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Experiments in critical mathematical modelling

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by

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To the extent that it remains obscure, irrelevant, irresponsible, incomplete, obnoxious, or erroneous, I claim all credit.

Abstract of the Thesis

Experiments in critical mathematical modelling

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The ongoing global economic crisis motivates the following questions: How does economic knowledge, and mathematical modelling in particular, act in the world? Indeed, how does it become 'knowledge'? What are the ethical commitments and political implications and stakes of such knowledge-making practices? Are the practices and dominant frameworks in contemporary economics useful in understanding the current crisis? Useful for whom? If the assumptions and commitments that inflect and undergird economic knowledge and knowledge-making prove to be impediments to understanding rather than preconditions for it, how can one build a modelling practice that is more useful? What exactly is meant by 'useful'? Again, for whom? Assuming what objectives?

This thesis grapples with these questions, both theoretically and by the construction of mathematical indices and models with working visualization interfaces.

Chapter 1

Introduction: Artistic practice and political action

The purpose of studying economics is not to acquire a set of ready-made answers to economic questions, but to learn how to avoid being deceived by economists.
—Joan V. Robinson

What is the meaning of the current 'global economic crisis'? What are its origins and implications? What *is* it? What, indeed, is 'the global economy', and how (and when) did we all decide that it was a reasonable and important thing to fret over? How do we see it, talk about it, make predictions about it, claim that it is healthy, or sick, or getting better, or has more dark days ahead? What connection does this talk and its associated modes of representation and knowing have to do with the lived experiences of people who appear to have been 'affected' by the 'crisis'? Whose crisis is it anyway?

The Capitalism Collaps-O-Meter and the indices displayed in association with it at the Beall Center for Art and Technology between June 9 and June 12, 2009 can be read as one step in the development of a set of quantitative techniques for representing, modelling, and interpreting economic relationships 'outside' the traditional concerns and conventions of academic economics. I have some formal training in mathematical modelling, in the shape of an undergraduate degree in applied mathematics, but almost no such training in academic economics. As such, I have some access to the language of economic analysis but little predisposition to accept its axiomatic

assumptions, approaches, frameworks, or tools. In this respect I am playing, in the development of this project, the role Claire Pentecost has named the 'public amateur', a figure who "consents to learn in public" [1]. For Pentecost, the practice of public amateurism "is a proposition of active social participation in which any nonspecialist is empowered to take the initiative to question something within a given discipline, acquire knowledge in a noninstitutionally sanctioned way, and assume the authority to interpret that knowledge, especially in regard to decisions that affect our lives" [1].

As of April 2009, the first-ever 'global financial crisis' is, on one reading, almost two years old, with substantial detrimental impacts continuing on parties far away from the centers of financial and political power. The speed with which the crisis has developed appears to have served as an opportunity for financial and political elites in industrialized economies to further concentrate financial and political power, and a number of protests of dubious efficacy have sprung up as this realization has circulated throughout industrialized societies. Despite the apparent short-term consolidation of financial and political power, efforts taken ostensibly to 'stabilize the economy' appear thus far to have failed. One reading of this failure suggests that the frameworks offered by academic macroeconomics that form the basis of these policy interventions are inadequate to the task of coherently analyzing the global political economy in the current moment. If the fundamental operating assumptions of leading academic macroeconomists and policymakers have proved intellectually bankrupt—and former Federal Reserve Chairman Alan Greenspan admitted as much in his testimony before the United States Congress on October 23, 2008, saying, "I made a mistake in presuming that the self-interests of organizations, specifically banks and others, were such that they were best capable of protecting their own

shareholders and their equity in the firms" [3]—then perhaps an argument can be made that it has become appropriate to examine these assumptions with great care, and to attempt to construct alternative models, interpretations, representations, and narratives about the functioning of 'the global [political] economy'. In the context of such dramatic epistemological collapse, as Pentecost writes of another case study, "self-experimentation becomes necessary because no one can trust the authorities or so-called experts":

> Our condition of reflexivity at this point is such that the amount of commentary or information on what we do is overwhelming not only us, but it's overwhelming the "experts." The dams around science's fantasy of objectivity are being breached. Science is more and more obviously driven by subjective interests. Even though technocratic knowledge still legitimizes a major portion of the spectrum of authority, our ability to rely on technoscientific pronouncements is crumbling under the weight of information and the light of examination. We have more tools for producing 'information' and fewer for making judgments; we have more networks for informing—press, publishing, publicity—so we have more opportunities for examination. The public, weary of conflicting authorities, constantly exposed to means of reporting on and reflecting on the claims of authorities, has to devise other means of producing information, i.e., their own experience, and also has to move further to self reliance, i.e., trusting their own observation, good "sense," and decision. [4]

In another formulation of the artist as amateur in the context of the biological sciences under neoliberalism, Pentecost enumerates a number of mechanisms by which science and the public are alienated from one another,

and a number of corresponding strategies that an artist can deploy in order to interrupt this alienation: "staging of scientific procedures in participatory theaters to provide experiences of the materiality of science; initiation across specialized knowledge fields enfranchises nonspecialists to author new narratives with a perspective on the real stakes involved; playing the amateur, the artist takes pains to find collaborators within scientific fields and/or consents to become a 'thief' of privatized knowledge in order to politicize or at least problematize this sequestering" [5]. While useful in the context of the biological sciences, these strategies appear to presume that the scientific knowledge being appropriated is valuable beyond its performative use in the construction of the discipline and the figure of the expert; that is, that it has some demonstrated explanatory and/or predictive power. While perhaps less frequently problematic in the context of the physical and biological sciences, whether this assumption holds in the context of academic economics is unclear. The role of the public 'amateur-economist' is less that of a translator or liberator of obviously valuable knowledge and more that of a critic or interrogator of dubious knowledge that has been used to make decisions affecting the lives of 'the public' at large.

In this particular project, I have brought to the role of 'public amateur' in academic economics some of the knowledge of a mathematically literate 'analyst'; following economic sociologist Michel Callon, I take "the task of the analyst" to be the "multipl[ication of] possible worlds" [6] (p. 15). The Capitalism Collaps-O-Meter acts as an instrument for effecting this multiplication by providing an expanded interpretation of what constitutes economically relevant data, facilitating the subjective reorganization and reinterpretation of that data, and furnishing a language for the construction of economic narratives and analyses beyond that of academic economics. In the

wake of the intellectual collapse of economic orthodoxy, the time seems ripe for analytic and linguistic experimentation.

But which 'public' is involved in this practice of ostensibly 'public' economic amateurism? I do not wish here to imply the existence of a smooth 'information space' or 'public sphere' into which interpretations, representations, methods, models, or taxonomies can be sent forth, to be impartially and definitively judged true or false, useful or unuseful, novel or trite, ideologically acceptable or 'problematic'. Rather, we can make use of Michael Warner's notion of a "public that comes into being only in relation to texts and their circulation"; which "*exists by virtue of being addressed*" [2] (p. 50; emphasis in the original). The 'experiments' in explicitly 'critical' mathematical modelling described here have been rendered to a very particular 'public'; namely, those individuals who happened to pass through the Beall Center for Art and Technology at the University of California, Irvine between June 9 and June 12, 2009, and later, readers of this document. Because of this numerically limited and temporally constrained circulatory scope, they should be thought of primarily as *experiments* rather than political actions; as Warner points out, "publics act historically according to the temporality of their circulation":

> The more punctual and abbreviated the circulation, and the more discourse indexes the punctuality of its own circulation, the closer a public stands to politics. At longer rhythms or more continuous flows, action becomes harder to imagine. This is the fate of academic publics, a fact very little understood when academics claim by intention or proclamation to be doing politics. [2] (p. 68)

Attributing political efficacy to these particular artifacts at this particular showing, then, would be somewhat suspect. I do wish, however, to explore

their political *implications*.

Chapter 2

Notes toward a critical mathematical modelling

Greenspan. I have found a flaw. I don't know how significant or permanent it is. But I have been very distressed by that fact.
Waxman. In other words, you found that your view of the world, your ideology, was not right, it was not working.
Greenspan. Absolutely, precisely. You know, that's precisely the reason I was shocked, because I have been going for 40 years or more with very considerable evidence that it was working exceptionally well.
—October 23, 2008

How are mathematical models used to generate knowledge and guide or justify action in the world? What would constitute a 'critical' practice of mathematical modelling, and why might such a practice be desirable? I will approach these questions by providing four accounts: first, an account of the modelling process as described by two practitioners in transportation modelling, accompanied by a simple but detailed example; second, an account of the way in which academic economics relies on activities and mechanisms 'outside' the substantive 'content' of the discipline in order to prepare 'natural experiments' that can form the basis of empirical work in the field, as analyzed by a political theorist; third, an account of the construction of the markets for financial derivatives and their grounding in the mathematical theory of derivatives pricing, as analyzed by a sociologist; and finally, an account of the failure of financial risk models and their role in the contemporary global

financial crisis (and a previous crisis), as told in five separate articles by four financial journalists and one economist. These accounts will serve to foreground some of the challenges of mathematical modelling as a knowledge production practice, and will animate a discussion of a first set of characteristics of a notional 'critical' mathematical modelling; these characteristics will focus on the question 'How can we model better?', and how the Capitalism Collaps-O-Meter stands in relation to these concerns.

There is, of course, a broader interpretation of 'criticality', at least as important as that flavor of critique which aims to revise disciplinary practices and formalisms in an attempt to solve better the technical problem under consideration. This is the strand of critique which, roughly understood, inquires about the social and political effects of the technical work: what should we model? to what purpose? who benefits? Put simply, this order of critique focuses less on the *how* of modelling and more on the *what*, *why*, and *for whom*. Although technical understanding of the details of particular models under analysis are central to this mode of critique, the critique itself is properly social, political, institutional, and/or economic; it is not necessarily technical in a mathematical sense, although the techniques used to effect this critique may include tools from almost any discipline—sociology, media studies, the plastic arts, quantitative modelling itself—the list goes on.

2.1 'Normal' mathematical modelling

Transportation analysts Juan de Dios Ortúzar and Luis Willumsen offer an eight-point portrayal of the planning processes within which mathematical models of transportation systems are constructed and used for 'decision support':

1. Formulation of the problem.

2. Collection of data.

3. Construction of an analytical model.

4. Generation of [possible] solutions.

5. Forecast[ing] the future values of the planning variables.

6. Testing the model and solution.

7. Evaluation of [possible] solutions.

8. Implementation of [selected] solutions. [7] (pp. 25-27)

I am particularly interested here in part (3), the construction of analytical models. Following Ortúzar and Willumsen, we can represent a model as a mapping

$$\boldsymbol{Y} = f(\boldsymbol{X}, \boldsymbol{\theta})$$

where the $\boldsymbol{X}$ are referred to as "variables" and the $\boldsymbol{\theta}$ as "parameters." In this case by "variables" ($\boldsymbol{X}$) what is meant is in fact *explanatory* or independent variables: these may include demographic data like income, age, and gender; data that describe the state of the transportation system (width and quality of roads, traffic signals, bus routes and frequencies, etc.); data describing the "activity system" or network of destinations that users access by means of the transportation system; and so on. The $\boldsymbol{Y}$ are the dependent variables whose values are predicted by the model and constitute its 'output.' Note that very often there will be some variables which appear in both $\boldsymbol{X}$ ('input') and $\boldsymbol{Y}$ ('output'); these variables are said to be determined 'endogenously'—that is, within the model (as opposed to being determined 'exogenously,' or 'given').

Obtaining the values of these variables generally involves solving a set of simultaneous equations.

To develop a very elementary familiarity of the techniques employed in this way, I will describe a toy problem and solution.[1] Researchers familiar with approaches from neoclassical economics will find this approach unsurprising, if somewhat naïve.

Suppose we have an origin "O" and a destination "D" joined by a single road, along which travel time t in minutes varies linearly with the number of vehicles v on the road at any particular time according to

$$t = 10 + \frac{1}{60}v$$

That is, if there are no cars on the road, it takes 10 minutes to travel from O to D; for the first car, it takes 10 minutes and 1 second; and for every additional car on the road it takes another $\frac{1}{60}$ of a minute, or one additional second (see Fig. 1). This is referred to as the "performance function" of the road under study. Additionally, we can define a "demand function," which predicts the number of vehicles on the road v (i.e., the number of drivers who wish to travel from O to D, in vehicles per hour or 'vph') as a linearly declining function of travel time t:

$$v = 5000 - 100t$$

That is, if travel between O and D were instantaneous (i.e, if $t = 0$), 5000 drivers would make the trip; for each additional minute, 100 would-be drivers decide it's not worth the time and abstain. At the so-called "base travel time" of 10 minutes, 4000 drivers *would like* to make the trip. But of course, things are not so easy; if 4000 drivers take to the road, the travel time increases far in

[1] I thank Michael McNally of the Institute for Transportation Studies at UC Irvine for this example, although I have altered the numbers slightly.

excess of 10 minutes, and more drivers abstain. This process is called "equilibration," and to obtain the model "equilibrium"—that is, the system state predicted by the model—we solve the demand and performance functions simultaneously, for example by substitution:

$$t = 10 + \frac{1}{60}v$$

$$t = 10 + \frac{1}{60}(5000 - 100t)$$

$$t = \frac{280}{3} - \frac{100}{60}t$$

$$\frac{8}{3}t = \frac{280}{3}$$

$$t = 35$$

$$v = 5000 - 100(35) = 1500$$

and we see that the model predicts an equilibrium volume of 1500 vph and an equilibrium travel time of 35 minutes.

We can also see that this model can be abstracted further. By abstaining from specifying the values of the parameters, we can write something like

$$t = \alpha_1 + \beta_1 v$$

$$v = \alpha_2 + \beta_2 t$$

where by writing α and β in front of the variables we are saying essentially, "there is probably some relationship here, but we don't know what it is yet." That is, the "parameters" α and β are not yet defined, and must be estimated or calibrated—and then validated—before the model can be used to predict future conditions.

Ortúzar and Willumsen describe these practices as follows:

> Calibrating a model requires choosing its parameters, assumed to have a non-null value, in order to optimise one or more goodness-of-fit measures which are a function of the observed data... Estimation involves finding the values of the parameters which make the observed data more likely under the model specification; in this case one or more parameters can be judged non-significant and left out of the model. Estimation also considers the possibility of examining empirically certain specification issues; for example, structural and/or functional form parameters may be estimated. ...in essence both procedures [calibration and estimation] are the same because the way to decide which parameter values are better is by examining certain previously defined goodness-of-fit measures. [7] (p. 18)

To illustrate this practice we can continue our example from above. Suppose now that we are interested in forecasting performance of an actual road, that we have a mechanism in place for collecting the values of these variables (traffic volume v and travel time t; for now we will refrain from speculating on the nature and implications of these mechanisms), and that we collect the dataset indicated below.

v (vph)	t (min)
0	15
100	16
1000	22

Clearly these three points do not fit a straight line. Our model is linear, however, so we perform a 'regression' analysis to select parameters whose

values cause our linear model to most closely approximate the data we have collected. Presenting and commenting on the various goodness-of-fit measures that can be used in regression analyses is beyond the scope of this paper, but we can select one (a typical one might be "Ordinary Least Squares") and, assuming that with this dataset we consider v the independent variable and t the dependent, obtain values for α_1 and β_1. We can repeat this process for α_2 and β_2 to 'identify'—that is, specify completely—our model for $v(t)$.

To 'validate' the model after it has been calibrated (or after its parameters have been estimated, whichever you prefer) "requires comparing the model predictions with information not used during the process of model estimation"—that is, collecting additional data in order to verify the model's predictive power. I will refrain from presenting the formal methods employed in this process but will illustrate this process with our model for $t(v)$ from above and two additional example datasets:

Dataset 1.

v (vph)	t (min)
1	15.02
110	16.1
1100	22.1

Dataset 2.

v (vph)	t (min)
1	22
105	10
1500	15

If, after calibrating our model, we were to collect Dataset 1 on the road for which our model is supposed to offer meaningful predictions, we might be

reassured; if, on the other hand, we were to collect Dataset 2 on that road, we might be skeptical of our model's ability to offer any such predictions.

Now suppose we have two roads, and drivers are expected to choose between them. To determine the equilibrium distribution of vehicles (and thence the equilibrium travel times on the roads), we assume that users have knowledge of the travel times on each road and will select the road with the shortest travel time. This is called assuming 'Nash equilibrium' or 'user equilibrium,' and it rests on three assumptions:

1. Users prefer shorter trips over longer trips (that is, they obtain greater *utility* from shorter trips);

2. users will act to maximize their own utility at the expense of others', and therefore will select the route with the shortest travel time even if it raises the travel time for the other users on the route; and

3. users have perfect (i.e., complete) information of the system at all times; in this case, they know the exact travel times along both routes.

