knowledge (Collins, 2010; Polanyi, 2009, 2015). While there is nothing inherently problematic with the inclusion of various considerations when formulating a model, the relations between the introduction of commitments and the carry-over phenomenon should be made explicit. Commitments are among the things being carried over from one context to another; from one context where they were explicit to another where they are taken for granted. This is one mechanism to transform commitments into standards. After being standardized, the implications of newly implicit assumptions may come to seem necessary.

To become standardized, a commitment must be embodied in the model as an assumption. Once it is identified, however, such an assumption could be seen as a "genealogical marker", in the sense that it could be used to flash out the process (model-formulation) in the artefact (model). To be sure, this is an in-principal aspiration that cannot always be met, due to inherent difficulties of model formulation, including the fact that the process of constructing a model is not always part of the public record; and that not all modeling decisions are intentional. Finally, and most importantly, even when model-builders specify the elements that were introduced to their models, requirements of the formal tools used for model-formulation may result in a conflation of ingredients so that non-epistemic commitments introduced to the model become indistinguishable from the epistemic ones. This observation is in line with the work of Marcel Boumans concerning that once a model is completed, it is not always possible to separate its ingredients, since they have to go through a process of mathematical moulding (Boumans, 1999, p. 90). The result of such moulding is that some of the ingredients are doing the justificatory work, but since disentangling the ingredients is not always possible, the justification applies to all ingredients.

Despite these difficulties, I think that the idea of a genealogical marker is potentially fruitful. There are three attributes of such markers: first, the modeling assumption is an assumption about the world and not merely an assumption required for technical reasons. Second, this assumption must be contingent in the sense that the model could be used to perform similar epistemic work (such as representing its target system) without it. In principle, such an assumption could be replaced.

Finally, the assumption should affect the results that derive from the model, and in turn, these models (potentially) take role in shaping subsequent research. The combination of these requirements implies that the contingent assumption affects not just the model's results, but also the subsequent research that builds upon them.

4. Richard Levins on the road to loop analysis.

I now turn to the works of the population geneticist and ecologist Richard Levins on feedback loop analysis. In the context of evolutionary biology, the first implementation of feedback analysis was made in (Fisher, 1930), where the notion of runaway selection relied on a mechanism of positive feedback. During the 1940s, Norbert Weiner and his colleagues developed many of the concepts and techniques related to feedback analysis, in their attempt to explain purpose-driven behavior. Weiner and his colleagues concentrated on negative feedback since this type of dynamic induces stability (Lamm, 2013). These works on negative feedback became especially important for genetics and molecular biology during the mid-1950s when researchers in the field attempted to understand mechanisms of molecular regulation (Fox Keller, 2002, p. 148). However, Levins' different approach to feedback analysis, as well as the theoretical problems that he tried to solve, render his use of feedback analysis exceptional. While Levins readily admits of his debt to Weiner's control theory (Morales & Levins, 1974, p. 22), he immediately highlights the differences between the original context of control theory and the biological context where this method was meant to be applied (Morales & Levins, 1974, p. 23):

> These [biological] complex systems differ in similar ways from the ones treated by electrical engineers in which the circuit elements are described fully first in isolation and then treated in well defined places in the circuit diagram. The variables of complex biological and social systems are only partially specified: only the directions of some of their effects are known and we are not given all of the interconnections. Indeed, it is not obvious what the appropriate subsystems and variables are […] Whatever the choice one makes it is clear here, as it is not in electronics, that the model is an intellectual construct put together for specific purposes. The issue is therefore not what the system really is, but rather which representations best reveal and which obscure what we want to know (or perhaps also wish to hide). [Q4.0.1]

Loop analysis is not a model nor is it a family of models. It is an approach to modeling that Levins had hoped to use in order to cope with complex systems. Levins was interested in the phenomenon of complexity in its own right, but he also thought that complex system analysis was the appropriate approach to studying ecological systems. As a rule, Levins did not define the general terms he had used, and in certain cases he even made a point not to define them (Levins, 1993, pp.

547-8); one exception, however, was the term "complexity". The quote above enumerates some of the features of complex systems; while Levins did not explicitly differentiate between features of complex systems as target systems that could be modeled and features of the model systems that formally represent complex systems, it is worth keeping this distinction in mind. The features of complex systems include:[7]

i. They are partially specified.

ii. Only the direction of the effects of parameters and variables upon the growth rate of other variables are known.

iii. There is no appropriate way, over and above the interest of the researcher, to divide such a system into subsystems [an issue extensively discussed in (Levins, 1970)].

iv. The analysis of a complex system produces epistemological rather than ontological results. No single analysis of a complex system can encompass all of its levels, all the connections within each level, and all the connections between different levels. As such, the results only reveal certain aspects of the system while obscuring others. Since there is no appropriate way to divide a complex system into subsystems, the analysis itself can only be performed in accordance with the motivations of the researcher. Thus, any result that such analysis produces is only correct within the decomposition that the researcher determined (and thus epistemological) and do not pertain to the system itself.

v. The central feature of complex systems is that they are difficult to study.

