Part I

Framing the Debates
The Inadequacy of Representation

In *Representing and Intervening*, a monograph dedicated to break away from the epistemic conundrum of representation in science, Ian Hacking narrates a fantastic fable.\(^1\) He starts by assuming the role of a nineteenth-century anthropologist laboring to discover the origin of the distinction between reality and representation. Many anthropologists have conjectured before that the essence of human is to be sought in her unique ability to speak. “It has been urged,” Hacking writes, “that rationality, of its very nature, demands language, so humans as rational animals, and humans as speakers are indeed co-extensive.”\(^2\) Instead of the popular narrative, he chooses to see his protagonist not as *homo faber*, but as *homo depitor*, a species that can represent, that can create likeness amidst the wilderness of unimaginative babble. He wants his readers to imagine “pictorial people making likenesses before they learn to talk,” and fiddles with the idea that “people invented language out of boredom” – by telling jokes to pass the long evenings once fire was invented.\(^3\) Soon, the *homo depitor* started to use sounds pointing to the “realness” of the clay figurines she had sculpted. Others joined the discussion with a spree of dissent: “no, not that, but this here is real instead.”\(^4\) Hacking argues that this story offers a more characteristically human narrative about the origin of language in connection with

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representation. From the very beginning, he claims, the discourse of representation touches upon the question of its reality, its second-order doppelganger: “This is the concept of reality, a concept which has content only when there are first-order representations.”

This view does not corroborate the idealist fantasy that nothing real exists before its representation, but postulates the concept of reality as secondary to that of representation. The conjoined concepts of reality and representation, however, become more intricate when alternate styles of representation come into being, creating a puddle of confusion as to which version of reality should be accepted as the true one. Apparently, there is no way to decide which of them among many is more suitable for explaining the world, and conversely, there is no way to decide which of the corresponding worlds one should choose to represent in the first place. This entails a crisis in science in any discipline that nurtures the ambition of attaining “scientific” status by imitating and imbibing the spirit of logico-deductive reasoning. Economics is surely one of them, or at least it claims to be. “Economics as a discipline is in crisis,” proclaims Alfred Eichner in his introduction to an anthology entitled ‘Why Economics is not yet a Science.’ This statement is elucidated in the concluding essay of the anthology, which – authored by Eichner himself – is titled after the name of the book. The crisis of the discipline, he thinks, has resulted from its reluctance to offer reasonable solutions to problems such as unemployment and inflation. This hasty indifference to the affairs of the “real world” has denied economics the prestige of scientificity. Eichner’s essay subscribes to an archetypical understanding of science in terms of a “presupposed distinction between empirical reality and theoretical form.”

Alain Badiou’s The Concept of Model provides a precise and insightful analysis of the “ideological formation,” which, by producing this image of science, makes way for two apparently conflicting discursive configurations – empiricism and formalism. In empiricism, Badiou argues, “the effective presence of the object” – its reality – is assumed to be the dominant element, and in formalism, “the mathematical code in which the present object comes to be represented” gets all the attention. Thinking in terms of such a dichotomy is the constitutive logic of what Badiou describes as the

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5 Ibid, 136.
7 Alfred Eichner, “Why Economics is not yet a Science” in ibid, 205-41.
9 Ibid.
“bourgeois epistemology.” Empiricism or formalism alone does not constitute bourgeois epistemology; it is the handiwork of “the ensemble of notions by which we designate, now, their distinction, and now, their correlation.”

In different disciplines, the duality assumes different forms. In a Lévi-Straussian structural anthropology, “the empiricism-formalism couple assumes the form of an opposition between neutral observation of facts and the active production of a model.” A model is defined in this discursive formation as “an artificial object whose purpose is to reproduce the real object, imitating it in the law of its effects.” In one of his short essays explaining “the meaning and use of the notion of model,” Lévi-Strauss himself cautions us against “mistaking a theoretical construction for a description of actual facts.” A theoretical construction is, however, fundamental to any structural analysis. As Lévi-Strauss candidly puts it, a structural analysis does not change the way we perceive “concrete social relations,” but explains them better by excavating the “unconscious categories” from a previously neglected level of investigation deeper than that of mere empirical observation.