Generally speaking, these assumptions describe the canonical 'rational' user. In more complex scenarios where users make additional choices (for example, selecting a transportation 'mode'—that is, selecting between driving, walking, cycling, taking the bus, and so on) we assume that users have 'rational preferences,' meaning that the user makes a choice from a set which is *exhaustive*—that is, it covers all possible choices—*mutually exclusive*—each user makes only one choice at a time—and *finite* [8] (p. 15). Further, we assume that users' preferences over this choice set are *complete*—that is, we can meaningfully compare any two choices—*transitive*—if a user prefers A to B and B to C, the user must prefer A to C—and *continuous*.[2] These six

[2]Continuity is an assumption made for technical convenience which we will not be too concerned about here; I have included it for completeness.

assumptions allow for the construction of a meaningful 'utility function,' which maps choices—quantities of goods consumed, routes or modes chosen, parking spots selected, and so forth—to levels of 'utility,' a constructed measure of well-being. We assume that all users will make use of their information (assumed to be complete and up-to-date at all times) to make choices that maximize their utility. The preference axioms (completeness, transitivity, and continuity) are widely acknowledged not to hold in all circumstances—there is a substantial economic literature exploring the situations in which transitivity does and does not hold, for example—but many contemporary transportation modelling approaches still rely on them.

2.2 The work outside the work

The political theorist Timothy Mitchell tells the story of an economic 'natural experiment' conducted in Peru and analyzed by development economist Erica Field [9], [10] which examines the hypothesis that the transformation of "the country's informal urban neighborhoods into legal, state-regulated housing"—i.e., granting property title to squatters, transforming them into legally recognized property owners—"offered a simple and inexpensive means to end widespread poverty" [11] (p. 299): newly-legalized landholders would be able to use their property titles as collateral for loans, which they would use to start small businesses. Ultimately the hypothesis was not supported—"there is no evidence," write Field and Torero [9], "that titles increase the likelihood of receiving credit from private sector banks"—but the finding reports an unexpected secondary finding: Fields' "estimates suggest that titling results in a substantial increase in labor hours, a shift in supply away from work at home to work in the outside market and substitution of

adult for child labor" [10]. That is, "those who became formal property owners...began to work harder". Mitchell summarizes the findings articulated by Field:

> Obtaining title to their property seemed to increase the average number of hours that members of a household worked by 17 per cent. The data suggested that over time, as the effect of titling intensified, the total number of hours worked might increase by 40 per cent. There was also a redistribution of labor from work within the home to employment outside, and from children to adults. Property titling was associated with a 47 per cent decrease in the number of hours worked inside the house, and a 28 per cent reduction in the use of child labor.
>
> To explain these findings, ... Field hypothesized that acquiring formal title freed members of the household to spend more time outside the home, based upon the intuition that in the absence of a formal title people had to stay home to protect their property from being seized by others. A further intuition suggested that adults had a comparative advantage over children in defending the home, so in the absence of secure property rights children were more likely to be sent out to work. Once the property was secured with a formal title, children could stay home and adults could take over children's jobs outside the household. [11] (pp. 300-301), citing [10]

Mitchell is skeptical about the conclusions of the study. "The research experiment," he explains, "was made possible by the political experiment that it studied. The agencies and arrangements that framed the property rights experiment framed the conclusions reached in the experiment on the

experiment" [11] (p. 301). Put another way, the conclusions of the research experiment were shaped by factors not explicitly considered or 'scoped in'. Some of these are attributes of the neighborhoods under study; others are attributes of the broader intellectual environment in which the research was conducted, or of the researchers themselves. "The paper's argument," writes Mitchell, "depends on the assumption that the informal neighborhoods of different Peruvian cities are similar to one another and that the sequence in which the titling program entered different cities and neighborhoods was random":

> The staggered implementation that made a natural experiment possible must be unrelated to any local differences that might influence the extent to which people in different neighborhoods work outside the home. The paper claims to resolve the possibility of non-random city timing by including city-level fixed effects in the regression estimates. However, more than half the survey neighborhoods already reached by the titling program were located in one city, Lima. Different neighborhoods of the capital were reached by the program at different times. If there were significant reasons for introducing the program in some neighborhoods of Lima before others, and for later extending it to certain neighborhoods of certain provincial cities before others, and significant differences among these neighborhoods, this might offer a more reasonable explanation for the outcome of the experiment. Simply allowing for city-level fixed effects would not be able to capture the possible interaction among this range of differences.
>
> The experiment was unable to test whether differences among neighborhoods were affecting the rate of employment outside the

> home. The author claims such differences can be ignored, on the grounds that eight district-level poverty indicators (rates of chronic malnutrition, illiteracy, fraction of school-aged children not in school, residential crowding, adequacy of roofing, and the proportion of the population without access to water, sewerage, and electricity) were similar for program and non-program neighborhoods. [... However,] indirect indicators of poverty levels...may be unable to capture major differences between different kinds of neighborhoods with different patterns of employment. [... Further,] the World Bank also says that the order [in which the program was launched in different neighborhoods and cities] was not random, but was based on "ease of entry" to the neighborhood. The bank's Peruvian program office reported that the order depended on "geographical situation, feasibility to become regularized, dwellers' requests, existing legal and technical documents, and linkages with other institutions involved in the existing obstacles." [11] (pp. 310-311)

In short, there were significant factors in the environment under study that were not modelled explicitly ("inputs" in the lexicon of Ortúzar and Willumsen discussed above) but that could entirely plausibly have had a substantial effect on the outcomes observed ("outputs"). Arguments put forth by the study authors that the effects of these factors were neglible are, at least according to Mitchell, unconvincing.

There are additional factors that Mitchell argues may have affected the *interpretation* of the study data, and thus the outcomes of the study as presented by the authors. These factors are not attributes of the environment under study (i.e., the informal urban neighborhoods in Peru) but rather

attributes of the authors themselves, and of the intellectual climate in which they operated:

> No plausible evidence is offered to support the author's intuition that households without a formal ownership document have to keep people at home to defend the property from being seized by others, or that gaining this document suddenly removes the alleged need for self-defense. The intuition is backed only by an anecdote from a World Bank report and the writings of Hernando de Soto [the neoliberal development economist on whose intellectual and political work the "natural experiment" analyzed in the study is based]. Evidence available in the same World Bank documents suggests a contrary view: Peru's informal urban communities are described as having very strong collective organizations and a great variety of neighborhood mutual-help arrangements. Typically a squatter neighborhood was formed by a single village, whose members would plan their relocation collectively in advance, allocate each family a building plot, and reproduce the communal associations of the village in the new location. None of this indicates a situation in which people feel so threatened they must stay home to guard their individual properties. (The World Bank also reports that titling programs tend to weaken these neighborhood associations.) Evidence from other studies suggests that the security of informal households depends on a wide range of factors and is not necessarily dependent on possession of formal title.
>
> What makes the intuition plausible is that it resonates with the work of neo-institutionalist economists like Douglas North and

> neoliberal theorists of development like Peter Bauer. It assumes that a world without formal property rights is anarchic, and that once the proper rules are in place a natural spirit of self-interested endeavor will be set free. *It derives its plausibility more from the reader's familiarity with certain texts in economics than from any knowledge of informal communities.* [11] (pp. 309-310; emphasis added)

If the objective of mathematical modelling in this 'natural experiment' is to make inferences about the world from data, Mitchell's analysis foregrounds two challenges for this project. First, it is difficult to know what factors are relevant 'inputs' to the phenomena ('outputs') under study; most social phenomena, however, are too complex to admit comprehensive observation (not to mention quantitative representation in a model) of all factors that could conceivably be relevant. This places upon the analyst the burden of deciding which factors to observe and which to ignore, which becomes problematic in a research culture in which ambivalent results (e.g., 'I think we need to go back and collect data on factors X, Y, and Z which were ignored in the previous study') are not rewarded as highly as decisive ones. Such a culture can incentivize dubious arguments about the 'negligibility' of factors which were omitted from the data collection process not because the analyst was convinced of their negligibility but simply because it would have been too onerous to collect them, because the analyst did not think to collect them until later, the requisite labor or equipment was unavailable at the time, or any number of other completely reasonable—but 'merely practical'—reasons. Second, it is difficult to understand the probable or even possible effects of one's own intellectual predilections or those of one's discipline on the preferential interpretation of data, or on the production of knowledge

generally. If, as the political psychologist Shawn Rosenberg suggests, "a discipline is a group of people who have all agreed to remain ignorant of the same things" [12], disciplinary histories can be reasonably expected to shape more and less acceptable interpretations of data, processes, and phenomena. The challenge to the critical modeller is to model the extent to which such predilections affect not just the interpretation of field data and model results, but also the ways in which experiments are constructed, surveys designed, classifications articulated, and so on; in short, to examine carefully the various ways in which disciplinary conventions and histories order the world, thereby shaping what is possible, epistemologically speaking, and what is not. This second challenge can be read as the challenge of rejecting the traditional notion of objectivity within positivist science, and operationalizing this rejection within one's modelling practice. The cyberneticist Heinz von Foerster [13] characterizes this particular flavor of "objectivity" in the following fashion:

> "The properties of the observer shall not enter the description of his observations."

"But," he asks,

> how would it be possible to make a description in the first place if not the observer were to have properties that allows for a description to be made? Hence, I submit in all modesty, the claim for objectivity is non sense! One might be tempted to negate "objectivity" and stipulate now "subjectivity". But, ladies and gentlemen, please remember that if a nonsensical proposition is negated, the result is again a nonsensical proposition. However, the nonsensicality of these propositions either in the affirmative or in their negation cannot be seen in the conceptual framework in

> which these propositions have been uttered. If this is the state of affairs, what can be done? We have to ask a new question:
>
> "What are the properties of an observer?"

Donna Haraway does not abandon the term "objectivity", but works to develop a new interpretation of it. This feminist objectivity addresses exactly this question:

> I would like a doctrine of embodied objectivity that accommodates paradoxical and critical feminist science projects: Feminist objectivity means quite simply *situated knowledges*. ...[the] view of infinite vision is an illusion, a god trick. I would like to suggest how our insisting metaphorically on the particularity and embodiment of all vision (although not necessarily organic embodiment and including technological mediation), and not giving into the tempting myths of vision as a route to disembodiment and second-birthing allows us to construct a usable, but not an innocent, doctrine of objectivity. ...objectivity turns out to be about particular and specific embodiment and definitely not about the false vision promising transcendence of all limits and responsibilities. The moral is simple: only partial perspective promises objective vision. All Western cultural narratives about objectivity are allegories of the ideologies governing the relations of what we call mind and body, distance and responsibility. Feminist objectivity is about limited location and situated knowledge, not about transcendence and splitting of subject and object. It allows us to become answerable for what we learn how to see. [14] (pp. 582-583)

That is: all 'seeing' (and 'understanding'), and by extension all modelling, is necessarily grounded and located in a particular body (or set of bodies, or physical assemblage), a particular context, and a particular set of assumptions, positions, and interests, and is therefore limited. No technique exists that allows for omniscient, disembodied, 'disinterested' analysis; no position exists 'outside' the 'system' under study, because the apparatus that allows for observation and analysis to occur at all necessarily joins 'observer' to 'system'. The act of observation itself renders boundaries between 'observer' and 'observed' analytically suspect. The corollary is that no model or practice of critical mathematical modelling can ever account for iteself completely, and therefore that every act of modelling (which might begin from 'observations') and every act of prediction or explanation (based on a model) is necessarily preceded by an act of interpretation, in which some 'facts' are decided to be more important than others, some trends more relevant than others, and some questions more interesting than others. Thus critical mathematical modelling—with its origins in positivist, instrumentalist knowledge production and its association with the fetishization of predictive power and techniques and technologies for abstract, disembodied representation—finds itself in a sort of epistemological badlands between 'positivist' and 'interpretivist' and even between 'quantitative' and 'qualitative' ways of knowing, seeing, and representing.

In some ways, ironically enough, the researcher who refuses to acknowledge the second challenge—who refuses to admit their particularity, specificity, locatedness, embodiment, intellectual predilections, disciplinarily-rooted assumptions, and so on—opens themselves to criticism stemming from the first challenge (that is, the impossibility of knowing all of the relevant variables and parameters). By rejecting omnipotent, omnipresent,

omnianalytic interpretations of objectivity, one allows oneself the luxury of non-omniscience: "I didn't think of that" (or "we didn't have enough people" or "we didn't have the equipment") becomes an acceptable, legitimate response to "Why isn't X in your model?" rather than a career-destroying admission of failure. As a result, readers get models that have the luxury of being truthful about their own limitations.

2.3 The model that makes the market

Economic sociologist Donald MacKenzie tells a story about the role of the Black-Scholes-Merton model for pricing options in the creation and standardization of the market for those options [15]. The title of his chapter is "Is economics performative?" and exploring this question is his primary concern. "To claim that economics is performative," he writes,

> is to argue that it *does* things, rather than simply describing (with greater or lesser degrees of accuracy) an external reality that is not affected by economics. But *what* does economics do, and what are the effects of it doing what it does? [15] (p. 54)

I will not dwell here on the various flavors of performativity discussed by MacKenzie; to provide a short account of the deployment of the Black-Scholes option pricing model and its effects on economic actors is sufficient to the task of articulating the challenge for modellers. I will quote MacKenzie at length:

> An option is a contract that gives the right, but does not impose the obligation, to buy (or, in an alternative form of the contract, to sell) a set quantity of a particular asset at a set price on, or up to, a given future date. ...The asset in question is classically a block of stock

(typically 100 shares), but options can also be written on many other assets: gold, oil, wheat, and other physical commodities; stock indexes and other more abstract assets; and so on.

A central question for the theory of options is how the cost of options is established. Intuition suggests certain parameters that can be expected to play a role in determining the cost... Unaided intuition is, however, not sufficient to go beyond [a] list [of parameters] to a formula for the option price. Nor is practical experience decisive in this respect. Options have been traded since at least the seventeenth century, and market practitioners developed rules of thumb for pricing options, but those rules of thumb did not add up to a precise or comprehensive theory.

...By the start of the 1970s, ... work by financial economists Fisher Black and Myron Scholes, with key additional input from their colleague Robert C. Merton, produced what has become the canonical theory of options. ...an old technology formed the key mediator between the model's mathematics and the shouting, sweating, gesticulating, jostling human bodies on the trading floors: paper. Away from the hubbub, computers were used to generate Black-Scholes prices. Those prices were reproduced on sets of paper sheets which floor traders could carry around...

How were Black's sheets [Fisher Black himself provided the most popular printouts] and similar option pricing services used? They could, of course, simply be used to set option prices. In April 1976, options trading began on the Pacific Stock Exchange in San Francisco, and financial economist Mark Rubinstein became a trader there. He found his fellow traders on the new exchange

> initially heavily reliant on Black's sheets: "I walked up [to the most active option trading 'crowd'] and looked at the screen [of market prices] and at the sheet and it was identical. I said to myself, 'academics have triumphed."' [15] (pp. 56-57; 62)

MacKenzie provides an exhaustive analysis of the many ways in which the Black-Scholes option pricing model was 'performative': in addition to allowing issuers to set option prices, the model also provided a way for traders to minimize risk while taking positions in the options market; it helped to legitimate options trading, which prior to the deployment of the model had been reviled and equated with gambling; it furnished traders with an entirely new language for guiding and understanding the options market and their own actions in it; and, perhaps most importantly for MacKenzie's thesis, it demonstrably altered trading practice to cause the price patterns of stock options to conform closely to what it said they should be. The nuances and distinctions between these effects and the taxonomy of performativities that MacKenzie develops around them are not central to my thesis; I wish only to foreground that models, even ones which ostensibly aim only to represent a reality to which they are external, retain a capacity to act (and in any number of ways, through any number of artifacts and processes) on that reality and to alter it. This is the third challenge for the critical modeller: the challenge of anticipating the ways in which the interpretation of results is complicated by the non-separation (indeed non-separability) of the model and the ostensibly independent world it represents. This separability provides a further complication for a modeller interested not only in technical 'criticality' (i.e., in asking 'how can we better model?') but in the second-order criticality that asks about the social, political, and generally 'ethical' ramifications of a model. This will be discussed in a later section.