Levins insists (Levins, 1974, p. 125) that complex systems can only be partially specified, meaning that some of the parameters are either essentially not quantifiable, or that we do not have the means to measure them,[8] or that the resulting difference or differential equations would be analytically insoluble. Under these circumstances, the analysis of complex systems must be qualitative in the sense that the models used to study the system can represent and predict its general trend, usually the sign of the second derivative. Moreover, it should be noted that the number of variables in a complex system, the growth rate of pairwise connections between variables, and the requirement to treat each such connection with an equation of its own, together mean that describing such a system would involve a large set of simultaneous non-linear equations that either cannot be solved,

[7] Levins sometimes uses an opaque language that needs clarification. Particularly, the first two sentences of the fourth feature are by Levins, while the rest are my clarifications.

[8] These parameters are usually abstract, such as carrying capacity or selection coefficient. Even if it is possible to give them a precise mathematical definition, measuring them is more problematic.

or that the solution would be a *complicated expression in the parameters that would have no meaning to us* (Levins, 1970, p. 75). For Levins, loop analysis was meant to circumvent these restrictions.

4.1. Loop Analysis: a qualitative description for qualitative modeling approach

The unit of interest for loop analysis is a chain of variables that forms a loop; the variables in this chain are causally related, and a loop is formed when the output of the system is routed back as an input. When Levins engaged with loop analysis, the variables he was interested in were factors in an ecological community such as *levels of resources used by the species* (Levins, 1975, p. 17). When considering an ecological community of *n* such variables with high degree of connectivity, an intuitive way to represent the community is with a matrix, with the variables conceived as nodes in the directed graph equivalent to the matrix. However, since the system is partially specified, it is not possible to know the magnitude of the effect of one node upon the growth rate of another node; rather, only the direction of the effect is known. As a result, the matrix is filled with signs for the direction of the effect: plus, if the effect is positive, minus if the direction is negative, and zero if there is no effect.

The important point is that the growth function of a given variable depends on other variables in the system (as well as certain parameters). If it were possible to assign values to the community matrix, then the dynamics of that community could have been represented by a set of *n* differential equations, each corresponding to the rate of change of the variable it handles; however, since the actual values are not known, Levins tried to deploy partial differential equations. The crucial point of loop analysis is that in the neighborhood of any equilibrium point there is an equivalence between the set of partial differential equations f_i that describe the rates of change for a set of variables x_i and the community matrix of the system. In such a system, there may be one or more equilibrium points; the meaning of an equilibrium point in this context is that the system returns to that point after small perturbation (Justus, 2006, p. 654). The "neighborhood" of an equilibrium point is the region where that point functions as an attractor. In this region, the differentiated terms $\partial f_i / \partial x_j$ are the elements a_{ij} in the community matrix (Levins, 1974, p. 125).

Moreover, the community matrix could be represented by directed graphs such that the variables $x_1, \dots, x_n$ correspond to vertices of a digraph. The definition is restricted to the neighborhood of an equilibrium point since it is highly implausible for an ecological system to be globally stable;

global stability is a feature of linear systems, but the ecological systems Levins was interested in (and complex systems in general) are clearly not linear (Justus, 2006, p. 661). In these conditions (Levins, 1975, p. 18):

> The qualitative properties of this system are represented by a "diagram", i.e., a picture that illustrates just where each variable fits into the system. Each variable is represented by a point or *vertex* in the diagram. The relations among variables appear as oriented *links* connecting the vertices, or variables, so that the line connecting X_j to X_i represents the interaction or effect of X_j on X_i and corresponds to the matrix element a_{ij}. A link or series of links that leaves and eventually reenters the same vertex is called a *loop*. Corresponding to the diagonal matrix elements a_{ij} there are loops that connect each X_i to itself, termed *self-loops*. These self-loops will be considered to be of unit *length*, and by analogy other loops will be of length 2, 3, or more depending on whether they are composed of two, three, or more of the elements a_{ij}. [Q4.1.1]

This quotation refers to several levels in the formation of a loop. The lowest level is a single vertex, that might affect itself at discrete time iterations (for example, the growth rate of a species could be self-damped at time *t+1* if at time *t* it exceeded a density threshold). Above the level of a single vertex there is the level of a link between two vertices; it should be noted that Levins was interested in the qualitative effect of X_j on the levels of X_i, where the value of the link connecting X_j to X_i is not known, but the sign between them can be zero (no link), positive (X_j increases X_i) or negative (X_j decreases X_i). A loop emerges when an output of a given vertex is routed back through one or more links as an input to the same vertex. The following figure, taken from (Levins, 1975, p. 19) gives an intuitive sense of Levins' intentions:

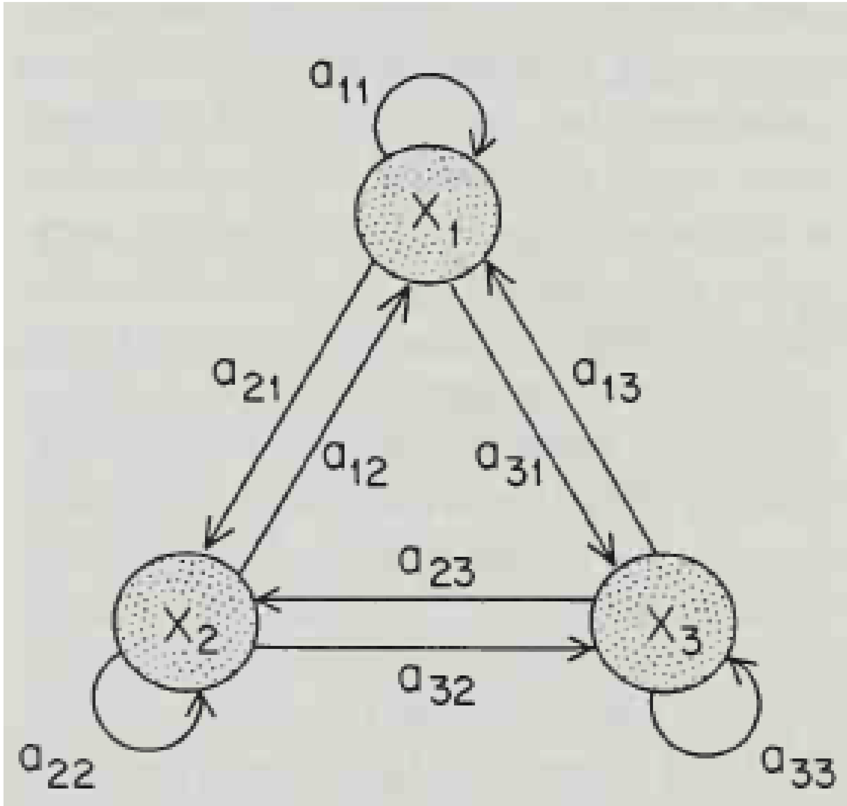

Figure 1: a three-variable system. Taken from (Levins 1975, p. 19).

Two related features of loops should be highlighted here: first, a loop could have several nested levels. A vertex may form a self-loop with itself, but the same self-loop could also be part of a higher-order loop. Loop analysis assumes discrete time units, with time intervals being measured through the iteration of a loop; for a loop of a given level, the relevant unit of time is the amount of time it takes for the loop at that level to close. Second, since the link between vertices must be either positive or negative (zero denotes absence of a link), when a loop closes, it has feedback that can be either positive[9] or negative.[10] In what follows, I will elaborate on each of these features, and explain how they are related.

The first important distinction, with regard to levels of a loop, is between two loops of the same length that share at least one common vertex (*conjunct* loops), and two loops of the same length that share no common vertex (*disjunct* loops). Levins denotes the number of disjunct loops in a

[9] Qualitatively speaking: an initial increase in the growth rate of a given variable results in further increase and initial decrease produces further decrease.

[10] Qualitatively: an initial increase in the growth rate of a given variable results in subsequent decrease and vice-versa.

given system with m and the number of vertices of a given loop with k. He then presents the following equation (F_k) that defines the sum of all possible products that involve just k vertices and consist of m disjunct loops (Levins, 1975, p. 20):

$$F_k = \sum (-1)^{m+1} L(m,k)$$

The product $L(m,k)$ is the product of k links which form m disjunct loops. If all loops are negative, F_k will also be negative; otherwise, the sign of the summation depends on the value of $(-1)^{(m+1)}$ which depends on the value of m. When m is an odd number, both the term $(-1)^{(m+1)}$ and the summation will be positive; when m is an even number, both the term and the summation will be negative. Since positive feedback increases the original effect (an initial increase results in further increase and an initial decrease in further decrease), when the system is predominated by positive feedback, it moves away from equilibrium in the same direction as the initial effect, thus rendering the equilibrium unstable (Levins, 1974, pp. 127-8). Thus, the first condition for the stability of a system is negative feedback at loops of all levels.

However, negative feedback is just one condition for stability of a system. the second requirement for stability is that the feedback at lower levels be stronger than that of higher level (Justus, 2006, p. 654). As Levins noted, since there is no direct way to measure the strength of a feedback, the second condition is not as clearly defined as the first. Thus, Levins required an indirect evaluation of strength, and the stand-in indicator he chose was the length of time required for the completion of an iteration at a given level. Loops of different lengths are characterized by iterations of different duration, and with this operationalization of strength, the second condition for stability is that *negative feedback with long-time lags cannot be too large compared to the shorter-loop negative feedback* (Levins, 1974, p. 128). This condition requires that feedback at a higher level to be less than the product of the negative feedback at lower levels. Put differently, the time it takes a loop to close must be a bounded function that does not increase indefinitely together with the number of vertices of a loop. Denoting F_i as feedback of the i^{th} level, the following inequality demonstrates the second condition for loop with three levels (Levins, 1974, p. 128):

$$F_1F_2 + F_3 > 0$$

We should remember that a precondition is that feedback at all levels is negative, such that the product F_1F_2 is positive. Levins focused on whether a loop is positive or negative since this question determines whether a system is stable or not; when he engaged in loop analysis during the late 1960s and early 1970s and implemented that method in his ecological models, he perceived loop analysis as a means to understand the features of the system that determined, first, whether a system has an attractor (or multiple attractors) and is thus stable in a given region, and second, the scenarios where the system is stable, as well as those where the system loses its stability. To clarify, Levins was not interested in the presence of an attractor as such, but on the attributes of the system that determine the existence of an attractor (Morales & Levins, 1974, p. 22):