The level can only be reached by “bringing together domains which, at first sight, appear disconnected to the observer.” In Badiou’s formulation, the activity of bringing together (or assembling) apparently disconnected domains of analysis is nothing but an instance of “active production of model.” This assemblage is performed “in such a way that the characteristic opacity of the real is absent from it.”

Eichner’s appraisal of the economic discipline, presuming the dichotomy between empiricism and formalism, rests its faith on the former perspective. In that sense, he is a realist, who believes that the effective presence of an object is ensured by the meticulous ritual of empirical confirmation – verification of a theory by observation from the real world. However, to his dismay, the study of economics has more often than not treaded the other way.

Constructing theoretical models with a formal system of axioms is a regular feature of economic enquiries. Gerard Debreu, in one of his essays written in defense of mathematical economics,

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10 Ibid.
11 Ibid.
12 Ibid, 10.
13 Ibid.
15 Ibid, 80.
16 Ibid.
18 Ibid.
recounts how the publication of the *Theory of Games and Economic Behaviour* by John von Neumann and Oscar Morgenstern in 1944 encouraged a host of young economists to deduce the principles of axiomatization of the discipline:

> According to this schema, an axiomatised theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the primitive concepts, of the assumptions, and of the conclusions of the model, its bare mathematical structure must still stand.\(^{19}\)

Most of Eichner’s diatribes are directed against this axiomatic mode of reasoning, although his interpretation of the crisis is not as nuanced as the one offered by Hacking. His discomfort with the mathematical orthodoxy, however naively, takes into consideration an important factor in any scientific investigation – the relation of the theoretical framework with the “outside world” – the world outside its constitution, where the proverbial reality resides and makes its presence felt. In case of economics, it is imperative to study this outside in detail, chiefly because it is expected from the discipline not only to make passive observations about reality, but to take useful and effective measures to change it, to mend its structural incongruities, to intervene for good. Even though Eichner’s preferred methodology does not explicitly allow him to go further than attaining a mimetic representation of the said outside, his intention remains to participate actively in the process of engineering the same as a potent field of intervention. As we have seen in case of Lévi-Strauss, the act of bringing together several seemingly disconnected domains of analysis for excavating the “true” structure of concrete social relations is considered the task of the model-builder. If the idea of a concrete outside placed next to the theoretical framework coded in terms of mathematical signs gives the impression of a horizontal arrangement of intervention, the actualization of the real by digging deep into the abyss of truth refers to a vertical one. In any case, the act of modeling is an act of intervention, vertical or horizontal. If for Eichner, the “meaning” of modeling is to bring the anomalies in an economic system – a national economy or a stock market – into order, for Lévi-Strauss, it is “to reproduce the order in which the information [gathered from the field] assembles itself.”\(^{20}\)

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If we shift our focus from the concept of model itself to that of “modeling,” we will arrive at a different register of analysis with different epistemic problems. After writing off representation as a predicament to the realization of scientific truth, Hacking adopts the idea of experimental intervention as a more effective mode of investigation about science and its reality. “Reality has to do with causation and our notions of reality are formed from our abilities to change the world,” he writes at the end of his anthropological fable.\textsuperscript{21} This argument is particularly valuable for economics, for which “the abilities to change the world” are assumed to be connected essentially to its epistemological situation. Economics is perhaps the only discipline, which openly admits of attaining scientific status by articulating a theory of intervention. In this context, the study of modeling as a practice concerned with changing the world may induce the possibility of locating the strategies by which it shapes the world before (and after) changing it.