2.4 Past performance is not an indication of future results

In 1994, the former head of bond trading at Salomon Brothers investment bank, two Nobel-winning economists, and a bevy of other economists with PhDs from the Massachusetts Institute of Technology founded Long-Term Capital Management (LTCM), a hedge fund which developed and implemented trading strategies based on quantitative models of the behavior of different asset prices. The "fund nearly tripled the money of its wealthy investors between its inception in March, 1994, and the end of 1997," write Peter Coy and Suzanne Woolley in the September 1998 issue of *BusinessWeek* [16].

> Its sophisticated arbitrage strategy was avowedly 'market-neutral'—designed to make money whether prices were rising or falling. Indeed, until last spring its net asset value never fell more than 3% in a single month.
>
> Then came...August [1998]. Long-Term Capital's...portfolio's value fell 44%, giving it a year-to-date decline of 52%...a loss of almost $2 billion.
>
> ...Nearly every major investment house and bank in the U.S. and abroad has a group of highly paid [quantitative traders] in its proprietary trading department trying to beat the market with complex, computer-aided trading strategies. In an announcement on Sept. 2, [1998,] Salomon Smith Barney Holdings disclosed that it had realized $300 million in losses from fixed income and global arbitrage... Then, on Sept. 9, Merrill Lynch & Co. announced that it had lost $135 million from trading and said that the losses had hurt

its own stock price.

Financial journalist Roger Lowenstein, in his comparison of the current crisis (written in September 2008) with the 1998 collapse—"through the lens of today's more widespread failure," he writes, "the Long-Term Capital collapse looks like a small dress rehearsal...but at the time, it sent tremors of fear through the corridors of Wall Street" [17]—offers a rough but useful conceptual sketch of what went wrong, which I will quote at length:

> How could the fund have been so far off? ..."risk management" calculations were and are a central tenet of modern finance. "Risk" is said to be a function of potential market movement, based on historical market data. But this conceit is false, since history is at best an imprecise guide.
>
> Risk—say, in a card game—can be quantified, but financial markets are subject to uncertainty, which is far less precise. We can calculate that the odds of drawing the queen of spades are 1 in 52, because we know that each deck offers 52 choices. But the number of historical possibilities [in financial markets] keeps changing.
>
> *Before 1929, a computer would have calculated very slim odds of a Great Depression; after it, considerably greater odds*. Just so, before August 1998, Russia had never defaulted on its debt [this default being the widely-accepted proximate cause of the huge losses incurred by LTCM and other quantitatively-oriented funds]—or not since 1917, at any rate. When it did, credit markets behaved in ways that Long-Term didn't predict and wasn't prepared for.
>
> This was the same mistake that scores of lenders would make in the housing industry. The United States had never suffered a

nationwide contraction in housing prices; they assumed that the pattern would hold.

Modern finance is an antiseptic discipline; it eschews anecdotes and examples, which are messy and possibly misleading—but nonetheless real. It favors abstraction, which is perfect but theoretical. Rather than evaluate financial assets case by case, financial models rely on the notion of randomness, which has huge implications for diversification. It means two investments are safer than one, three safer than two.

The theory of option pricing, the Black-Scholes formula, is the cornerstone of modern finance and was devised by two Long-Term Capital partners, Robert C. Merton and Myron S. Scholes, along with one other scholar [Fisher Black]. It is based on the idea that each new price is random, like a coin flip.

Long-Term Capital's partners were shocked that their trades, spanning multiple asset classes, crashed in unison. But markets aren't so random. In times of stress, the correlations rise. People in a panic sell stocks—all stocks. Lenders who are under pressure tighten credit to all.

And Long-Term Capital's investments were far more correlated than it realized. In different markets, it made essentially the same bet: that risk premiums—the amount lenders charge for riskier assets—would fall. Was it so surprising that when Russia defaulted, risk premiums everywhere rose?

More recently, housing lenders—and the rating agencies who put triple-A seals on mortgage securities—similarly misjudged the

> correlations. The housing market of California was said to be distinct from Florida's; Arizona's was not like Michigan's. And though one subprime holder might default, the odds that three or six would default were exponentially less. Randomness ensured (or so it was believed) a diverse performance; diversity guaranteed safety.
>
> [LTCM]'s partners likened their disaster to a "100-year flood"—a freak event like [Hurricane] Katrina or the Chicago Cubs winning the World Series... But their strategies would have lost big money this year, too.
>
> ...If 100-year floods visit markets every decade or so, it is because our knowledge of the cards in history's deck keeps expanding. (emphasis added) [17]

Of course, Lowenstein is not implying that any computer is doing calculation on its own; rather, we should say that *the more widely used models*, which rely exclusively on historical data, would have calculated slim odds of a Great Depression before 1929, and substantially greater odds of one after. This conundrum brings the fourth and final challenge of critical mathematical modelling into view: generally put, this is the challenge of managing data. As is well known in computing, "the quality of the output [of a model or computer program] is a function of the quality of the input"; this is expressed by the acronym and aphorism "GIGO": "put **g**arbage **i**n and you get **g**arbage **o**ut" [18]. (The Jargon File offers the additional interpretation "Garbage In, Gospel Out," explaining, "this more recent expansion is a sardonic comment on the tendency human beings have to put excessive trust in 'computerized' data" [19].) Although simple, it could be argued that the operators of the risk models whose failure was the proximate cause of the current financial crisis

ignored or otherwise failed to observe the GIGO principle; that is, they put garbage in (wittingly or unwittingly) and expected non-garbage out.

Lowenstein's account highlights the primary dimension along which the principle was violated: time. Analysts fed risk models historical price data, expecting inferences made from that data to hold true in the future, even under changed market conditions. What assumptions were being made implicitly? Even in cases where the implicit assumptions were understood, the social, political, economic, and/or financial mechanisms by which they were expected to hold true were hazy at best. Niels Bohr is said once to have noted that "prediction is very difficult, especially about the future." The distinction is important here; although modellers often wish to make predictions about the future, the only data they have with which to make such predictions are about the past.

If the primary dimension along which the GIGO principle was violated in the context of models of financial risk is foregrounded by the question "data from when?", an additional such violation occurred when analysts asked themselves the question "data about what?" This complication is highlighted in an account of the current financial crisis by Felix Salmon, published in February 2009 in *Wired Magazine*, entitled "Recipe for disaster: the formula that killed Wall Street" [20]:

> Bond investors also invest in pools of hundreds or even thousands of mortgages. ...But mortgage pools are messier than most bonds. There's no guaranteed interest rate, since the amount of money homeowners collectively pay back every month is a function of how many have refinanced and how many have defaulted. There's certainly no fixed maturity date: Money shows up in irregular chunks as people pay down their mortgages at unpredictable

times—for instance, when they decide to sell their house. And most problematic, there's no easy way to assign a single probability to the chance of default.

Wall Street solved many of these problems through a process called tranching, which divides a pool and allows for the creation of safe bonds with a risk-free triple-A credit rating. Investors in the first tranche, or slice, are first in line to be paid off. Those next in line might get only a double-A credit rating on their tranche of bonds but will be able to charge a higher interest rate for bearing the slightly higher chance of default. And so on.

The reason that ratings agencies and investors felt so safe with the triple-A tranches was that they believed there was no way hundreds of homeowners would all default on their loans at the same time. One person might lose his job, another might fall ill. But those are individual calamities that don't affect the mortgage pool much as a whole: Everybody else is still making their payments on time.

But not all calamities are individual, and tranching still hadn't solved all the problems of mortgage-pool risk. Some things, like falling house prices, affect a large number of people at once. If home values in your neighborhood decline and you lose some of your equity, there's a good chance your neighbors will lose theirs as well. If, as a result, you default on your mortgage, there's a higher probability they will default, too. That's called correlation—the degree to which one variable moves in line with another—and measuring it is an important part of determining how risky mortgage bonds are.

...Using some relatively simple math...[analyst David X. Li, while working at JPMorgan Chase in 2000] came up with an ingenious way to model default correlation without even looking at historical default data. Instead, he used market data about the prices of instruments known as credit default swaps [insurance against bond borrowers defaulting].

When the price of a credit default swap goes up, that indicates that default risk has risen. Li's breakthrough was that instead of waiting to assemble enough historical data about actual defaults, which are rare in the real world, he used historical prices from the CDS market. It's hard to build a historical model to predict [borrower] Alice's or [borrower] Britney's behavior, but anybody could see whether the price of credit default swaps on Britney tended to move in the same direction as that on Alice. If it did, then there was a strong correlation between Alice's and Britney's default risks, as priced by the market. Li wrote a model that used price rather than real-world default data as a shortcut (making an implicit assumption that financial markets in general, and CDS markets in particular, can price default risk correctly).

...The damage was foreseeable and, in fact, foreseen. In 1998, before Li had even invented his copula function, Paul Wilmott wrote that "the correlations between financial quantities are notoriously unstable." Wilmott, a quantitative-finance consultant and lecturer, argued that no theory should be built on such unpredictable parameters. And he wasn't alone. During the boom years, everybody could reel off reasons why the Gaussian copula function wasn't perfect. Li's approach made no allowance for

> unpredictability: It assumed that correlation was a constant rather than something mercurial. Investment banks would regularly phone [Stanford University professor of finance Darrell] Duffie and ask him to come in and talk to them about exactly what Li's copula was. Every time, he would warn them that it was not suitable for use in risk management or valuation.
>
> In hindsight, ignoring those warnings looks foolhardy. But at the time, it was easy. Banks dismissed them, partly because the managers empowered to apply the brakes didn't understand the arguments between various arms of the quant universe. Besides, they were making too much money to stop. [20]

So the analysts who deployed the Gaussian copula to price their collateralized debt obligations fell prey to at least three of the challenges of critical mathematical modelling. They were tripped up by the fourth challenge (the challenge of data) when they failed to imagine that the possibility space for future behavior might be different than that of past behavior, and deemed their historical data sufficient for predicting future performance; and again when they chose to assume that the market for credit default swaps would accurately indicate both individual probabilities of default and default correlations. As for the third challenge (the challenge of performativity): almost single-handedly, Li's application of the Gaussian copula to the pricing of collateralized debt obligations transformed the market:

> The effect on the securitization market was electric. Armed with Li's formula, Wall Street's quants saw a new world of possibilities. And the first thing they did was start creating a huge number of brand-new triple-A securities. Using Li's copula approach meant

> that ratings agencies like Moody's—or anybody wanting to model the risk of a tranche—no longer needed to puzzle over the underlying securities. All they needed was that correlation number, and out would come a rating telling them how safe or risky the tranche was.
>
> As a result, just about anything could be bundled and turned into a triple-A bond—corporate bonds, bank loans, mortgage-backed securities, whatever you liked. The consequent pools were often known as collateralized debt obligations, or CDOs. You could tranche that pool and create a triple-A security even if none of the components were themselves triple-A. You could even take lower-rated tranches of other CDOs, put them in a pool, and tranche them—an instrument known as a CDO-squared, which at that point was so far removed from any actual underlying bond or loan or mortgage that no one really had a clue what it included. But it didn't matter. All you needed was Li's copula function.
>
> The [credit default swap] and [collateralized debt obligation] markets grew together, feeding on each other. At the end of 2001, there was $920 billion in credit default swaps outstanding. By the end of 2007, that number had skyrocketed to more than $62 trillion. The CDO market, which stood at $275 billion in 2000, grew to $4.7 trillion by 2006. [20]

Finally, they were blindsided by the second challenge (the challenge of reflexivity); in this case, the challenge was particularly great, as anyone seriously confronting the inadequacy of the model for the task to which it was being repeatedly applied would have risked substantial damage to their paycheck. Analysts offering critiques of the model appear, at least in Salmon's

account, to have been in institutional contexts in which the model was not being applied; either in firms making use of different strategies, or, as in Duffie's case, in a university [20].

2.5 A broader criticality

But these four challenges of critical mathematical modelling form the grounds for nothing more than a *technical* critique; that is, they furnish language ('variables', 'reflexivity', 'performativity', 'data') for answering 'how can we better model?' without obviously touching the questions of what we are modelling, for whose benefit, and to what end. Thus the four challenges of the critical modeller articulated thus far align well with the 'critical technical practice' articulated by the computer-scientist-turned-social-scientist Philip Agre in his seminal essay "Toward a Critical Technical Practice: Lessons Learned in Trying to Reform AI" (where "AI" indicates the 'discipline' of artificial intelligence) [21] in that they bring concepts and practices from outside the discipline proper to bear in evaluating the discipline's success in accomplishing its own goals, and use those concepts and practices to critique and negotiate with existing concepts, assumptions, and practices within the field in the hope of bringing about useful and substantive change. In theory, such change might conceivably extend to a reconfiguration of the criteria by which success is defined within the field; in my reading of Agre, while not excluded, such a reconfiguration does not appear foregrounded. To the extent that we limit ourselves to essentially technical critiques of technical practice—i.e., critiques of the form "technical practice or method X is inferior to practice or method Y in accomplishing agreed-upon technical objective Z"—we are vulnerable to the accusation levelled, for example, by theorists

Fred Moten and Stefano Harney when they claim that "critical academics are the professionals par excellence" [22] (p. 111). That is, the purpose of this kind of (technical) critique is to evaluate and refine approaches to agreed-upon "technical difficult[ies]" [21]; to provide a comprehensive analysis, or a critique on grounds other than those customary within a particular discipline or community of practice, one must step outside the technical language of discipline (or, for Moten and Harney, 'profession'):

> ...communities of composition teachers, mentorless graduate students, adjunct Marxist historians, out or queer management professors, state college ethnic studies departments, closed-down film programs, visa-expired Yemeni student newspaper editors, historically black college sociologists, and feminist engineers...what will the university say of them? It will say they are unprofessional. This is not an arbitrary charge. *It is the charge against the more than professional.* [22] (p. 104; emphasis added)

What is 'wrong' with these figures? They are not, one might suggest, *objective* in the traditional sense of the term: how can one be a 'good engineer' when one is also a feminist, or a 'good management professor' when one is also queer? (For that matter, can one be a 'good' sociologist when one is also a Republican? A 'good' paleobiologist and also a Christian?) Does not one run the risk of (assumed) political commitments 'polluting' the work? Don't engineers and management researchers wear buttoned-down shirts and suits, not skirts? Aren't crosses forbidden in the biology department? Disciplinary performances of political neutrality or scientific objectivity aside, I suggest that, if we take 'good' to mean 'ethically responsible', one cannot be a 'good' engineer, or management professor, or anything else, without apprehending and engaging the political dimensions of one's 'technical' work—without, that

is, *having a politics*. Of course, 'good' does not traditionally mean 'ethically responsible' in the technical disciplines.

How does one develop a language and, centrally, a practice of critical engagement with disciplinary discourses in which it is acceptable and 'productive' to critique the activities (and objectives) of disciplinary practitioners, and of 'the discipline' itself? What constitutes 'productive' engagement at all if there is no agreed-upon definition of 'good' or 'useful' practice; indeed, if the definition of 'good practice' is exactly what is being contested in the process of critique? We can begin to get a handle on these questions with two examples of disciplinary critique in which the practices and/or relations under critique are not explicitly technical but rather concern the relation of the discipline to institutional formations 'outside' itself.

The first, offered by the Institute for Applied Autonomy (IAA) under the title "Engaging Ambivalence: Interventions in Engineering Culture" [23], begins with a portrayal of the relationship between engineering research within the U.S. university context and the Defense Advanced Research Projects Agency (DARPA), and military objectives generally:

> The most significant underwriter of engineering research in the United States in the Department of Defense, largely acting through the Defense Advanced Research Projects Agency (DARPA). DARPA exists to channel funds from the military to academic and corporate research labs in exchange for technological innovations that serve the needs of its clients—the Army, Navy, Air Force, and Marines. As DARPA public relations officers are fond of pointing out, innovations funded by DARPA grants may also find expression in civilian applications, particularly in the communications and aerospace industries.