> In mathematical terms, a system is at equilibrium if none of its variables are changing. If such an equilibrium is stable, then it will return to equilibrium after a small displacement. But if the equilibrium is unstable, a small displacement in any direction is sufficient to cause it to move further away from that state. […] Therefore the particular cause of change is less important than those properties of the system which caused the equilibrium to be unstable. [Q4.1.2]

4.2. <u>How does loop analysis embody the principles of the dialectical view?</u>

As discussed above, one attribute of a system that makes it difficult to study is multiplicity of variables that mutually influence one another, such that the growth rate of a given variable can interfere with itself, albeit in an indirect way. The important point for Levins, in this regard, is that the influence between different components of the system is not unidirectional but works, directly or indirectly, in multiple directions (Levins, 1970, p. 74). The following highlights what Levins wanted to achieve by formalizing methods for complex system analysis (Levins, 1970, p. 73):

> The question under consideration is whether we can develop a fairly general and widely applicable theory on the structure and dynamics of complex systems, which would be applicable to work in biology at the level of the population, the cell, development, and perhaps be of some use in the analysis of other complex systems of a social kind, even of the complexity in the evolution of languages and other areas. This does not mean that we can have a theory which will provide the answers to problems in those fields. […] However, what we can hope to do with a fairly general theory of complex systems is, as a minimum, to warn against certain kinds of conclusions that do not necessarily follow. [Q4.2.1]

As this quotation shows, Levins was seeking an analytical approach that would be (to some extent) content independent and applicable to various levels of analysis. On the other hand, his expectation from that approach were restricted to a boundary work between conclusions about the system that are necessary and those that are not necessary. The "necessity" of the conclusions should be content independent in the sense that they derive from the attributes of the system that make it complex. But complex system analysis for Levins was not merely a method to studying a given set of systems with certain characteristics that render them as "complex"; rather, it was an ontological and epistemological commitment, and Levins associated that commitment with his adherence to the Hegelian-Marxist tradition (Levins, 1974, p. 124).

To be sure, the Hegelian-Marxist tradition could be understood in numerous ways; however, the following should be made clear: in the terminology used in the current paper, Marxism is a thick commitment in the sense that it has different evaluative and non-evaluative contents, that are not easy to untangle. Marxism contains ethical, political, and ideological components. But it also contains epistemological and methodological aspects aimed at understanding the world around us. These aspects are themselves normative in the sense that they also have claims about how the world should be understood. In the terminology of the present paper, Marxism is not just a thick concept, it is an epistemic commitment. My goal here is to identify the methodological and epistemological commitments that Levins recognized as Marxist. Levins' earliest systematic treatment of the methodological implications of his Marxist commitments appeared in the concluding chapter of (Levins & Lewontin, 1985),[11] where the authors defined four principles of their dialectical view:

- A whole is a relation of heterogenous parts that have no prior independent existence as parts.
- The properties of parts have no prior alienated existence but are acquired by being parts of a particular whole (Levins & Lewontin, 1985, p. 273).
- The interpenetration of parts and wholes is a consequence of the interchangeability of subject and object, of cause and effect (Levins & Lewontin, 1985, p. 274).
- Because elements recreate each other by interacting and are recreated by the wholes of which they are parts, change is a characteristic of all systems and all aspects of all systems (Levins & Lewontin, 1985, p. 275).

Finally, it should be noted that for Levins and Lewontin, dialectical reasoning is analogous to first principles; these are the epistemic lens through which they see the world (Levins & Lewontin, 1985, p. 268). In the terms used in the current paper, for Levins and Lewontin, the dialectical view is a fundamental epistemic commitment. With these features of a dialectical view in mind, the next step would be to find out how they are embodied in loop analysis.

All these principles are quite abstract and could benefit from illustration; in what follows, I will elaborate on each of these principles, and explain how they are embodied in Levins approach to loop analysis. Regarding the first principle, *whole is a relationship of heterogenous parts that have*

[11] Since the works discussed in the present paper were published between 1968 and 1975, anachronism is a genuine concern when referring to *The Dialectical Biologist*. However, the similarities between the Marxist terminology that Levins had deployed in the works from 1968-1975 and the Marxist terminology of *The Dialectical Biologist* indicate that in 1985 Levins explicitly expressed previously held notions.

no prior independent existence as part, Levins and Lewontin offer such illustration when they describe the role of the ecologist, and the difficulties that the ecologist faces when fulfilling that role (Levins & Lewontin, 1985, p. 272-3):

> The community in ecology does not lose its meaning as a unit of analysis nor its effectiveness as a level of interaction just because it is possible to connect every species in the world with every other one by some long chain of remote biotic interactions. The problem for the ecologist is not to divide up the world of organisms once and for all into communities, but to look for groups of species within which there are strong interactions and between which there are weak relations in particular circumstances. A single species may be part of two communities without thereby joining those communities into one. [Q4.2.2]