MODELS: MATERIAL AND METAPHORICAL

The shifting of focus from the concept of model to that of modeling points to the diversity of practices that can be grouped together under the label of a model: “wooden ships and plastic molecules, wax embryos and a perspex economy, monuments in cork and mathematics in plaster, casts of diseases and displays of stuffed animals, anatomies to take apart and extinct monsters rebuilt in bricks and mortar.”\textsuperscript{22} All these models, however different in their materials of construction and modes of application, have one thing in common: an additional dimension from the theoretical models that we have discussed in the last section – the third dimension. The practice of using three-dimensional models in economic studies, especially in classroom teaching, is not as old as in many other disciplines like Physics or Chemistry. Mary Morgan and Marcel Boumans in a study of one of the most popular 3-D models in economics – the Phillips machine – show that it was as late as in the late 1940s when it was invented and named after its creator Bill Phillips in Great Britain to demonstrate and explain the working of a Keynesian macroeconomy “by flows and stocks of colored water in a system of perspex tanks and channels.”\textsuperscript{23} It was not the first instance of a physical

\textsuperscript{21} Hacking, \textit{Representing and Intervening}, 146.
model being used to represent a so-called economy. Timothy Mitchell has mentioned that, even in the 1890s, there were instances of conceiving and explaining economic phenomena through mechanical models. Originally developed as his doctoral dissertation, Irving Fisher’s model of an economic market consisted of “a network of cisterns, levers, pipes, rods, sliding pivots and stoppers, through which the flow of water represented the working of the principle of utility.” There were other models by many economists, some simple and some extremely complex, which made use of the same machinic analogy with the working of an economic system, although, Mitchell argues, in the 1930s, a shift occurred in terms of conceptualizing the dynamism of a totality of economic practices and relations. In the previous models, the working of an economic entity represented by the movements of the rods and levers within a static apparatus could not be imagined as part of a dynamic reality. The solution came in the form of reconceptualization of the structure of economic totality by demarcating the exterior and interior of the structure and re-structuring of the interior as a complete network of economic actions. Mitchell’s larger claim is that the conception and institution of the notion of “economy” as the totality of economic relations and practices did not predate the shift in the machinic paradigm of economic representation; rather the economy was invented through these analogical practices.

The Phillips machine in Morgan and Boumans’ essay belonged to the second generation of dynamic models. They claim that Phillips designed his model to overcome a usual problem with the mathematical models of Keynesian macroeconomics – the discrepancies arising from using static equations to explain a dynamic system. As a strategy to have a better understanding of Keynes’ General Theory, he took recourse to an analogy previously made by Kenneth Boulding between economic and hydraulic ideas: “It provided for a circular flow of national income, with the relationships between the elements in the economy to be represented by tanks of liquid with in- and out-flows, and by valves governing the stocks and flows.” Morgan and Boumans argue that this shift from a two-dimensional to a three-dimensional model evinces certain constraints and commitments, the former on the physical or the material side of the model, and the latter in terms of being faithful to the economic principles that it seeks to represent. The size, shapes, and relative positions of the tanks, for example, have to be in perfect correspondence with the exact economic mechanisms that the model is trying to explicate. This element of “materiality” resulting from

25 Ibid.
interactions between various constraints and commitments leads to speculations about an interdisciplinary trend in economics starting from the late nineteenth century. From the description of the range of models used in other disciplines, it is evident that the element of materiality has been an important consideration for modeling in most of these disciplines. On the other hand, in economics, even the abstract models came to be a dominant form of representation, as we have seen before, only in the mid-1940s. Even if there was a formal system of reasoning based on mathematical calculations and an axiomatic structure, it was seldom called a model: most of the times, it was called a “theory,” for example, the Ricardian theory of rent, or the Marshallian theory of demand and supply.