> Researchers ('principal investigators') are held accountable to DARPA programme managers via aggressive schedules of milestones, deliverables, and administrative review. Framing this process as a form of cultural co-production implicates both researchers and military officers as active participants in constructing military-funded civilian research, and highlights tensions between martial and academic approaches to knowledge production. This depiction reveals opportunities for interventions that pose deep challenges to engineering culture. [23]

The IAA develops engineering projects based on metaphorical readings of DARPA texts; for example,

> our 'Contestational Robotics' (2004) initiative proceeds from a loose reading of DARPA's *Tactical Mobile Robotics* programme:
>
> > The *Tactical Mobile Robotics* program is developing robotics technologies and platforms designed to revolutionize dismounted ooperations by projecting operational influence and situational awareness into previously denied areas.
>
> Recognising the references to 'denied areas' as a metaphor for the privatisation of public space, we developed several devices that allow artists, activists, and juvenile delinquents to 'project operational influence in ways that humans cannot by using reliable semi-autonomous robotic platforms'. Like their military counterparts, our graffiti writing and humanoid propaganda machines are intended to perform actions too risky for human

> actors—although, in our case, the 'operations' include spray-painting slogans and distributing subversive literature, and the 'denied areas' are government buildings, shopping malls, and public streets. [23]

The IAA present their work "as 'research findings' at university lectures and technical conferences", and their projects "are reported on in engineering journals and trade publications." Their "critique of engineering practice", they argue, "thus comes from within engineering culture, and is given material weight by the production of working artifacts." The *mechanism*, then, is similar to that employed by Agre in his critique of AI's methods—the production of working technical systems—but the objective is broader in scope:

> ...DARPA involvement in academia normalises ambivalence among students and researchers. Although the agency's motivation is to enhance the military's ability to win wars and kill enemies, open declarations of martial efficacy are rare within academia. Instead, DARPA-supported research is presented to the academic community (including the students working on military projects) in abstract terms, as 'optimization algorithms' and 'enabling technologies'. Civilian applications are highlighted, thus fostering a sense that the particular (and, by extension, all) technologies are neutral. The rhetorical work done by this positioning of military research relies on the slippage between 'dual use' technologies, which have a varied but limited set of military and civilian applications, and 'general purpose' tools, which can be brought to bear on virtually any problem. While it may be argued that in practice there can be no such thing as a general purpose tool, emphasising civilian applications for a DARPA-funded research

> project downplays the particular application for which it has been designed and frees the engineer from responsibility for the uses to which it will most likely be put. The culture that celebrates technology's neutrality thus mobilizes ambivalence as a mechanism that enables thoughtful, well-intentioned individuals to work on projects they would otherwise find morally repugnant.
>
> ...By acting as engineers who address contentious political issues, we undermine the normalised ambivalence that characterises engineering practice. [Our] works thus act as Trojan horses, carrying our critique through the gates of detachment that guard engineers against taking responsibility for the products of their labour. In lieu of ambivalence, we offer the engineering community the image of an 'engaged engineering' that works diligently in the service of freedom and human dignity, and takes responsibility for the world it helps create. [23]

To use the terms of the second example of disciplinary critique, that offered by human-computer interaction (HCI) researcher Nicholas Knouf, drawing on the work of designer Victor Papanek, in his presentation to the "alt.chi" session of the 2009 Computer-Human Interaction conference "HCI for the Real World", the politically engaged projects presented to the engineering community by the Institute for Applied Autonomy "foreground the *agency* of the designer" (or engineer). What does HCI's comfortable and unquestioned relationship with "the corporate world...mean in terms of an ethics of *problem choice*," asks Knouf, "meaning the considerations that influence what types of design projects HCI researchers consider as important?" [24] (emphases in the original) Knouf, following Papanek, offers a vision of design practice as fundamentally socially and politically engaged:

> ...I must agree that the designer bears a responsibility for the way the products he designs are received at the market place. But this is still a narrow and parochial view. The designer's responsibility must go far beyond these considerations. His social and moral judgment must be brought into place long *before* he begins to design, since he has to make a judgment, and a prior judgment at that, as to whether the products he is asked to design or redesign merit his attention at all. In other words, will his design be on the side of the social good or not. [25] (p. 66), as cited in [24]

Given this conception of design, Knouf argues that the close linking of HCI with industrial concerns has brought "two diametrically opposed poles into close proximity: corporatism, which exists to increase profits for shareholders, and design, that ideally exists to improve the human conditions irrespective of monetary gain." Thus Knouf, although not operating extra-textually through the demonstration of working technical systems as Agre and the IAA do, encourages technical practitioners to consider objectives traditionally beyond those normally prioritized in the technical practice of the discipline. His critique, although perhaps perceived by some reviewers as snarky or strident, seems generally to have been accepted by the disciplinary community as well-articulated, relevant, and productive [26]; sufficiently so, at least, to have been accepted for presentation and archival publication.

Where do these examples lead us in terms of a theory and practice of socially and politically engaged critical mathematical modelling, in particular when that modelling serves the purpose of economic analysis? The central result, I would submit, is that the critical modeller—*as practitioner*, not just as 'theorist'—is obliged to engage with the question "Why this project, and not another?"—not only on the grounds of technical logics and 'open problems'

already understood as 'important' within a particular disciplinary discourse, but also on the grounds of personal values, beliefs, and politics, as they relate to broader social, economic, political, and cultural concerns, trends, and terrains. Under this rubric, then, the engineer who designs 'optimization algorithms' ostensibly for 'general use' is obliged to address seriously the question of whether or not said algorithms are in fact primarily for use in missile guidance systems, and subsequently to either defend or renounce the cultural and political logic which deems work in missile guidance acceptable; similarly, the human-computer interaction researcher who works to 'democratize access to information' is encouraged to subject this easy slogan to a more rigorous cultural and political analysis, and subsequently to reaffirm, reposition, or reassess their technical work given this more broadly situated understanding of its potential impacts, uses, preconditions, and/or implications.

In considering this broader analysis, two caveats should be made clear. The first is that it is unreasonable to expect any 'designer' to know the ways in which a 'design' will be taken up, or will influence the lives of its 'users' (and others); to postulate perfect knowledge would of course be to postulate a perfect technological determinism. At the same time, I will argue that it is utterly irresponsible to posit its opposite; a nuclear weapon is not a fork is not a book. "The idea," writes legal scholar Yochai Benkler, "is...distinct from a naïve determinism":

> Different technologies make different kinds of human action and interaction easier or harder to perform. All other things being equal, things that are easier to do are more likely to be done, and things that are harder to do are less likely to be done. All other things are never equal. That is why technological determinism in

> the strict sense—if you have technology "t," you should expect social structure or relation "s" to emerge—is false.
>
> ...Neither deterministic nor wholly malleable, technology sets some parameters of individual and social action. It can make some actions, relationships, organizations, and institutions easier to pursue, and others harder. In a challenging environment—be the challenges natural or human—it can make some behaviors obsolete by increasing the efficacy of directly competitive strategies. However, within the realm of the feasible—uses not rendered impossible by the adoption or rejection of a technology—different patterns of adoption and use can result in very different social relations that emerge around a technology. Unless these patterns are in competition, or unless even in competition they are not catastrophically less effective at meeting the challenges, different societies can persist with different patterns of use over long periods. [27]

The onus, then, is on the designer, modeller, or analyst to imagine plausible uptake scenarios for their technology, model, or narrative, and to make value judgments about these scenarios. It is not at all obvious that any particular methodology, disciplinary language, or approach should be naturally better than any other in developing these scenarios of making judgments about them; the problem, I submit, demands techniques and knowledges from many disciplines. The questions and evaluations are as political as they are technical, and as such the (self-)'critique' in 'critical modelling' requires the modeller not only to assume but to explicitly articulate and defend a political position.

This being said, we should be wary of approaches that reduce the analysis of sociotechnical systems or 'ecologies' to "the question of impacts."

Sociologists Robert Latham and Saskia Sassen write:

> ...impacts are only one of several forms of intersection of society and technology... Other forms of intersection have to do with the constitution of whole new sociotechnical relations and domains—digital formations—that in turn need to be constructed as objects of study. This means examining the specific ways in which these technologies are embedded in often very specialized and distinct contexts. And it requires examining the mediating cultures that organize the relation between these technologies and users, where we might think of matters as diverse as gendering or the utility logics that organize use. Because they are specific, these mediating cultures can be highly diverse; for example, when the objective is control and surveillance, the practices and dispositions involved are likely to be different from those involved in using electronic markets or engaging in large-scale computer-based conversations. [28] (pp. 8-9)

Agre highlights these imbrications and complications as well:

> Every technology fits, in its own unique way, into a far-flung network of different sites of social practice. Some technologies are employed in a specific site, and in those cases we often feel that we can warrant clear cause-and-effect stories about the transformations that have accompanied them, either in that site or others. Other technologies are so ubiquitous—found contributing to the evolution of the activities and relationships of so many distinct sites of practice—that we have no idea how to being

> reckoning their effects upon society, assuming that such a global notion of "effects" even makes sense.
>
> Computers fall in this latter category of ubiquitous technologies. In fact, from an analytical standpoint, computers are worse than that. Computers are representational artifacts, and the people who design them often start by constructing representations of the activities that are found in the sites where they will be used. This is the purpose of systems analysis, for example, and of the systematic mapping of conceptual entities and relationships in the early stages of database design. A computer, then, does not simply have an instrumental use in a given site of practice; the computer is frequently *about* that site in its very design. In this sense computing has been constituted as a kind of imperialism; it aims to reinvent virtually every other site of practice in its own image. [21]

The analogy to be drawn here between 'computers' and 'models', 'computing' and 'modelling' is exact: modelling, like computing—indeed as a precondition for computing, as beneath or implicit within every interactive software system is a model of the user, and encoded within every database a model of the knowledge it is designed to store, (re)order, and (re)present—is a way of knowing, a way of seeing, a way of interacting with and acting in the world.

The richness, texture, and flexibility of the analytic language called for here, then, seems likely to exceed, for example, those developed for the formal modelling approaches offered by Paul Weston and Heinz von Foerster as part of efforts to develop a "second-order cybernetics" or "cybernetics of cybernetics" (e.g., [29], as cited in [13]). Analyses grounded in assessments of costs and benefits, argues Donna Haraway, are, although perhaps politically

useful, similarly deficient:

> ...there is a way of thinking about any technology that has the same baleful effects as ...universal categories of human and machine, and that is thinking in terms of costs and benefits. And if you can make up an economic accounting sheet, an audit, you can audit a technology for cost over here, benefits over there, who gets which costs, who gets which benefits, and so forth. You handle it like an accounting problem. And I think that's a terrible mistake, or rather that's a tiny little bit of work that ought to come after we ask questions like "What kind of world is this?" Literally, ontological questions: What sorts of entities exist here, and with what kind of relationality? What are the practices here? We might find much more interesting things, including things that bear on who lives and who dies, that aren't well gotten at by thinking as an accountant or cost-benefit analyst. [30]

In the case of formal models (or formal modelling languages), one might wish to foreground the corollaries: what sorts of entities *don't* exist here? what ways of living and decision-making logics are written *out*, and go unrepresented, unaccounted for? What is 'marginalized' in the 'real' world becomes de-inscribed in the modeled world; it is not represented in the first place; it does not exist at all.

Latham and Sassen, following Latour [31], note the instability of some of the concepts fundamental to this flavor of analytical work:

> We do not assume that technology and society are actually separate entities, and we accept many of the propositions in the critical social science literature that posit that technology is one particular

> instantiation of society—society frozen, that is, one moment in a trajectory that once might have been experienced as simply social. Without losing this critical stance, we want, nevertheless, to capture the distinctiveness and variable weight of "technology" and to develop analytic categories that allow us to examine the complex imbrications between the outcome of society that we call technology and the social, economic, political, and cultural dynamics through which relations and domains are constituted. [28] (p. 1)

The critical modeller is confronted, then, with the task of developing a model, and simultaneously with the task of understanding not only what it *says* but what it *does* in the world; or, put another way, with the task of understanding (and articulating and 'defending') not only the world it represents but also the world it creates.

At this point I will leave off the discussion of critical mathematical modelling in general terms. We proceed now to a detailed technical description of the indices and models at hand, and subsequently to an interpretation and discussion of the work aided by the critical apparatus outlined above.

Chapter 3

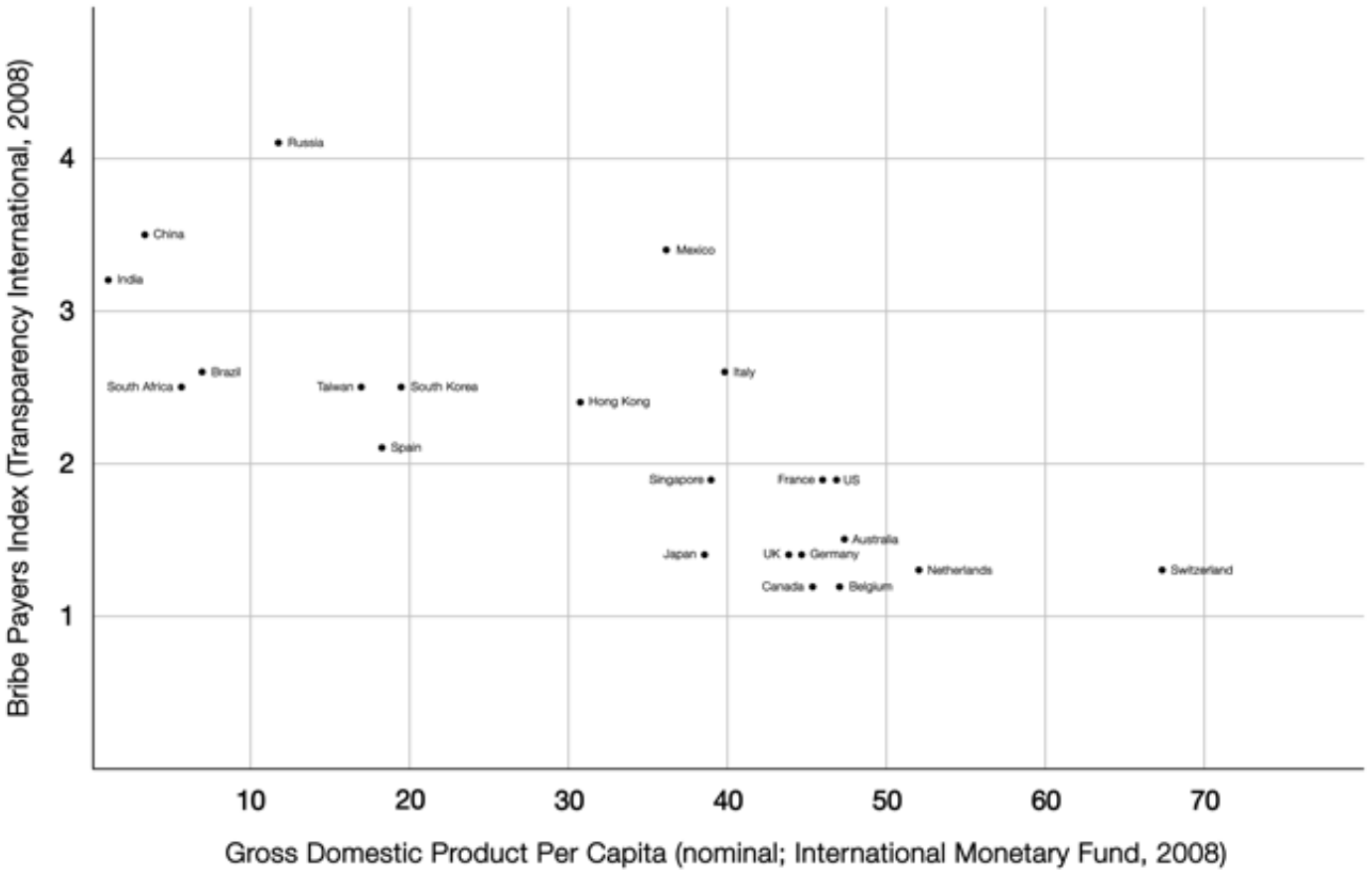
Technical description of the work

A visitor to the corner of the Beall Center for Art and Technology in which the Capitalism Collaps-O-Meter was displayed between June 9 and June 12, 2009 was presented with three 36" x 24" prints mounted on a wall, each accompanied by 8.5" x 11" wall texts divided into two sections; a fourth, 'stand-alone' 8.5" x 11" wall text; a 52" flatscreen LCD showing an interactive visualization mounted on a freestanding cart; and a smaller flatscreen monitor showing runtime logs and source code for the visualization. Each print featured a plot of a different dataset. The first print featured a scatterplot labeled "Bribe Payers Index" on the vertical axis, with nominal gross domestic product per capita on the horizontal axis, with each data point on the plot corresponding to (and labeled as) a particular nation-state; the second, a similar scatterplot, labeled "Index of Imperial Domination" on the vertical axis and "Fuel Exports, Percentage of Total Exports" on the horizontal axis; the third, a vertical bar graph, with bars in red, labeled "Miles Per Casualty" on the vertical axis and the years 2003 through 2008 on the horizontal axis. Each print will be discussed in detail below.