This quotation highlights an important aspect in the dialectical view, namely, that it acknowledges the parts of the system (species in an ecological community for example) but it is more interested in the relations and interactions between parts of the system than it is in the parts themselves. This is an aspect of a broader ontological commitment, whereby properties of the parts derive from the whole in which they take part: *the fact is that the parts have properties that are characteristic of them only as they are parts of wholes; the properties come into existence in the interaction that makes the whole* (Levins & Lewontin, 1985, p. 273). While parts may have properties outside the whole, the whole can bestow new properties upon the parts; these new properties could negate the properties of the parts when outside the whole. Since the properties that the whole bestows upon the parts can negate the properties of the part when they are outside the whole, the properties that the whole bestows should not be treated as less fundamental than the properties of the individual part.[12]

As for the way that loop analysis embodies this principle, some historical background can help to elucidate the matter. When engaging with loop analysis, Levins was not only interested in formulating a general approach to studying complex systems, but also with a specific question about the nature of complexity. The question was whether complexity, as a feature of a system, induces stability or instability in the system it characterizes. Robert MacArthur, a close colleague of Levins, supported the hypothesis that an increase level of complexity induces stability (MacArthur, 1955), whereas Robert May deployed differential equations to demonstrate that an increase level of complexity induces instability (May, 1972).

[12] The example of Levins and Lewontin (p. 273) is flying: a person cannot fly, nor can a group of people. But due to certain social organizations, people (individuals in society) do fly. The social organizations that allow air travel have negated a physiological restriction.

When a system is completely specified, analyzing it with differential equations is both technically possible and sufficient in the sense that the equations cover all possible dynamics of the system. As discussed above, Levins insisted that complex systems can only be partially specified since some of the parameters are either not quantifiable or unmeasurable. However, the standard way to treat such parameters is to exclude them from the model entirely.[13] One feature of Levins' modeling approach that makes it unique is his insistence to include the parameters that are not quantifiable. This qualitative approach relates to Levins' need to cope with May's result; if the system is only partially specified, then the differential equations do not capture the system in its entirety. The trade-off in this situation being that differential equations could not be technically used to describe the areas where the system is only partially specified.[14] As much as Levins might have objected to May's approach, he could not have rejected it out of hand; indeed, as a solution to the dispute between MacArthur and May, loop analysis can accommodate both hypotheses. I will elaborate on that point later in the current section. The important point for the moment is that the insistence that the system be only partially specified on the one hand, and that partial specification is enough for the purpose of (qualitatively) model the system on the other hand. These are modeling decisions, and they relate to dialectical commitment.

Regarding the second principle, that *properties of parts have no prior alienated existence but are acquired by being parts of a particular whole*, as [Q4.2.2] shows, Levins and Lewontin recognize that much of the interactions in the whole are irrelevant in practice. Accordingly, the problem is to recognize the important interactions. While the ability to assign significance is content-dependent, loop analysis was meant to provide a more abstract way to estimating the importance of different interactions in the loop.

As discussed in section 4.1, the crucial point of loop analysis is that in the neighborhood of any equilibrium point there is an equivalence between the set of partial differential equations that describe the rates of change for a set of variables (with each equation dealing with a single variable) and the community matrix of the system. This equivalence, together with the use of matrix algebra without specifying the values of the elements in the matrix (only whether it is positive or negative) in conjunction with differential equations, are the features that differentiate loop analysis from other modeling approaches. The question then becomes, what does this particular use of matrix

[13] And then to acknowledge that the model is not realistic in its treatment of those parameters.

[14] I am grateful to Prof. Sahotra Sarkar for highlighting this point for me.

algebra give? What did Levins gain from using this technique that he could not have gained otherwise?

The particular use of matrices enabled loop analysis in the following way: being a qualitative approach to model formulation, loop analysis is only interested in the type of link between vertices. The loop analysis modeling approach thus treats the vertices themselves equivalently, such that the qualitative differences between vertices do not influence the relations between them. Put differently, while the loop may consist of any number of types of vertices, the model can only treat them as if they were the same and focus on the relations between them. This kind of treatment is in line with Levins' notion that *the issue is therefore not what the system really is, but rather which representations best reveal and which obscure what we want to know (or perhaps also wish to hide).* Levins enumerated his reasons for advocating loop analysis already in (Levins, 1974, p. 124), using terms that strongly resonate the dialectical approach as conceived in *The Dialectical Biologist*:

> [The] Marxist tradition has always emphasized complexity itself as an object of study and has stressed interconnection, wholeness, qualitative relations, multiple causality, the unity of structure and process, and the frequently contra intuitive results of contradictory processes. [Q4.2.3]

So, how does loop analysis solve the problem of assigning significance? In the easy case, there is simply no connection between the vertices. Whenever there is no path leading from vertex A to vertex B, there is simply no way for A to influence B. However, that does not actually solve the problem; the problem is assigning significance to an existing connection so that its importance to the working of the system could be estimated. In the more subtle case, there is a path from vertex A to vertex B, but the properties of the path are such that the influence of A upon B are so marginal that they can be safely ignored.[15] Being only interested in the type of link between two vertices, loop models can assign significance to these links. Comparing significance of different links could be used to recognize the important interactions.