Modeling, therefore, comes in a common plane with other scientific disciplines on the point of this concern with materiality. It also points to the multiplicity of the practical aspect of an economic model, one of which is to relay the connections between economic theories and the world by conceiving “theoretical models” – presently the most popular form of modeling in economics. Mary Morgan and Margaret Morrison offer a fresh perspective on the use of theoretical models in various disciplines.27 In their essay on the instrumental role of models in mediating between the theory and the world, they illuminate on various instances of how the models function as “instruments of investigation” to the end of attaining certain “autonomy” – a partial independence from both the edicts of theory and the norms of correspondence with the world.28 Although, to some extent, the autonomy of a model is attained during the very process of its construction, not much about it is available in the otherwise exhaustive literature on the subject. “Some might argue” Morgan and Morrison add, “that it is because modelling is a tacit skill, and has to be learnt, not taught.”29 The absence of guidelines for building models in scientific texts also suggests that, at least to some people, it remains as a highly creative activity, and hence, “not susceptible to rules.”30 In the same volume, however, Marcel Boumans offers an excellent description of the complex and eclectic process of model-building in economics.31 Economic models can be of various types. Apart from the “properly theoretical or mathematical ones” favored by Debreu in his defense of formalization,

29 Ibid., 12.
30 Ibid.
these models also present “non-spatial processes in a synthetic fashion” in the form of graphs and diagrams – a practice quite popular among the peers of mathematical economics. In Boumans’ account, theoretical models have to address many concerns simultaneously: “models have to meet implicit criteria of adequacy, such as satisfying theoretical, mathematical and statistical requirements, and be useful for policy.” Hence, to construct a successful model, one has to integrate a large number of items – theoretical notions, policy considerations, mathematical entities, and empirical data. The diverse elements are molded into a mathematically expressed formal system, which forges and explicates connections among different variables under study. Morgan and Morrison find in this description a confirmation of their own thesis of autonomy. Since the models have to assemble various other elements than theory and empirical data – sometimes even from outside the domain of original investigation – they “embody an element of independence from both theory and data.”

The autonomy, Morgan and Morrison argue, is a necessary precondition for the models to mediate between theory and the world. They start with the observation that testing the validity of theories in terms of empirical observation requires mutual independence between the respective domains of theorization and verification. Then they extend the logic to the case of models as instruments of investigation: “we can only expect to use models to learn about our theories or our world if there is at least partial independence of the model from both.” But there must be some connection between the two as well; otherwise no conclusion derived from the models would seem legitimate. Since the models are assumed not to be situated inside the “hierarchical structure between theory and the world,” they are accepted as the perfect instrument of mediation between the two. But how do the models function as instruments of mediation? Sometimes they are used as instruments to build a theory; sometimes they are employed to explore and correct a theory already in existence. Most of the times they are utilized to construe the implications of the theories in concrete situations. Much of the model-building in economics is performed with this prospect in mind.

36 Ibid.
37 Morgan and Morrison here refer to the role of the conception of “rational economic man” in exploring both the “theoretical implications of the most single-minded economizing behavior” and “accounting for the divergence between the observed behavior of real people in experimental situations and that predicted from the theory of such a model man in the same situation” (*ibid*, 20).
The performance of the models as instruments of mediation, however, produces knowledge about both the theory and the world. The production of knowledge happens at two points in the process of modeling: once during the construction of the model; and once when it is put to a specific use. Constructing a mathematical model on the basis of observed data provides the opportunity to learn the “two-fold process of conceptualizing both evidence and available theories into compatible terms.” One process involves interpretation of the observations in a form conducive to modeling, and the other incorporates adjustments in the original equations from the theory to envelop those interpretations. Thus both the theory (the sets of equations) and the world (the observed data) make themselves available to a process of knowledge production. Same thing happens, when a model is required to function either as an instrument of measurement, or as a technology of intervention. Again the production of knowledge assumes a two-step procedure: “when we manipulate the model, or calculate things within the model, we learn, in the first instance, about the model world – the situation depicted by the model.” But then, “learning about and from the model’s own internal structure provides the starting point for understanding actual, possible, and physically impossible worlds.” The purpose of developing econometric models, for example, is not only to explore theory, but also “to explore past and future conditions of the world and perhaps to change it.”