3.1 Bribe Payers Index

The Bribe Payers Index (BPI) is an index constructed from data collected for the nonprofit Transparency International (TI) in their 2008 Bribe Payers Survey. In their words:

> The 2008 Bribe Payers Survey consists of 2,742 interviews with


Bribe Payers Index (Transparency International, 2008)
4
3
2
1
Russia
China
India
Mexico
Brazil
South Africa
Taiwan
South Korea
Italy
Hong Kong
Spain
Singapore
France
US
Australia
Japan
UK
Germany
Netherlands
Switzerland
Canada
Belgium
10
20
30
40
50
60
70
Gross Domestic Product Per Capita (nominal; International Monetary Fund, 2008)

senior business executives in 26 countries and territories completed between 5 August and 29 October 2008. The survey was carried out on behalf of TI by Gallup International, which was responsible for the overall implementation of the survey and the data quality control process. Gallup International relied on a network of partner institutions to carry out the survey locally.

The countries surveyed were selected on the basis of their Foreign Direct Investment (FDI) inflows and imports, and importance of regional trade. Total inflows of GDI and imports of goods from these 26 countries amounted to 54 percent of world flows in 2006.

In each country there were a minimum of 100 senior business executives interviewed and samples in each country were designed taking into consideration the following variables: the size of firms, sector and location. Additionally, due to the nature of the phenomenon under analysis, the survey oversampled large and foreign-owned firms.

To assess the international supply side of bribery reflected in the 2008 BPI, senior business executives were asked about the likelihood of foreign firms from countries they have business dealings with to engage in bribery when doing business in the respondents' country. In short, senior business executives provided their informed perceptions of sources of foreign bribery, and these views formed the basis of the 2008 BPI.

The 2008 BPI is calculated based on two questions from the Bribe Payers Survey. Senior business executives were first asked which of the 22 countries to be ranked they have commercial relationships

> with. For those countries that they selected, they were then asked to assess the frequency with which companies from these countries engage in bribery when operating in their own (the respondents') countries. [32]

In TI's report, countries are scored on a scale of 0 to 10, with a score of 10 indicating that companies based in that country were reported to never engage in bribery. (No country received a score better than 8.8.) To create the plot, I subtracted the scores from 10 in order to create an index that approximated something like the (perceived) 'frequency' of bribery.

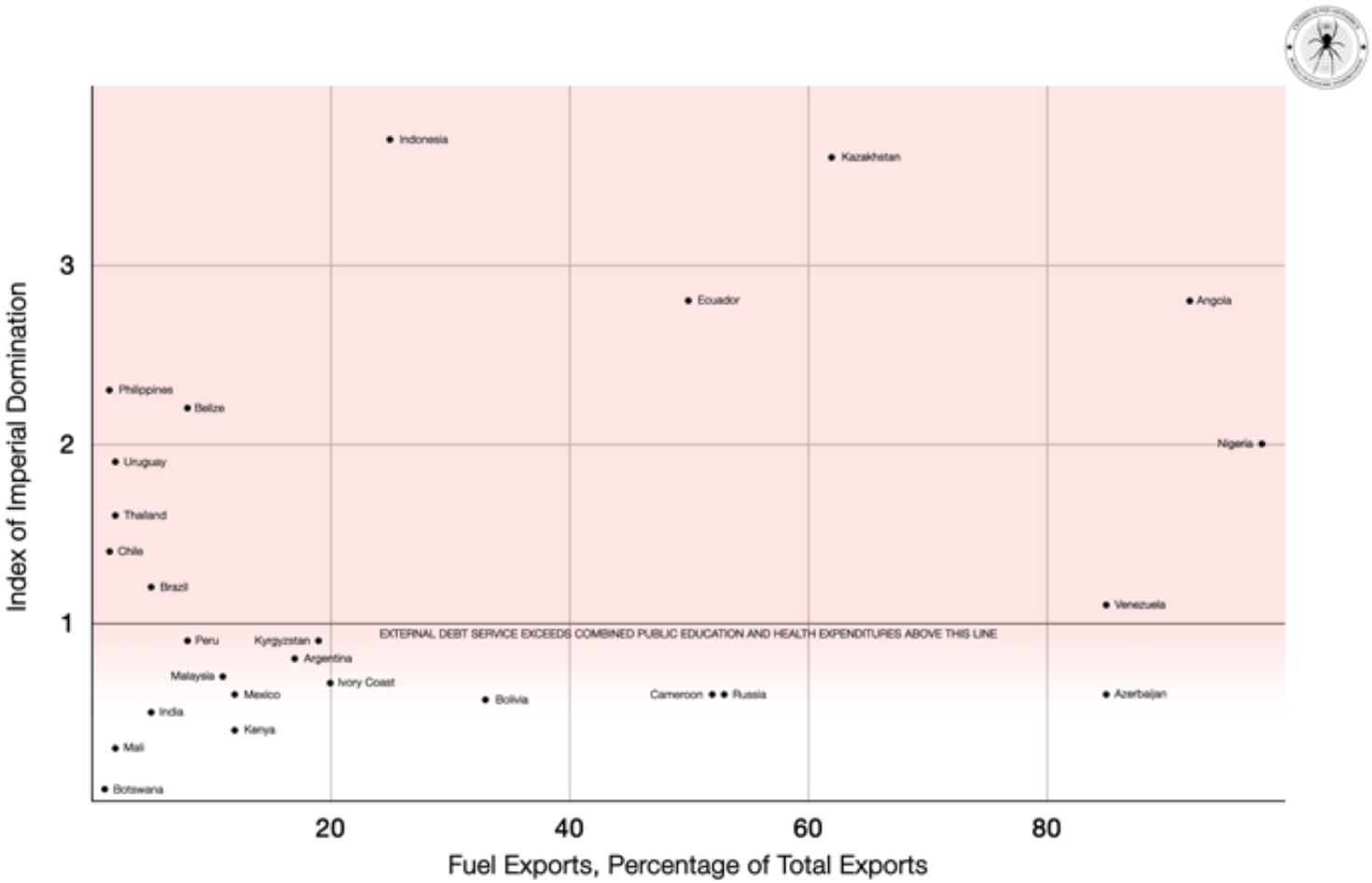
Along the horizontal axis I plotted nominal Gross Domestic Product per capita.

3.2 Index of Imperial Domination

The Index of Imperial Domination (IID) purports to quantify the degree to which the political and financial elite of some nations control economic and social policy in other nations. It is a per-country indicator. The value of D for a given country indicates the extent to which policy in that country is "dominated" by external actors. In particular, for a given country in a given time period,

$$D = \frac{d}{h + e}$$

where d is the fraction of the country's total government budget dedicated to debt service in the period in question, and h is the fraction dedicated to health care in that same period. In practice, this is computed as

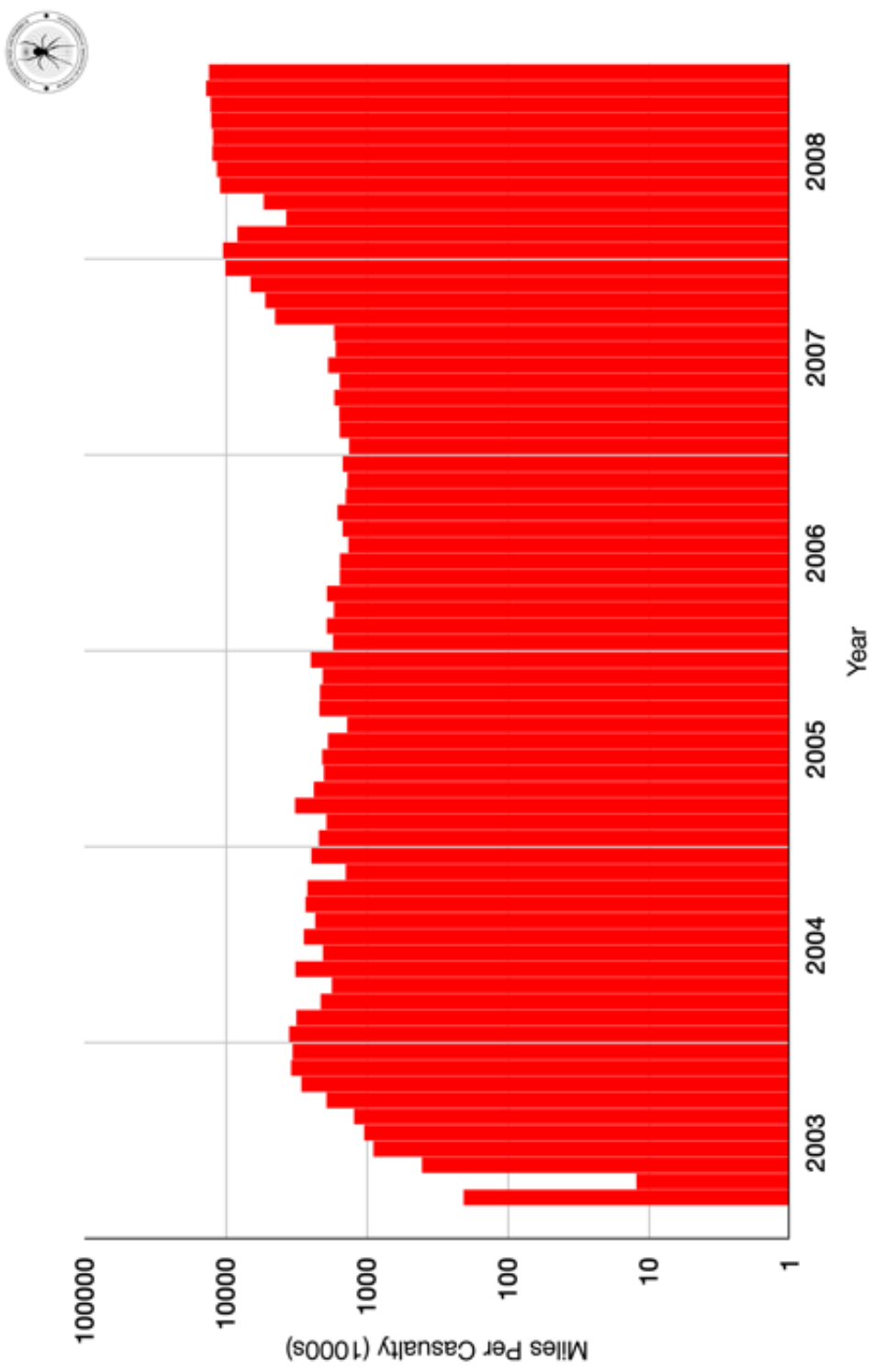

Index of Imperial Domination
3
2
1
Indonesia
Kazakhstan
Ecuador
Angola
Philippines
Belize
Nigeria
Uruguay
Thailand
Chile
Brazil
Venezuela
EXTERNAL DEBT SERVICE EXCEEDS COMBINED PUBLIC EDUCATION AND HEALTH EXPENDITURES ABOVE THIS LINE
Peru
Kyrgyzstan
Argentina
Malaysia
Ivory Coast
Mexico
Bolivia
Cameroon
Russia
Azerbaijan
India
Kenya
Mali
Botswana
20
40
60
80
Fuel Exports, Percentage of Total Exports

$$D = \frac{d_x \hat{x}}{\hat{h} + \hat{e}}$$

where d_x is the total debt service as a fraction of export earnings, $\hat{x}$ is the total value of exported goods and services as a fraction of GDP, and $\hat{h}$ and $\hat{e}$ indicate government expenditure on public health and public education (respectively), as a fraction of GDP. These data are taken directly from the World Resources Institute's EarthTrends database [33], which for these variables relies primarily on data adapted from the World Bank's World Development Indicators.

Countries with index values exceeding 1 spent a larger fraction of their budgets in the period in question on external debt service than on public health and education combined. This suggests that one interpretive approach to the IID is to consider whether governments spending more money on debt service than on health and education have effectively had their national sovereignty compromised by the Bretton Woods institutions (the IMF and World Bank in particular).

In the print, the Index of Imperial Domination is plotted against fuel exports as a percentage of a nation's total exports. This quantity can be read in some cases as an indicator of the extent to which a nation's economic life is dependent on exports of fuel (primarily oil and natural gas), and has been examined a great deal in discussions of the so-called "resource curse," a phrase used to denote the observation that "resource-poor economies often vastly outperform resource-rich economies in economic growth" [34] (p. 1).


Miles Per Casualty (1000s)
100000
10000
1000
100
10
1
2003
2004
2005
2006
2007
2008
Year

3.3 Miles Per Casualty

Miles Per Casualty (MPC) is the number of automobile miles driven by an average American for each Iraqi civilian casualty associated with the extraction and refinement of the oil required to fuel those automobiles. Specifically, for a given time period t, the MPCM_t is determined by

$$M_t = mg\frac{B_t}{K_t}$$

where m is average US automobile mileage in miles per gallon (MPG), here estimated at 22.5 based on data from the US Bureau of Transportation Statistics [35]; g is the number of gallons produced by an average barrel of oil (here estimated at 19, based on US Energy Information Administration data [36] and a discussion archived on the server of the Argonne National Laboratory [37]); B_t is the number of barrels of oil exported from Iraq during time period t (here based on US Energy Information Administration data [38]); and K_t is the number of civilian casualties during the same period. Monthly values for K (one value K_t for each month t) are computed by multiplying the background monthly casualty rate k_L implied by the October 2006 *Lancet* study [39] by a month-specific multiplier η computed from data compiled by Iraq Body Count [40],

$$K_t = \eta_t k_L$$

where

$$\eta_t = \frac{1}{\sum_t k_t^I} k_t^I$$

that is, η_t is the number of total casualties, k_t^I, recorded by Iraq Body Count

in month t divided by $\sum_t k_t^I$, the total casualties recorded by Iraq Body Count over the study period.

3.4 Capitalism Collaps-O-Meter

The Capitalism Collaps-O-Meter is a simulation of economic change with allows the 'user' to manipulate different parameters describing a social-political-economic system, and observe the effect of these parameters on the distribution of incomes, 'income utilities', and 'situated income utilities' in the system, and through these, their effect on the system's position on a hypothetical spectrum between 'stability' and 'collapse'. For a population of citizen-subjects (or, in the language of computational economics, 'agents') of a given size n, at any time t the value of the Collaps-O-Meter $c(t) \in [0, 1]$ is defined by

$$c(t) = 1 - \frac{1}{n\hat{v}} \sum_{i=1}^{n} v_i(t) \tag{3.1}$$

where $i = 1, 2, ..., n$, and

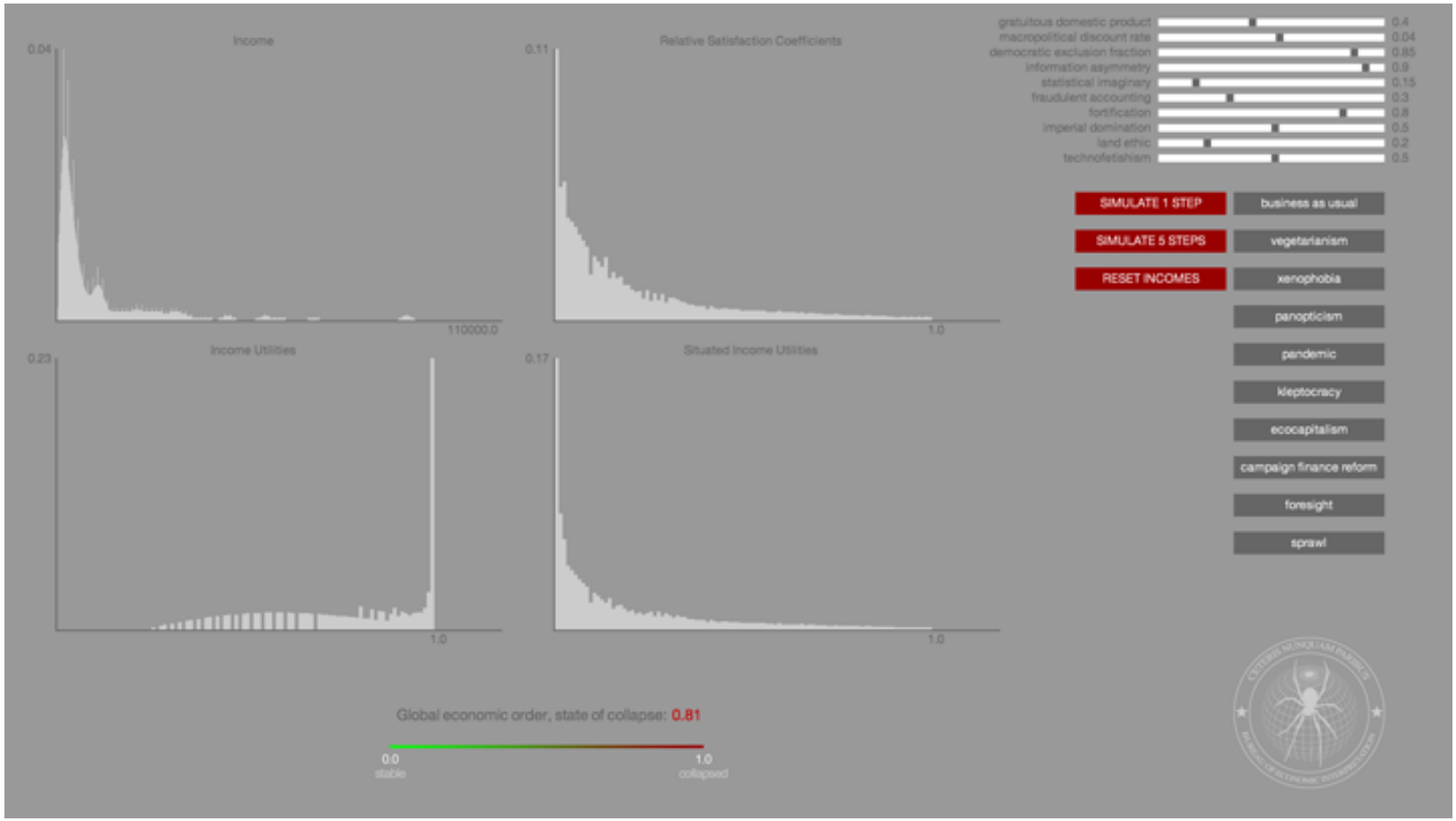
$$v_i(t) = \alpha_i(t) u(y_i(t), y^*(t)) \tag{3.2}$$