The meaning of the third principle of the dialectical view [*the interpenetration of parts and wholes is a consequence of the interchangeability of subject and object, of cause and effect*], relates to the stance that there is no single appropriate way to study complex systems independently from the interest of the researcher. The status of an element of the loop as "cause" or as "effect" depends on the causal path that the researcher studies. From the perspective of the loop itself, any two elements

[15] For example, a very long path between the relevant vertices, where there are both positive and negative links in the middle could result in an exceedingly small influence, effectively indistinguishable from absence of path.

are interchangeable with regard to the causal direction between them; however, it should be noted that the causal path in the direction from vertex X to vertex Y can be significantly shorter than in the other direction. This principle is embodied in the treatment of the variables as if they were of the same type. The elimination of differences between types of entities, discussed above, is the very thing that allows both qualitative relations and multiple causality (or the bidirectionality of causal effects).

The system that results from this elimination could be used to study, for example, the interrelations between an organism and its environment. Moreover, since we are discussing loops, it does not matter where the analysis begins: every variable could be seen as a parameter of other variables, and the important decision is what variable we are interested in. Put differently, no single variable receives ontological priority over the others. The status of a given variable as “cause” or “effect” is entirely epistemic in the sense that it derives from the thing we want to explain and is in no way intrinsic to the variable. This is at least correct with regard to the types of relations between entities (even if not to the entities themselves). The entities are treated the same, which is not surprising, given that loop analysis is interested in the type of relations between them.

One approach that Levins had rejected as inadequate to coping with complexity was reductionism; Levins thought that reductionism was prevalent in modern science and regularly expressed his discontent with it. Levins considered reductionism to be inadequate in several ways, but the one most relevant here is that the explanatory power of reductionism derives from ignoring or distorting the phenomenon it sets to explain. While Levins did not object to distortions as such, and models certainly introduce such distortions, the pressing issue for Levins was the function of the distortion. Modeling strategies introduce various distortions, but they are meant to overcome our cognitive limitations while allowing the model explanatory power. According to Levins, the distortions that reductionism introduce to the explanation serve nothing but the commitment to reductionism (Levins, 1973, p. 110). It should also me noted that in several places, Levins connects his rejection of reductionism with his Marxist commitments, e.g., (Levins & Lewontin, 1985, p. 274-5). Levins’ rejection of reductionism is the reason that he could not have accepted May’s result that an increase in the level of complexity reduces the stability of the system. This result entails the conclusion that the complexity level of all systems that are stable enough to endure over time is limited. This, in turn, would mean that reductionism is the correct epistemological and methodological approach.

It could be argued that whether a reductionist analysis of a complex system can be carried out is an empirical question; such an account, if possible, would disconfirm the dialectical approach that Levins advocated. While this argument is valid, it also demonstrates the thickness of the dialectical approach. As seen in part 2, FECs are thick in the sense that they have evaluative content; when it comes to epistemological commitments, that content relates to the evaluation of the aptitude (or lack thereof) of a principle to produce reliable knowledge. Levins' rejection of reductionism shows that while his dialectical approach is not anti-empiricist, it lays claim to the differentiation between empirical and non-empirical questions. Within this view, it is entirely uncertain that the possibility to reductionistically analyze a complex system is indeed an empirical question; in fact, the second principle seems to dictate that the question is *not* empirical. Committing to a dialectical view, as Levins and Lewontin envisioned it, entails the evaluation that reductionism is, categorically, an inapt approach to complexity.

Returning to the dialectical principles suggested by Levins and Lewontin, the first principle can be seen as the commitment from which the argument that the properties of a complex system cannot be fully specified derives. But the combination of the second and third dialectical principles go further than that: it is not just that a complex system cannot be reduced to its constituent parts; rather, some of the properties of the parts can only be determined through the dynamics of the system.

Finally, two points are relevant with regard to the principle that *change is a characteristic of all systems and all aspects of all systems*. First, as discussed above, the discrete time iterations that Levins assumes were meant to determine whether the system was locally stable, but Levins was highly skeptical of global stability in systems such as ecological communities, since global stability characterizes linear systems. Second, while the use of two mathematical techniques when formulating a model does not, in itself, guarantee that the resulting model will not be reductionistic, other than the discrete time iterations there is nothing in Levins' approach to loop analysis that treats the entities that form a loop hierarchically.

These two points highlight the ways that loop analysis embody the fourth dialectical principle in the following way: at the level of a single variable, each is assigned with a partial differential equation, but that equation only concerns the *growth rate* of the variable, not explaining it (let alone mechanistically). In this sense this qualitative approach entails treatment of the variables as if they were "black boxes". It should be noted that even the boundaries of these black boxes are not determined independently from the researcher. In certain cases the researcher only determines whether an element of the system is a variable or a parameter; in other cases, however, it is the researcher who determines what constitutes the "element" of the system.

Moreover, at the level of the loop, all constituent variables are, in a sense, on the same level; the loop as a whole only treats the direction of influence of one variable upon another, with "influence" relating to changes in growth rate of the affected variable. More generally, Levins was not simply interested in the equilibrium points as such; rather, he was interested in the features of the system that determine whether the system has an equilibrium point, how many equilibria points there are, whether those equilibria are stable or not, and how to differentiate stable from unstable equilibria points. It should also be noted that loop analysis is meant to explain the stability or instability of the system through interactions between its components rather than reduction to them.