As Morgan and Morrison indicate, the preliminary function of the economic model is to “explore the implications of the theories in concrete situations.” This view seems to impose a responsibility on theory, not only to fare well in the test of empirical validation, but also to successfully perpetuate technologies of intervention for controlling and manipulating the existing conditions of the world. The world is characterized by concreteness as opposed to the speculative domain of theoretical abstraction. Thus if the world is afflicted by a “problem,” the theory has to work out some effective strategy to salvage it from the distress. Boumans, on the other hand, criticizes the theory of models as instruments of intervention as “too one-dimensional.” According to him, the process of model-building calls for integration of several other elements, such as metaphors, analogies, mathematical concepts and techniques, policy views, and stylized facts. As a result, the model is expected to meet a number of a priori criteria of quality. Whatever be the function of the model, it transforms the

39 Ibid, 33.
40 Ibid.
41 Ibid, 35.
42 Ibid, 19.
ingredients into an integrated mathematical framework, which contains even “the generic facts that the model is supposed to explain.” In this account, the data supposedly collected from the world of concrete situations are always-already embedded in the structural process of model-building, leading to what Boumans prefers to call the practice of “built-in justification.” The self-sustained practice of justification envisions the problems accruing in the outside world as extensions of the set of formal axioms situated in the model. Offering solutions to these problems, therefore, becomes just a formality. Boumans’ theory of built-in-justification refers back to an old critique against economics—that of tautological reasoning. “The word “tautology” has become quite a stock weapon in the discussions of economists for bludgeoning unpopular theories,” observed Terence Hutchison in 1935. He defined the term as “an analytic proposition which cannot conceivably be false because its truth is assured by the, in a certain sense arbitrary, process of assigning definitions.” Hutchison noted that the validity of a tautological argument did not have to be guaranteed by data observed in the real world. Only a “manipulation of symbols” was required to maintain the formal consistency of the system. The tautology in economic theory, however, is inevitable due to its dependence on mathematical deductions. “When mathematical reasoning is applied to problems outside of mathematics itself,” John Blatt comments discussing the “misuse” of mathematics in economic theory, “then the results are no better than the initial assumptions.”

There can be two ways to overcome the circularity of economic reason: one, by resorting to an inductive method of enquiry, which would acknowledge nothing but empirical confirmation as the true and effective means of knowledge production; and two, by proposing a theory of model, which would exploit the very notion of tautological reasoning to achieve a “functional understanding” of the world. Allan Gibbard and Hal Varian take a good look at the second option in their paper published in 1978. “The triumph of model building can evoke suspicion,” they confess at the beginning. “Do they tell anything about the world of genuine people, work, production, and

44 Ibid, 95.
45 Ibid.
47 Ibid.
48 Ibid.
49 Ibid.
They attempt to answer this question by defining the model as a “story with a specified structure”:

The structure is given by the logical and mathematical form of a set of postulates, the assumptions of the model. The structure forms an uninterpreted system, in much the way the postulates of a pure geometry are now commonly regarded as doing. The theorems that follow from the postulates tell us things about the structure that may not be apparent from an examination of the postulates alone.

The authors agree that the structure itself is often confused with the model, but it is important also to take account of the story, which explains the seemingly “uninterpreted predicates, quantifiers, and the like” assembled in the structure. Not only are they aware of the fictive dimension of any model, they also emphasize it by calling it a story, whose reality makes its appearance only in the structure that it seeks to interpret. “Indeed,” Gibbard and Varian write, “the only statements of most applied models in economics that are true exactly are truths with no empirical content, such as definitions and mathematical truths.” Empirical confirmation, therefore, does not make a model more useful or applicable than others. It depends on how “efficiently” the model can serve the purpose of its investigation, which is to predict rationally expected consequences of certain economic phenomena like rise in money supply or fall of interest rate. In this context, the authors inform, one has to distinguish between general and applied models:

whereas a model speaks of entities of certain general kinds – prices, consumers, information, and the like – without saying which particular entities in the world they are, an applied model specifies the particular classes of entities it treats.