$$\hat{v}(t) = u(\hat{y}(t), y^*(t)) \tag{3.3}$$

$$\hat{y}(t) = \frac{1}{n} \sum_{i=1}^{n} y_i(t) \tag{3.4}$$

$$\alpha_i(\boldsymbol{y}(t)) = \frac{1}{\sum_{j=1}^{n} y_j(t)} \sum_{j : y_j(t) \leq y_i(t)} y_j(t) \tag{3.5}$$

$$u(y_i(t), y^*(t)) = \exp\left(-\exp[\beta(y_i(t) - y^*(t))]\right) \tag{3.6}$$


Income
0.04
110000.0
Relative Satisfaction Coefficients
0.11
1.0
Income Utilities
0.23
1.0
Situated Income Utilities
0.17
1.0
gratuitous domestic product 0.4
macropolitical discount rate 0.04
democratic exclusion fraction 0.85
information asymmetry 0.9
statistical imaginary 0.15
fraudulent accounting 0.3
fortification 0.8
imperial domination 0.5
land ethic 0.2
technofetishism 0.5
SIMULATE 1 STEP
SIMULATE 5 STEPS
RESET INCOMES
business as usual
vegetarianism
xenophobia
panopticism
pandemic
kleptocracy
ecocapitalism
campaign finance reform
foresight
sprawl
Global economic order, state of collapse: 0.81
0.0
stable
1.0
collapsed

$$\frac{dy^*(t)}{dt} = g\phi DK - se \tag{3.7}$$

$$\frac{dy_i(t)}{dt} = (y_i(t) - y^*(t))\left(gxif\phi DK - \frac{se}{r}\right) \tag{3.8}$$

where

- $y_i(t)$ denotes the income of 'agent' i at simulation time t, and $\boldsymbol{y}(t)$ denotes the vector of all incomes y_i at time t;
- $\hat{y}(t)$ denotes the mean world income at time t;
- $v_i(t)$ denotes the 'situated income utility' obtained by agent i at time t as a result of having income $y_i(t)$, given the global distribution of incomes $\boldsymbol{y}(t)$;
- $\alpha_i(t)$ is a 'relative satisfaction coefficient' used to represent the assumption that an agent whose basic needs are met will be happier if it is wealthier than most other agents in the population, and less happy if it is less wealthy than most other agents in the population;
- $u(y_i(t), y^*(t))$ is the 'utility' obtained by agent i at time t, given that its income at that time is $y_i(t)$, and the global poverty line at that time is $y^*(t)$; and
- $\hat{v}(t)$ is the utility obtained by an agent with the mean global income $\hat{y}(t)$.

The parameters controlled by the sliders in the upper-right hand corner of the interface are

- $g \in [0, 1]$, the 'gratuitous domestic product';
- $r \in [-1, 1]$, the 'macropolitical discount rate';

- $x \in [0,1]$, the 'democratic exclusion fraction';
- $i \in [0,1]$, 'information asymmetry';
- $s \in [0,1]$, 'statistical imaginary';
- $f \in [0,1]$, 'fraudulent accounting';
- $\phi \in [0,1]$, 'fortification';
- $D \in [0,1]$, 'imperial domination';
- $e \in [0,1]$, 'land ethic'; and
- $K \in [0,1]$, 'technofetishism'.

When the interface is loaded (or when the 'user' clicks on the red 'RESET INCOMES' button), the histogram in the upper left hand corner of the interface (titled 'Income') is displayed, with a distribution that corresponds roughly to the world distribution of income in 2006, according to the World Resources Institute's EarthTrends database [33]. The model is populated with 6197 'agents', approximately one for every one million inhabitants of the world in 2006. When the 'user' clicks the button 'SIMULATE 1 STEP', the model:

1. adjusts the incomes of each agent $y_i(t)$ based on the poverty line and the parameter values the 'user' has chosen with the sliders (Eqn. 8);
2. adjusts the poverty line $y^*(t)$ based on the parameter values the user has chosen (Eqn. 7);
3. computes the 'income utilities' $u(y_i(t), y^*(t))$ of each agent based on the updated incomes and poverty line (Eqn. 6);
4. computes the 'relative satisfaction coefficients' $\alpha_i(t)$ of each agent based on the new global distribution of income (Eqn. 5);

5. computes the 'situated income utilities' $v_i(t)$ of each agent, which weights their 'raw' income utilities with their relative satisfaction coefficients (Eqn. 2); and finally

6. computes the value of economic collapse $c(t)$ (Eqn. 1), which depends on the income utility of a hypothetical agent with the mean global income (Eqns. 3 and 4).

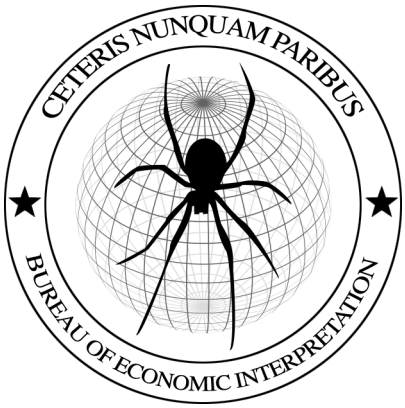
As each set of values is computed, the histograms in the interface are updated; after the value of economic collapse is computed, the graphic at the bottom of the interface is updated to reflect the new value. If the user clicks on the button labeled 'SIMULATE 5 STEPS', the computational process is run 5 times before the visualizations are updated.

The gray buttons on the far right hand side of the interface (labeled 'business as usual', 'vegetarianism', 'xenophobia', etc.) can be understood as 'scenarios'. Each scenario corresponds to a particular set of values for the parameters. The scenarios are:

scenario name	g	r	x	i	s	f	ϕ	D	e	K
business as usual	0.6	0.04	0.8	0.8	0.1	0.5	0.8	0.5	0.2	0.6
vegetarianism	0.4	0.04	0.8	0.8	0.1	0.5	0.8	0.5	0.5	0.5
xenophobia	0.6	0.04	0.8	0.8	0.05	0.5	0.95	0.7	0.2	0.8
panopticism	0.6	0.04	0.8	0.5	0.3	0.3	0.8	0.5	0.2	0.9
pandemic	0.6	0.2	0.9	0.95	0.1	0.6	0.95	0.6	0.1	0.8
kleptocracy	0.6	0.1	0.8	0.9	0.1	0.9	0.9	0.7	0.2	0.6
ecocapitalism	0.4	0.04	0.8	0.8	0.2	0.5	0.8	0.5	0.6	0.6
campaign finance reform	0.55	0.04	0.5	0.8	0.1	0.4	0.8	0.5	0.2	0.6
foresight	0.6	-0.5	0.8	0.9	0.1	0.5	0.8	0.5	0.2	0.7
sprawl	0.8	0.08	0.8	0.9	0.1	0.5	0.7	0.5	0.05	0.8

Because scenarios cannot be 'combined' by clicking one button and then another (say, 'xenophobia' followed by 'panopticism' in order to get 'xenophobia plus panopticism'), the distinctive features of each scenario are 'translated' into the 'language' of the parameter set as changes made to the 'business as usual' scenario, which is used as a baseline.

3.5 The Bureau of Economic Interpretation



The Bureau of Economic Interpretation (BEI) is imagined as a hypothetical research agency which might produce the indices and models presented here. The circular BEI seal, a sphere covered in lines of latitude and longitude overlaid by a stylized spider and surrounded by two stars and the Latin phrase 'ceteris nunquam paribus', appears in the upper right hand corner of all three prints and in the lower right hand corner of the interface to the Collaps-O-Meter. This phrase is a reference to the condition one often finds in discourses surrounding formal models of complex phenomena of all kinds, 'ceteris paribus,' meaning, 'all else being equal.' It often appears in the context of prediction: "If [condition], then, *ceteris paribus*, [outcome]." 'Nunquam' means 'never', indicating a general skepticism about the habit of ignoring variables presumed to be unimportant.

Chapter 4

Critical mathematical modellings

4.1 Reading the Bribe Payers Indices

The Bribe Payers Index, as discussed above, is compiled by a nongovernmental organization called Transparency International (TI), which self-describes as "the global civil society organisation leading the fight against corruption." Their "mission," according to the 'About' page on their website, "is to create change towards a world free of corruption" [41]. The original 2008 "Bribe Payers Index" is released in a 24-page report, available as a PDF on the TI website, with associated press releases in English, French, Spanish, and Japanese. In the report, the salient data—values of the Bribe Payers Index for each country—are presented in tabular form on page 5, following a description of the survey methodology. BPI values fall between 0 and 10; in TI's original formulation, a higher value indicates that survey respondents perceived companies from the country in question as less likely to engage in bribery, not more. This seemed counterintuitive to me; on a "Bribe Payers Index," it seemed to me, the most practiced 'bribe payers' ought to score the highest! Accordingly, for the plot, I subtracted the original values from 10 in order to reverse the 'polarity' of the index. In the index's original discursive context, however—a report published by a well-known NGO with an international publicity apparatus, invested in fostering 'transparency' (and in being seen as at least somewhat successful in fostering it)—it is not difficult to imagine that a decision might have been made to equate a higher score with the ostensibly preferable political condition (less perceived 'bribery'). Additionally, this

ordering is commensurate with TI's previous work; for example, the widely-publicized 'Corruption Perceptions Index', on which 'clean' countries like Denmark are ranked higher than 'corrupt' ones like Haiti [42].

"Corruption and bribery are complex transactions," begins the introduction to the 2008 BPI [32] (p. 2)—and certainly this is difficult to dispute. The phenomenon quantified in the report, however—'perceived bribery'—is more rather than less conceptually opaque than the already slippery terms 'corruption' and 'bribery'. By plotting perceived bribery as reported in the BPI against nominal GDP per capita, the BPI plot helps us to ask what it means both to quantify 'bribery' and, more specifically, to attempt to do so with data collected through a survey of ostensibly well informed business executives consisting of questions to be answered on a 5-point Likert scale. Why does 'perceived bribery' correlate negatively with GDP?

If we take the BPI ('perceived bribery') as a trustworthy proxy for 'bribery', the correlation should encourage us to direct our attentions to the conditions under which 'bribery' occurs, and what it achieves. Do businesspeople from poorer nations have a stronger profit imperative than businesspeople from richer ones, such that they turn to 'bribery' as a technique to ensure or augment shareholder returns? Are they 'less skilled' at 'business' and therefore find themselves compelled to use 'bribery' as a technique to achieve the same sorts of returns as their competitors from wealthier (and perhaps more 'culturally' business-savvy) nations?

Explanatory power can be read the other way on the plot, of course; perhaps we should not look for GDP to explain bribery but rather for bribery (or its lack) to explain GDP? Armed with this kind of plot (although perhaps with the axes switched, so as to conform to the convention of plotting the ostensibly 'independent' variable on the horizontal axis and the 'dependent'

on the vertical), one could deploy fairly straightforwardly a narrative about how poor countries are 'held back' economically by their endemic corruption, or even by their cultural inability (so compellingly demonstrated by the methodologically unobjectionable BPI, especially if one is prone to eliding 'nation' with 'culture'—see e.g. Geert Hofstede's much-cited *Culture's Consequences* [44]) to play by the sensible, rational, impartial, rules of global capitalism. Indeed, the International Monetary Fund seems to have done exactly this; on a page entitled "The IMF and good governance," we learn that "poor governance clearly is detrimental to economic activity and welfare", and that the IMF "promot[es] good governance through IMF surveillance, lending, and technical assistance", "work[ing] with its members to prevent and address corruption in areas where the Fund has a mandate and expertise":

> These include, notably, public resource management and financial sector soundness where the emphasis is on establishing strong and transparent procedures and institutions in order to ensure accountability. The Fund also helps strengthen countries' capacity to combat corruption by advising on appropriate anti-corruption strategies, commissions, and legislation. [43]

In this way, then, narratives of corruption and poor governance as explanatory factors for poverty have paved the way for international intervention in the domestic policies of ostensibly sovereign nations. (The effects of these interventions will be discussed at some length in the following section, in the context of the Index of Imperial Domination.)

Another reading of the plot is to infer that the correlation implies that the BPI ('perceived bribery') is a bad proxy for 'bribery', because respondents simply use their preconceived ideas about different countries' corruption or lack thereof (which may be based substantially on widely-held knowledge

about relative national wealth) to make guesses about the relative frequencies of bribery.

Yet another reading of the plot is that it problematizes 'bribery' as a globally useful ontological category. Suppose we accept that GDP actually does correlate meaningfully (negatively) with something the staff of a Berlin-based NGO would recognize as 'bribery'. Does this definition map onto notions of what is or is not appropriate business practice in other cultural contexts? This can be read as an argument about the 'backwardness' of poor nations or, just as easily, about the perverse estrangement of the 'economic', the 'political', and the 'social' in market 'democracies'; if the reader refuses to take as unproblematic the category of 'bribery', the plot, by marking difference, can serve to render normalized practices in the 'West' anthropologically strange. The plot does not, however, do this conceptual work 'on its own'; it does not take a straightforwardly didactic approach.

What else can we say the Bribe Payers Index *does*? TI's report points out that "much blame has been apportioned over the years to the bribe takers,"

> those who pocket the wealth and take advantage of the influence and authority that corruption affords them. And, indeed, bribe takers must be exposed, prosecuted and appropriately punished. The systems that breed this behaviour require holistic reform, so that bribes are not demanded in the first place.
>
> TI believes it is also critical to shine a spotlight on the bribe payers—whose supply of bribes, irregular payments and other forms of influence-buying fuel the machinery of corruption. [32] (p. 2)

This "shining the spotlight", this foregrounding of the "supply side of corruption" is ostensibly the purpose of the BPI report. As a discursive object,

I think it succeeds, even if the analytical categories deployed are somewhat suspect and the method fraught. The report also claims, however, that "part of TI's mission" is "to *curb* the...supply side of corruption" (p. 2, emphasis added). The report can be a first, taxonomic and perhaps even cartographic piece of this project. But it has serious epistemological flaws that limit its use in constructing actionable knowledge of its object. How would one go about 'curbing' the supply side of corruption? The report does little to imagine approaches, to examine the conditions under which offering a bribe seems like a reasonable thing to do (or the mechanisms which bring these conditions about), or even to warn readers of issues which might contraindicate use of the BPI data in such an examination. Rather, it "calls on governments and the private sector to renew their efforts to curb the supply side of corruption" (p. 2). By plotting the BPI against GDP, the index is more easily deconstructed. After doing this, we can more easily develop an understanding of what it can and can't (or at least shouldn't) be used for.