As seen with regard to the first dialectical principle, Levins suggested an analysis method for complexity, where under certain conditions complexity would lead to stability. Those conditions are not influenced by the level of complexity of the system, or by the increase (or decrease) of that level; if the conditions are met, the system will be characterized by negative feedback and thus will be locally stable. On the other hand, Levins' specification of the conditions for negative feedback shows that it has more requirements than instability-inducing positive feedback. Since more conditions must be met to achieve negative feedback, it should be seen as the exception, rather than the rule.

5. What general conclusions can be inferred from Levins' approach to loop analysis?

In the first section we have seen characteristics of FECs that include adopting a policy of regarding a given principle as true (commitment); that the principle in question relates to the level of trustworthiness attributed to a given method of forming believes (epistemic); that such principle is circular such that it cannot be shown to be true without employing the method that it endorses as reliable (fundamentality). In the second section I have added two more characteristics to FECs, namely, that such commitments are thick in the sense that they express evaluative as well as non-evaluative content, and that they could have a compelling power over their adherents in the sense that they are default positions that must be proven wrong in order for an agent to stop adhering to them. Given the other characteristics of FECs, it is questionable whether it is possible to prove them wrong on purely epistemic grounds.

In section 4.2 we have seen that loop analysis embodies a dialectical view of nature that is informed by Marxist commitments; before concluding, it should be highlighted that Marxism meets all the criteria of FEC. Marxism is a thick commitment since it has both evaluative and non-evaluative contents. Marxism contains epistemological and methodological aspects aimed at understanding the world around us. Levins' adherence to the epistemic principles that derive from Marxism demonstrates both the commitment aspect and the epistemic aspect described here. The final two characteristics (fundamentality and compelling power) relate to their self-justificatory nature; and while the compelling power of a FEC tends to relate to the agent's lack of awareness to her own commitments, this is not a necessary condition, and it is certainly not the case with Levins. Indeed, loop analysis vividly demonstrates that an agent's awareness of her own FEC does not necessarily diminish her level of commitment to them. Levins kept insisting that his commitments were not merely ethically but also epistemologically superior to the alternatives, and even tried to embody them in a scientific research program by giving them a formal representation. Put differently, an agent could be aware of her commitments while introducing them to the relevant community, thus turning them into general standards.

Having clarified that, a few points could be drawn from the way loop analysis embodies a dialectical view of nature, when this view is construed in terms of FEC. I would like to address two such issues; the first issue relates to the introduction of commitments to a scientific community in a way that changes the status of that commitment from being considered non-epistemic to being considered epistemic. Such introduction could be described as a process where value-judgements (whether ideological, political, or otherwise) are re-conceptualized in a terminology that obscures their normative aspect. This re-conceptualization need not be intentional, yet intentional or otherwise, it allows the deployment of accepted scientific standards when discussing those value-judgements. A complementary, although not necessary, step is the construal of these judgements such that a value-free decision concerning them is not seen as an in-principal possibility, but as an in-fact settled matter.[16]

[16] An example may help explain my point; while in *The Selfish Gene* (1976) Dawkins had advocated the gene-centered view of evolution, in his *The Extended Phenotype* (1982) he argued that the matter is essentially closed. Seeing that the current paper is written in 2024 and the controversy over levels of selection is still very much an open discussion, one could argue that Dawkins's portrayal of the state of the argument from 1982 is not entirely accurate.

This is, of course, a very coarse definition, but loop analysis actually demonstrates it rather well. As discussed, Levins' implementation of loop analysis was recognized at the time as idiosyncratic, and this idiosyncrasy is related to his attempt to formally represent a dialectical world view. However, with the benefit of hindsight it is quite safe to say that by the time the current paper is written, Levins' approach to loop analysis did not materialize as a viable research program. This is a sociological, rather than epistemological statement; Levins did not concede that his approach was problematic and continued to develop his method in the decade following 1975, a work culminated in (Puccia & Levins, 1985). One could argue that Levins' approach to loop analysis also involves an epistemological failure, namely, that he tried to formulate differential equations from partially specified community matrices, whereas Pfaffian differential forms might have been a more appropriate approach to use with such matrices.[17] However, this argument is based on the benefit of hindsight, since Levins was not aware of Pfaffian differential forms when working on loop analysis. Since our awareness to the epistemological failure of loop analysis is based on the benefit of hindsight, the sociological failure (that by and large ecologists' interest in loop analysis faded away) is more easily attributable to loop analysis.

Niche construction demonstrates that point: the relations between an organism and its ecological niche seem like a prime candidate for analysis with loop analysis, and Levins himself engaged with these relations starting from (Levins, 1968b). However, the central works that concern niche construction from (Odling-Smee et al., 2003) onwards do not use the modeling approach that Levins advocated; to add insult to injury, the standard modeling approach in these works is ecosystem ecology, the very approach that Levins considered inappropriate (Levins, 1968a). To be sure, there are legitimate reasons to reject loop analysis and these reasons could be demonstrated the following way: formalizing the requirement that feedback at a higher level be less than the product of feedback at lower levels[18] intuitively generalizes to:

$$\prod F_n > -F_{n+1}$$

Generalizations of this kind do not work for Levins' benefit for two reasons; first, F_i is assumed to be negative, such that the right-hand side of the inequality is actually positive. However, if *n* is an odd number, the left-hand side of the inequality is the product of odd number of negatives (thus

[17] I am grateful to Prof. Stuart Newman for highlighting this point for me.