How does the BPI—both in the report and plotted against GDP—encounter the 'challenges' of 'critical mathematical modelling'—'variables'; 'performativity'; 'data'; 'reflexivity'—outlined above? In fact, I read TI's report as being rather savvy about these challenges. If we take the stated objective of the report—to 'shine the spotlight' on the 'supply side of corruption'—at face value, we can then assume that the report is self-consciously a performance. It seems reasonable, following this, to imagine that an audience of NGOs and their corporate and governmental sponsors might be partial (or at least accustomed) to what they might perceive as the rational communication of objective data and analysis. By conducting a survey of presumably reputable businesspeople (especially a quantitative survey), the report authors leverage the fantasy of objectivity and the fallacy of concreteness to gain discursive

legitimacy and achieve their performative objective. However, the flaws that limit the index's utility in usefully theorizing 'bribery' in such a way as to guide 'strategic' or even 'tactical' action to mitigate some of the "extreme inequity" [32] (p. 2) attributed to it highlight a well-discussed pathology of the NGO sector: the staff of any organization which 'solves' the 'problem' it publicly assigns itself is out of work. By dealing in perceptions and abstractions, TI can safely foreground the 'supply side of corruption' while refraining from generating actionable recommendations (for which their method is unfit). A close reading of the plot of BPI vs. GDP suggests that if we take the problem of 'corruption' as posed by TI seriously, the knowledge being generated is inappropriate to the analytical task at hand. That non-actionable knowledge is being generated (and generally lauded) is taken to indicate a broader pathology in the context of knowledge production; here then I follow Agre's inclination to locate the "intellectual utility of technical exercises, aside from the practical utility that they might actually have in the world, ...precisely in their limitations and failures":

> Perhaps we can learn to approach technical work in the spirit of reductio ad absurdum: faced with a technical difficulty, perhaps we can learn to diagnose it as deeply as possible. Some difficulties, of course, will be superficial and transient. But others can serve as symptoms of deep and systematic confusions in the field. We are only aware of this possibility if we develop the critical tools to understand the depths below the ordinary practices of a technical field. Some of these critical tools will draw on the methods of institutional and intellectual analysis that have served generations of philosophers, sociologists, and literary critics. Others may be responses, each sui generis, to the specific properties of technical

> work. Research could proceed in a cycle, with each impasse leading to critical insight, reformulation of underlying ideas and methods, fresh starts, and more instructive impasses. [21]

Of course, in the case of statistical indices produced by international NGOs ostensibly for the purpose of combating corruption (through which, it is presumed, we will eventually combat "extreme inequity", poverty, unequal opportunity, and other ills many and various), we may sometimes find that the failure to be of "practical utility" is itself an intellectual failure, and can be read, as in this case, as symptomatic of larger structural or philosophical contradictions.

4.2 Running (and reading) the Capitalism Collaps-O-Meter

The Capitalism Collaps-O-Meter is a simulation and representation of economic change. When the simulation is initialized, the operator is presented with:

- a histogram labeled 'Income': this is a representation of the incomes of the 6187 computational 'agents' which make up the 'world' of the simulation;
- ten 'sliders', each corresponding to one of the model parameters ('gratuitous domestic product', 'information asymmetry', etc.; see above for the complete list);
- ten gray buttons, each corresponding to a 'scenario', or preprogrammed collection of parameter settings ('xenophobia', 'vegetarianism', etc., see above for the complete list);

- two red buttons, labeled 'SIMULATE 1 STEP' and 'SIMULATE 5 STEPS'; and
- a value between 0 and 1 indicating the state of the model economic system, with a graphic indicating that a value of 0 corresponds to 'stable' and a value of 1 to 'collapsed'.

In the current version, the income distribution corresponds roughly to the world distribution of income in 2006, with each simulated 'agent' corresponding to a block of approximately one million human beings; based on this distribution, the initial system 'collapse value' in this version is 0.81. When the operator clicks the button labeled 'SIMULATE 1 STEP', the incomes $y_i = y_1, y_2, ..., y_n$ of each agent $i = 1, 2, ..., n$ are updated according to the formulae

$$\frac{dy^*(t)}{dt} = g\phi DK - se \tag{4.1}$$

$$\frac{dy_i(t)}{dt} = (y_i(t) - y^*(t))\left(gxif\phi DK - \frac{se}{r}\right) \tag{4.2}$$

$$y^*(t+\tau) = y^*(t) + \frac{dy^*(t)}{dt}\tau \tag{4.3}$$

$$y_i(t+\tau) = y_i(t) + \frac{dy_i(t)}{dt}\tau \tag{4.4}$$

where $y^*(t)$ is the 'poverty line' in the simulated world (in this version of the model, initialized at 730, which could be taken to stand for USD 2 per day) and τ is the length of a 'time step' (in this version of the model, always equal to 1).

Eqn. 11 can be interpreted as follows: At some time $t + \tau$, the poverty line $y^*(t+\tau)$ is equal to the poverty line at a prior time $y^*(t)$ *plus* the derivative of

the poverty line with respect to time (that is, the 'rate of change' of the poverty line) *at the prior time*, $\frac{dy^*(t)}{dt}$, multiplied by the length of the interval between the current time and the prior time, τ. Eqn. 12 can be read identically, substituting the income of agent i, y_i, for the poverty line y^*.

The rates of change used in Eqns. 11 and 12 to update the values of the poverty line and agent incomes are defined in Eqns. 9 and 10. These equations constitute what might be referred to as the ideological core of the model. Recall from the prior section that g, x, i, f, ϕ, D, K, s, and e must lie between 0 and 1, and r must lie between -1 and 1. This means that Eqn. 10 describes the world as existing in one of three regimes:

$$gxif\phi DK - \frac{se}{r} > 0 \tag{4.5}$$

$$gxif\phi DK - \frac{se}{r} = 0 \tag{4.6}$$

$$gxif\phi DK - \frac{se}{r} < 0 \tag{4.7}$$

We can call these Regime I, Regime II, and Regime III, respectively. Eqn. 10 can be expressed more simply as

$$\frac{dy_i(t)}{dt} = p_i(t)w \tag{4.8}$$

where $p_i(t) = y_i(t) - y^*(t)$ and $w = gxif\phi DK - \frac{se}{r}$. If $p_i(t) < 0$, agent i's income is below the 'poverty line' $y^*(t)$; agent i is 'poor'. If at some time t the world is in Regime I, then $w > 0$, and agents i with $p_i(t) < 0$ will have $\frac{dy_i(t)}{dt} < 0$ also. Conversely, in Regime I, agents with $p_i(t) > 0$ will have $\frac{dy_i(t)}{dt} > 0$. That is:

In Regime I, the rich get richer and the poor get poorer.

If, on the other hand, $w < 0$, then agents i with $p_i(t) < 0$ will have $\frac{dy_i(t)}{dt} > 0$; that is:

In Regime III, the rich get less rich and the poor get less poor.

In Regime II, $w = 0$, so $\frac{dy_i(t)}{dt} = 0$ for all agents i: all incomes stay the same.

What causes the simulation to move between regimes? As g, x, i, f, ϕ, D, and K increase in relation to s and e, the model economy moves toward (or deeper into) Regime I. As s and e increase, it moves toward (or deeper into) Regime III. As the discount rate r rises, the effect of s and e is reduced. If r is negative, w is nonnegative and the model economy is in Regime I (unless $gxif\phi DK - se = 0$, in which case it is in Regime II).

Why arrange these parameters this way? In short, the model suggests that g, x, i, f, ϕ, D, and K contribute to economic inequality over long times, while s and e reduce it. These parameters are:

- $g \in [0, 1]$, the 'gratuitous domestic product', denoting the fraction of economic activity in excess of that required to fill human 'needs' (that fraction which is 'gratuitous');

- $r \in [-1, 1]$, the 'macropolitical discount rate'. This is the 'discount rate' familiar to students of mainstream economic theory; that is, it is the rate at which we "discount future benefits that come from current costs". In mainstream economic theory it is traditionally positive. I allow it to be negative in the model to point to ongoing discussions in the economics of climate change, which suggest that if we believe that climate change will seriously damage economic activity if steps are not taken 'ahead of time' to mitigate its effects, then we should apply a negative discount rate in evaluating economic decisions (eg., [45]). Upon some reflection, I suspect that $w = \frac{1}{r}gxif\phi DK - se$ may more accurately reflect this

analytical position than the expression used in the current version of the simulation.

- $x \in [0, 1]$, the 'democratic exclusion fraction', denoting the fraction of the population excluded from taking part in political and economic decisions which affect their lives and livelihoods;
- $i \in [0, 1]$, 'information asymmetry', denoting the extent to which these decisions are discussed and taken effectively in secret;
- $f \in [0, 1]$, 'fraudulent accounting', denoting the extent to which complex systems of economic bookkeeping are gamed (e.g., Enron);
- $\phi \in [0, 1]$, 'fortification', denoting the extent to which, although 'globalization' may facilitate transnational flows of capital, media, and 'intellectual property', transnational flows of persons are often strictly regulated;
- $D \in [0, 1]$, 'imperial domination', a normalized version of the Index of Imperial Domination discussed in the previous section;
- $K \in [0, 1]$, 'technofetishism', denoting the extent to which policymakers tend to look for 'silver bullet' technological or policy 'fixes' for complex social-political-cultural 'problems';
- $s \in [0, 1]$, 'statistical imaginary', standing in for Immanuel Kant's notion of "enlarged mentality" (later taken up by Hannah Arendt), "the capacity to think...from the standpoint of everyone else" [46]; and
- $e \in [0, 1]$, 'land ethic', indicating the prevalence in the simulated world of the land ethic promulgated by Aldo Leopold, in which "a thing is right

when it tends to preserve the integrity, stability, and beauty of the biotic community" and "is wrong when it tends otherwise" [47].

That is: in the model, gratuitous products, democratic exclusion, information asymmetry, fraudulent accounting, fortification, imperial domination, technofetishism, and a high discount rate are all seen as contributing to economic equality over long times; a statistical imagination (or "enlarged mentality") and widespread land ethic are seen as eroding it.

In the current version of the model, all of the parameters are independent, and under direct operator control; that is, they are 'exogenous'. One might imagine a more sophisticated model in which the parameters interacted (that is, in which they were 'endogenous': given an initial value for each parameter, the model computes a new value at each time step without additional operator input). Consider for example

$$\frac{df}{dt} = c_1 K + c_2 x + c_3 i \tag{4.9}$$

$$\frac{dx}{dt} = c_4 i \tag{4.10}$$

$$\frac{di}{dt} = c_5 x + c_6 \alpha K \tag{4.11}$$

$$\alpha = D - \hat{D} \tag{4.12}$$

and suppose we select $0 < c_j \leq \frac{1}{100}$ for $j = 1, 2, ..., 6$. Then we can interpret these equations as follows:

Eqn. 17 indicates that fraudulent accounting increases as technofetishism, democratic exclusion, and information asymmetry increase.

Eqn. 18 indicates that democratic exclusion increases as information asymmetry increases.

Eqn. 19 indicates that information asymmetry increases as democratic exclusion increases, and

- increases as technofetishism increases, if $\alpha > 0$,
- decreases as technofetishism increases, if $\alpha < 0$, and
- is independent of technofetishism, if $\alpha = 0$.

Recall from the previous section that in the original formulation, the Index of Imperial Domination can be any nonnegative number; that is, $D_{original} > 0$. In the Collaps-O-Meter, however, imperial domination is represented as a number between 0 and 1: $0 \leq D \leq 1$. We can say (perhaps appropriately) that in the Collaps-O-Meter, imperial domination is 'normalized'. To do this, one 'maps' the original space (the 'domain') of values 'onto' the new space (the 'range'). It is difficult (but not impossible) to map an infinite domain onto a finite range, so for the sake of simplicity we may be inclined to select a finite domain. But recall that the Index of Imperial Domination as originally formulated conveys what might be described as politically salient information around 1; because the IID is the ratio of external debt service expenditures to public health and education expenditures, a value of $D > 1$ indicates external debt service greater than domestic expenditures on education and health combined. The politically salient value in the normalized index will depend on the normalization process. Suppose we decide to normalize by selecting a finite domain $0 \leq D_{original} \leq D_{max}$, where D_{max} is the largest value in the set (3.7 in the plot above) and mapping it onto the interval between 0 and 1. Then the politically salient value becomes $\frac{1}{D_{max}}$; in any event, it is also between 0 and 1. We may wish to perform some other normalization procedure: for example,

we may wish to select the finite domain between 0 and $2D_{max}$, or some other multiple of D_{max}; alternatively, we may wish to select the infinite domain $0 \leq D_{original} < \infty$. In any event, we can find the politically salient value that corresponds to 1 in the original formulation. We can insert this value into the expanded formulation of the Collaps-O-Meter as $\hat{D}$. Then, following Eqn. 20 above,

- $\alpha > 0$ indicates external debt service expenditures greater than health and education expenditures combined;
- $\alpha = 0$ indicates debt service equal to combined health and education expenditures; and
- $\alpha < 0$ indicates debt service less than combined health and education expenditures.

This construction then encodes a belief that the ratio between external debt service and combined health and education expenditures is a good way to predict the political effects of widespread use of technology: it says in effect, "if government is captured by external interests, technology uptake will lead to greater economic inequality; if not, it can, depending on other factors, lead to greater economic equality." This is not too far from a mathematical articulation of Benkler's position, cited earlier, on "technological determinism"—

> Different technologies make different kinds of human action and interaction easier or harder to perform. All other things being equal, things that are easier to do are more likely to be done, and things that are harder to do are less likely to be done. *All other things are never equal*.[1] That is why technological determinism in the

[1] That is, *ceteris nunquam paribus*.

> strict sense—if you have technology "t," you should expect social structure or relation "s" to emerge—is false. [27] (emphasis added)

—although it does not represent the more subtle relation between 'technology' and 'society' offered by Latour ("technology is society made durable" [31]).

Note also that Eqns. 18-19 define a 'positive feedback loop': greater democratic exclusion leads to greater information asymmetry (Eqn. 19), which leads in turn to greater democratic exclusion (Eqn. 18). Feedback loops (both 'positive' and 'negative') are a common feature of models of this flavor.

Nobel laureate in economics George Akerlof and his Yale University collaborator Robert Shiller (one of two namesakes of the widely used Case-Shiller home price index), in what may have been the most anticipated monograph-length publication to come out of economics as a discipline in the first half of 2009, write, in the opening paragraph of their book *Animal Spirits: How Human Psychology Drives the Economy, and Why It Matters for Global Capitalism*:

> To understand how economies work and how we can manage them and prosper, we must pay attention to the thought patterns that animate people's ideas and feelings, their *animal spirits*. We will never really understand important economic events unless we confront the fact that their causes are largely mental in nature.
>
> It is unfortunate that most economists and business writers apparently do not seem to appreciate this and thus often fall back on the most tortured and artificial interpretations of economic events. They assume that variations in individual feelings, impressions, and passions do not matter in the aggregate and that

> economic events are driven by inscrutable technical factors or erratic government action. In fact, as we shall discover in this book, the origins of these events are quite familiar and are found in our own everyday thinking. [49] (p. 1; emphasis in the original)

Akerlof and Shiller are not quibbling about so much about the way in which, say, interest rates affect business investment, or fiscal policy affects unemployment; rather, they are saying that these are not the only things that matter; that the concepts that have populated previous efforts of academic economics to model 'the economy' (indeed, the world, or at least the parts of it that were taken to mattered) have been inadequate. New nouns, new entities, are needed. Akerlof and Shiller offer a few:

> This book will describe five different aspects of animal spirits and how they affect economic decisions—*confidence*, *fairness*, *corruption and antisocial behavior*, *money illusion*, and *stories*:
>
> - The cornerstone of our theory is *confidence* and the feedback mechanisms between it and the economy that amplify disturbances.
> - The setting of wages and prices depends largely on concerns about *fairness*.
> - We acknowledge the temptation toward *corrupt and antisocial behavior* and their role in the economy.
> - *Money illusion* is another cornerstone of our theory. The public is confused by inflation or deflation and does not reason through its effects.
> - Finally, our sense of reality, of who we are and what we are doing, is intertwined with the story of our lives and of the

> lives of others. The aggregate of such *stories* is a national or international story, which itself plays an important role in the economy. [49] (pp. 5-6; emphasis in the original)

This proposal would have been economic heresy not more than ten years ago, because the orthodox discourse of the discipline had no room for these entities; indeed, for entities that exceeded the rational actor, the calculating firm, the efficient market, and those other abstractions with which analysts and critics of economic thinking will already be familiar. Akerlof and Shiller's contribution to contemporary economic discourse is anticipated exactly by Haraway's "ontological questions":

> "What kind of world is this?" ...What sorts of entities exist here, and with what kind of relationality? What are the practices here? [30]

That is, Akerlof and Shiller's contribution is an ontological one: they have added new entities to the world of economic analysis. They suggest a 'something else' that might, in their view, be important "to understand how economies work."