[18] $F_1F_2 + F_3 > 0$

the product is also negative) and we receive a result whereby a negative number is larger than a positive. To be sure, Levins could have easily gotten to the same generalization, but it seems that at the mid-1970s he was not concerned with the general case but with a simpler nested loop of four or five levels; the point, however, is that this absurdity demonstrates one ground for criticizing qualitative modeling in general, namely, the standard of mathematical robustness (M. W. Feldman, personal communication, January 18, 2023). We have seen that Levins presented an outline for a qualitative modeling approach, but this can raise two concerns about robustness: first, the notion of qualitative modeling can simply be rejected. Second, it could be stressed that Levins only gave a mathematical sketch of loop analysis without developing it in full. It should be noted here that Levins himself grappled for some time with the mathematics required to complete this outline and knew that others will have to pick up the project from where he left it. In the current example, the issue is not that Levins failed to generalize a formula, rather, he probably did not consider it necessary to generalize beyond the loop's fifth level.

The second hazard that the generalization above demonstrates is that without more strict guiding lines for the continuation of his work, Levins' approach to loop analysis runs the risk of becoming a modeling approach without closure (Huss, 2022). Put differently, loop analysis does not offer a readily available criteria for inclusion and exclusion of either parameters or variables, since all variables are treated the same and each can act as a parameter for others. The absence of criteria for inclusion and exclusion means that loop analysis has the potential of producing a model that encompasses the entire target system.[19] The decision of when to stop modeling remains entirely at the discretion of the modeler.

However, the central point of the current discussion is that once Levins articulated loop analysis as he did, it had to be dealt with in its own formal terms; put differently, it was no longer possible to reject loop analysis off-hand on the ground of being "Marxist-driven". This point demonstrates one general conclusion that can be drawn from the case study, namely, that the very formalization of a (private) commitment is an important step in its transition into a (general) standard. In this sense, model formulation is a central mechanism for the introduction of commitments to epistemic communities.

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[19] There is a level of irony here, since Levins thought that such complete encompassing of a complex system is impossible.

The second issue that could be inferred from the case study is that programmatic failures also have different levels. We have seen that the methodological approach to modeling feedback loops that Levins advocated did not materialize into a widespread research program. Moreover, while Levins did not aspire to turn ecologists into Marxists, if he had held such motivation, that would have been another failure. However, there are certain aspects where Levins did succeed; first, ecology as a discipline adopted the approach that ecological systems must be understood in terms of multiple causality and that causality within such systems is not linear and unidirectional. This acceptance implies some degree of commitment to dialectical ontology. Second, while not accepting Levins' particular approach to qualitative modeling, the discipline did accept the reasons he had offered for the need of such approach, and qualitative modeling is a legitimate approach in ecology, as demonstrated by (Dambacher, Li, et al., 2003; Dambacher, Luh, et al., 2003). Put differently, Levins' approach was not without its triumphs, even though the triumphs it did have may not have been those envisioned by Levins.

Conclusions

The question at the center of the current paper was how a commitment to a principle, adhered to by an individual agent, becomes an accepted standard of an epistemic community. Addressing this question required several steps: the first step was to define the terms used throughout the paper, and especially the characteristics of commitments to a principle. For that purpose, the first section followed Lynch's definitions of epistemic commitments; however, since Lynch addresses different set of questions than the current paper, the second section was dedicated to highlighting some other characteristics of commitments that Lynch does not address, namely, that epistemic commitments are thick in the sense that they contain both evaluative and non-evaluative aspects, and that they could also have a compelling power over those who adhere to them.

Once the main characteristics of commitments had been defined, the second step was finding a mechanism through which commitments with those features are introduced to an epistemic community and in certain cases are also adopted as standard by that community. One mechanism that can bring about that result (although not the only one) is to give them a formalized representation as a modeling assumption. Accordingly, the third section focused on modeling traditions, a process where practitioners in a certain discipline adopt a standard approach to modeling, such that the modeling toolbox becomes routinized. While my assumption was that modeling traditions are the rule, rather than the exception, I highlighted that the deployment of an existing model to a new context raises some concerns that derive from the transition of modeling assumptions from being explicit in the original context to being implicit in the new context, a situation dubbed as "carry-over phenomenon". This phenomenon could be a mechanism for modeling assumptions to become epistemic commitment, and this is especially severe with regard to assumptions that are meant to reflect a belief concerning some state of affairs in the world outside the model.

Finally, since the discussion in the first two steps was abstract, the third step was to demonstrate the analytical framework developed in the first two steps in a case study. Thus, the fourth section focused on Richard Levins' unique approach to loop analysis, and especially the way it embedded his commitment to a dialectical view of nature, a methodological and epistemological commitment that derived from his interpretation of the Hegelian-Marxist tradition. The fifth section highlighted two observations that could be generalized from the case study: first, once a commitment is formalized, it can be rejected, but not on the grounds of the motivations behind it. The second observation was that the terms of success or failure of an agent to persuade her community to adopt her commitments as general standards could be divorced from the agent's own terms of success.

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