Although it may be an important contribution, I do not believe that Akerlof and Shiller's model will be found to "explain fully and naturally how the U.S. economy, and indeed the world economy, has fallen into the current crisis" [49] (pp. 6-7), as they claim it does; that is, it will not be the last word on the psychology of economic life and its implications for policy, even within the discipline of economics. Rather, I suspect it will begin a series of ontological inquiries, renewing discussions and reopening questions thought closed within the discipline for centuries. The Collaps-O-Meter purports to perform a similar discursive maneuver; through it I suggest that peculiar and often 'subjectively' defined 'parameters' (like 'gratuitous domestic product',

'fortification', and 'statistical imaginary') might be important objects in the world of (political) economic analysis. Like (for example), "stories," a complex set of socio-techno-cultural artifacts and practices are imbricated in each ostensibly unitary 'parameter.' The model presented here, I suggest, is thus the kind of artifact that might play a role in the ongoing ontological reconstruction of our understandings of economic life.

Chapter 5

The epistemological crisis

What began as merely a 'financial crisis' and developed into an 'economic crisis' has in turn become what might be called an 'ontological crisis'. It turns out that it has simultaneously become an ideological and epistemological crisis. In October of 2008, as real estate markets across the United States collapsed, global stock markets crashed and volatility spiked, and liquidity evaporated as a contagion of fear spread throughout the banking system, each bank uncertain which of their peers would be the next to collapse, ex-"maestro" Alan Greenspan publicly declared intellectual bankruptcy on the floor of the United States House of Representatives. "Your view of the world, your ideology, was not working," California Representative Henry Waxman accused him. "Absolutely, precisely," replied the former Federal Reserve Chairman. "That's precisely the reason I was shocked," he continued, "because I have been going for 40 years or more with very considerable evidence that it was working exceptionally well" [3]. In April of 2009, Seth Freeman, a professor with appointments at the business schools of both Columbia University and New York University published an op-ed in the *New York Times* which began:

> Since I don't understand the economic crisis, I listen to lots of talk and go to lots of conferences about it. I still don't understand it, but my efforts have led me to some surprises: How tempting it is to say, "I understand," how hard it is to govern well, and how little anyone really knows. I've seen that the pretense of our scientific understanding of economics is false, which means that a hidden

> gift of the crisis is its insistent invitation to be humble and grateful, and respond accordingly.
>
> At a recent economics conference, one speaker confessed he'd been so humbled by the crisis that he'd stopped calling himself an economist. Almost none had seen the collapse coming. The field, he'd realized, simply didn't understand its subject. Later he asked a top economist: How do we know what to do about the crisis? What's the basis for our knowledge? The man placidly replied he thought his side was right. His response—or lack of one—echoed loudly. What do we really know? [48]

As of this writing, the Princeton University Press website for Akerlof and Shiller's book *Animal Spirits* comes up as the second hit on Google's results page for the query "animal spirits," after a website about shamanism [50]. In light of Freeman's account, the juxtaposition with shamanism hardly seems inappropriate: the explanatory and predictive powers of shamanism (and their epistemological grounding) seem to compare favorably with those of academic economics; conversely, the dogmatic conviction apparently inspired by academic economics compares well with that inspired by any venerable and long-lived religious faith. As the journalistic accounts discussed in previous sections highlight, doubt—that most reputable methodological tool of positivist knowledge-making since before Descartes—has become unprofitable ('disincentivized', we might say) among the makers of economic knowledge.

The purpose of this comparison is not to rehearse boring old performances of the 'science wars', or to build a wall around the citadel of 'science' or some other hypothetical 'reality-based community' characterized by ostensibly 'rigorous' methodological practices and epistemological groundings,

somehow unpolluted by the temptations of the 'outside' world, and to cast the economists out of the city with the barbarians and primitives. Rather: after acknowledging the instability, provisionality, and strictly limited generalizability of our (in this case economic) knowledge, we can begin meaningfully to ask questions about those limits, and to develop a generative relationship with our ignorance. We return to Freeman, who at this point in his discussion turns to literature for an understanding of his discipline's epistemological predicament:

> In "War and Peace," Leo Tolstoy describes a German officer so confident of his battle plan he believes only a fool could question it. Yet, it's so complicated, assumes so much, and so ignores human behavior that it's doomed. Still, the officer is set: If his plan fails, it'll be because lesser men failed to perform it.
>
> Tolstoy's thinking about war comes to mind a lot lately because it challenges our belief that we understand highly complex events. He argues war is too complex for anyone to fathom. Studies strongly suggest he's right: that wars occur in predictably unpredictable ways, like avalanches and stock market crashes. There may be many complexities we cannot fully grasp or control.
>
> This idea that the world is uncontrollable challenges ideals of the Enlightenment, which sought to master human behavior as physicists master the universe. We cannot. Isaac Newton himself discovered this truth when he got caught up in one of the first speculative bubbles—a craze involving stock of the South Sea Company. When the bubble burst, thousands took huge losses—including Newton. "I can calculate the motion of heavenly bodies," Newton said, "but not the madness of people."

> At one conference, a panelist confidently asserted we should let the banks fail. An audience member challenged him, explaining how she'd helped the government quietly and inexpensively solve an earlier bank crisis. The panelist seemed unimpressed. But her story told me we sometimes get things right, and that experience may matter more than theory in highly complex situations. [48]

Epistemologically, then, where do we stand? How are we to theorize our relation to our own (putative) knowledge, and make decisions in vast and complex systems whose responses to our own actions (let alone those of our governments' policy apparatus) we cannot fully predict, or perhaps know at all, even after they occur? One (perhaps reasonable) reaction is simply to chuckle, observe that the economists have finally caught up to Lyotard (or perhaps vice versa), and move on with our lives and our thick descriptions, close readings, microhistories, and so forth. But to turn willfully away from the examination of complex transnational systems—to refuse to engage the 'micro-to-macro' question—is, in a sense, to abdicate responsibility for understanding our own material conditions; it is to abjure the political. If, as Garrett Hardin suggested in his (much misappropriated) 1968 essay on "the tragedy of the commons", "the morality of an act is a function of the state of the system [in which it is performed] at the time it is performed" [51]—and possibly, by extension, of its effects on future states of the 'system'—then it seems to follow that even while we commit to a healthy skepticism of *theories*, we remain obligated to theor*ize*.

What kind of theorization? Given the epistemological constraints operating in the domain of 'economic' inquiry, it seems sensible to supplant 'analysis'—a reductionist technique involving the *lysis*, the breaking up, of an ostensibly unproblematically delineated 'whole' (which might in this case be taken as

'the economy', the object of analysis)—with, as Agre says in a different context, "something more like hermeneutics" [21]. In complex systems, 'agents' tend to act 'locally', but their actions sometimes have effects at scales beyond that which they can perceive directly—beyond the 'local'. In 'studying' the system sometimes called 'the global economy', the 'student' (a term I find preferable to, say, 'analyst') is necessarily also an 'agent': to use Haraway's terminology, they are *situated* somewhere specific in the 'system' under study. In fact, as von Foerster pointed out, situatedness within the system is a precondition for the ability to study it at all [13]. So we must begin where we find ourselves, and attempt to understand the ways in which local options, actions, *and interpretations* are conditioned by, and condition in their turn, the macroscopic state and trajectory of the 'system'—as well as our understanding of it. In attempting to understand the constitution of the 'local', our attention is directed 'outward', both spatially and temporally, and we encounter larger-scale phenomena that must be explained in turn by some combination of larger- and smaller-scale phenomena; that is, by the interaction of processes in and across other 'local[e]s'. The process is hermeneutic; applied to 'economic' phenomena, it seems reasonable to refer to such a method as 'economic interpretation'. Miscreant humanists Kavita Philip and Ackbar Abbas, in articulating "poor theory", offer an orientation relevant to this approach, and to the concerns of economists suffering from epistemological shock:

> Poor theory is less a theory than a way of proceeding.
>
> Poor theory proposes to find ways of making the most of limited resources.
>
> Poor theory uses the tools at hand to take the present to task. In the process it tinkers with theoretical technique and analytical object.

Poor theory suggests the need to work around intransigent problems, when clear solutions are not discernible and the means at our disposal are limited.

Poor theory reflexively re-encounters the history of theory through paying attention to the murky, unsystematic practices and discourses of everyday life. Poor theory is conditioned by reflexive imbrication with probable pasts and arguments with/about possible futures, and thus comes to see the present, too, as heterotemporal.

Poor theory proceeds not through tabula rasa, not by wiping the slate clean and starting afresh. That, to some extent, was the modernist aspiration, which has proven to be always costly and often undesirable. By contrast, poor theory proceeds through appropriations and improvisations, through descriptions that do not leave what it describes unchanged.

Poor theory is theory shaped by the fact that we are always confronted by objects and situations that are 'riddled with error' (Benjamin) and that outpace theory. Critically using the tools at hand, poor theory questions their construction and re-orients their practice. Tinkering calls for a tactical, recombinatorial, experimental ethic toward theoretical objects, and an historical analysis of the changing political contexts of intellectual inquiry.

Poor theory suggests not a resignation to epistemological futility but an openness to that which outpaces understanding. Objects of analysis present, in their contingency, in their being unsystematic, a degree of intransigence that frustrates mastery. The intractability of the object throws into relief the possibility of error in our methods.

[52]

Models reliant on economic rationality have always been haunted by its other—by the excess, the irrationality, immediately and patently apparent in any study of human 'economic' behavior. In economic models, as Philip points out, "remainders, residuals, and ghosts...have been problems to explain away, but in moments of crisis, they are mechanisms of bringing back fragments of meaning formerly discarded" [53]. It is (epistemologically) worse even than this: these excesses and residuals are posited as the fundamental driving forces, the explanatory factors for, crises and otherwise inexplicable phenomena. Consider for example a 2006 paper by economists Sharon Harrison and Mark Weder describing a quantitative model simulation in which "the residual from a regression from the spread on fundamentals is taken to measure nonfundamental confidence" [54] (p. 1328). Under certain model conditions, "changes in agents' expectations are self-fulfilling and therefore serve as a primary impulse behind [economic] fluctuations. [They] find that such a model, driven only by these measured [autonomous changes in agents' expectations, also called] sunspot shocks, can explain well the entire *Depression era*" [54] (p. 1328). The sources of these shocks are not modelled; they are simply represented. A reasonable interpretation of Harrison and Weder's claim, then, seems to be: the behavior of the US economy during the Depression era can be explained (only) *by phenomena that lie entirely outside the purview of economics as a discipline*. In economic theory 'sunspot shocks' and 'animal spirits' have come to serve a function analogous to 'idiopathy' in medicine, or 'the cosmological constant' and 'quintessence' in general relativity: they are 'fudge factors', wielded as necessary to bridge the gap between the predictions offered by the model as derived logically from disciplinary first principles and the apparent reality. These residuals can be

named and even quantified, but not, by and large, modelled or explained. This, I would suggest, is exactly what Akerlof and Shiller are up to in naming their 'animal spirits' ('confidence', 'fairness', 'corruption', and so on, as described in the previous chapter) [49] (pp. 6-7). This taxonomy seems hardly more than the proliferation, renaming, and clever (i.e., empirically—and perhaps more importantly, *intuitively*—plausible) arrangement of additional fudge factors: idiopathies, quintessences, cosmological constants. That is: if we follow Akerlof and Shiller, we will find that we are playing games and inventing categories, not "paying attention to the murky, unsystematic practices and discourses of everyday life" [52]; we are not doing "poor theory"—we are just theorizing poorly.

Is there space for, say, a 'poor systems theory', or, put another way, a practical approach to the study of complex systems which takes into account, even leverages, the limits of our own knowledge (the 'challenges' of 'critical mathematical modelling')? Does mathematical modelling have any place in this approach (or, more likely, set of approaches)? These experiments in 'critical mathematical modelling' highlight the flexibility of mathematics as language, modelling as thought technique, visualization as communicative apparatus, and simulation as medium for political and epistemological critique and world-making. These are not bad things to remember, or foreground, even if they are hardly novel results. They offer little consolation, admittedly, to the modeller interested in generating comprehensive (and especially predictive) models of complex 'social-ecological' systems, and confronted by the challenges enumerated in the discussion here. It is hoped however that this discussion serves to point simultaneously to the utility of quantitative modelling as a speculative method, and to its perils as a predictive method. Both, I suggest, have been historically under-emphasized.

Chapter 6

Conclusions

I offer the following conclusions about modelling, economics in the contemporary moment, and 'critical mathematical modelling'.

- Predictive modelling of complex social-political-economic-ecological 'systems' confronts a number of technical challenges:
  - **Ontological challenge.** What kinds of entities exist in the world (or 'system') being modelled; of these, which are most relevant to the phenomena under investigation? In modelling terms, what variables are required?
  - **Epistemological challenge.** How do we get enough data to make the predictions we want to make? How do we know when we have enough? If we are making predictions about the future, are we sure that we can make them only with data about the past?
  - **Challenge of performativity.** Sometimes theory acts on its 'object' (or, a model acts on the 'system' being modelled). This is because the theorist/modeller is necessarily a part of the object/system being theorized/modelled, even if they choose not to represent it in a way that makes this obvious. In fact, this is a precondition for theorization/modelling. A predictive model of a system must take into account the possibility that the existence of the model itself (within the system, as it necessarily is) will alter the behavior of the system.

  - **Challenge of objectivity.** Sometimes a model of a system tells us more about the modeller than about the system. To what extent this has occurred in any particular instance of modelling is often difficult to know, especially if the other modellers who are available for comment tend to think about the system in question in the same terms as the modeller whose model we wish to inquire about.

- Additionally, a modeller may be confronted with the challenge of examining the ethical or political implications of their model, especially in the event that it comes to be widely taken up and acquires some degree of performative force. Often this is not seen as a 'technical' modelling challenge, because the disciplines involved in modelling lack the language to apprehend ethical or political questions. Put another way, this is a technical question, but usually in another discipline.

- As of this writing, the understanding of economic activity and of the contemporary political-economic moment offered by economics as an academic discipline has failed to address all four of the technical challenges:

  - **Ontological challenge.** The kinds of actors that populate economic models do not, generally speaking exist in the world. Conversely, the kinds of actors that populate the world that economic models attempt to represent possess properties and develop agencies that are important to understanding economic activity but are omitted from economic models.
  - **Epistemological challenge.** Given the entities populating current economic models, it appears that the past is not a useful guide to the future. Economists, however, persist in acting as though it is.

  - **Challenge of performativity.** Economic theory has shaped and continues to shape the world that it now measures. Economic theory includes no theory of its own action on the world, however.
  - **Challenge of objectivity.** Economic theory has been radically historically determined. Economic theory includes no theory of its own historical development, however.

- Economic theory purports to be value-neutral, non-normative, apolitical, and scientifically objective. This has turned out to be false; rather, economists have simply resisted apprehending the ethical and political implications of the performativity and historical contingency of economic theory.

- The current crisis of the global *economy* can be usefully read as simultaneously a crisis of *economics*; in particular, the discipline lacks a model of the contemporary economic configuration that is ontologically or epistemologically adequate to the task of generating predictions of the future which are accurate to within a margin of error that can be bridged with empirically and intuitively plausible fudge factors. That is, the economic crisis is also—indeed, perhaps *fundamentally*—an ontological and epistemological crisis.

- For positivist knowledge production, ontological uncertainty, partiality, and situatedness are liabilities. **Critical mathematical modelling relies on these phenomena**, leveraging them to engage in a different kind of discursive action which is *ontologically imaginative*, *epistemologically opportunistic*, *intentionally* and *strategically performative*, *materially embodied and socially embedded*, and actively and openly engaged with—indeed, often primarily concerned with—the ethical and the political.

- The epistemological challenge of positivist modelling creates a condition in which more data is always better. Critical mathematical modelling resists this panoptic impulse by making use of the data at hand to *explore* (e.g., Bribe Payers Index; Index of Imperial Domination; Capitalism Collaps-O-Meter), *critique* (e.g., Index of Imperial Domination; Miles Per Casualty), *foreground* (e.g., Miles Per Casualty), and *re-imagine* (e.g., Capitalism Collaps-O-Meter) existing ontologies, relations, and practices. It is a socially situated practice of discursive action, political engagement, speculation, and worldmaking that makes use of a combination of qualitative, formal, and qualitative ways of thinking, seeing, and representing.

- Finally, critical mathematical modelling can be allied with a practice of 'economic interpretation'. Rather than attempting to explain particular relations, events, or patterns as examples of general laws, or large-scale and long-term trends and 'structures' as simply the emergent result of the preferences and decisions of individual, autonomous actors, economic interpretation tries to apprehend the circular, mutually constitutive relations between events and 'agents' at multiple scales, both in space and in time.

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