Applied models are deemed more suitable for particular economic situations, while the general ones are required to predict economic behaviors as universal phenomena. The theory of consumer’s choice or the theory of firm is believed to hold water all over the world for every individual consumer or firm, but an applied model has to specify which particular individuals or firms it wishes

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51 Ibid, 664.
52 Ibid, 666.
53 Ibid, Cristina Bicchieri, for example, defines a model only as “a set of assumptions about a system attributing to it a given structure, so that many of its properties are explained by reference to this structure” [Cristina Bicchieri, “Should a Scientist Abstain from Metaphor?” in Arjo Klamer, Donald (Deidre) McCloskey, and Robert Solow (eds.), The Consequences of Economic Rhetoric (Cambridge, New York: Cambridge University Press, 1988), 107].
55 Ibid, 667.
to study, thus giving its “predicates particular extensions, its quantifiers particular domains.”\(^\text{56}\) The applied models feature in Gibbard and Varian’s paper more prominently than do the general ones, as they offer a better opportunity to speculate on the reality of the structure and the accuracy of the interpretations.

The authors dismiss the empiricist hypothesis that “assumptions of [an economist’s] applied model” should be “true of the situation” as “naïve,” since it is “preposterous” to imagine such perfection of exactitude.\(^\text{57}\) The validity of a model as an “explanatory approximation” is not contingent on how well it represents the reality of any situation, but in offering a version of it “real enough” to make sense of the predicted behavior – the “fact of their approximate truth.”\(^\text{58}\) The assessments based on proximity to truth adopt a tautological framework, where it is assumed, first:

if the assumptions of the applied model were true, the conclusions would be – here the proof is mathematical. Second, the assumptions in fact are sufficiently close to the truth to make the conclusions approximately true.\(^\text{59}\)

Gibbard and Varian criticize Milton Friedman’s view that the applicability of a model to a particular situation depends on the approximate trueness of the conclusions only. “According to us,” they argue, “something further is hypothesized: that the conclusions are sufficiently close to the truth because the assumptions are sufficiently close to the truth.”\(^\text{60}\)

Gibbard and Varian’s paper has been criticized by many economists and philosophers of science. Alexander Rosenberg, for example, points out that the erroneous assumptions of an unrealistic model must be corrected in view of new facts, by relaxing earlier restrictions.\(^\text{61}\) Rosenberg seems to believe in cumulative progress of science, where a new model is considered better than the old ones, if it supersedes them by becoming more complex in terms of representational capacity:

The problem is that if the economist’s model is supposed to explain something about the world, then it is to be expected that in assessing its explanatory force, the economist should

\(^{56}\) Ibid.
\(^{57}\) Ibid, 668.
\(^{58}\) Ibid, 669.
\(^{59}\) Ibid, 670. Authors’ emphasis.
\(^{60}\) Ibid, 671. Authors’ emphasis.
be engaged in the very tasks that Gibbard and Varian deny him: formulating and testing hypotheses, and establishing laws that reflect the confirmation of his models.62

The empiricist-realist argument is further endorsed by R. M. Dancy, who claims that the weakness of economic models lies, not in their approximate or selective assumptions, but in the fictional ones, which also deny the discipline the coveted scientific status.63 Both Rosenberg and Dancy overlook Gibbard and Varian’s insistence on the functional aspect of economic models, which is to purport an understanding of the “real world” by exploring causal relationships between the formal structure and its analytical extensions.

Mary Morgan, on the other hand, offers a more substantial criticism of the theory by drawing attention to the element of “story” in a model.64 Morgan points out that, although Varian and Gibbard claim to consider the story as an essential component of a model, they hardly define it in clear terms. She, on the other hand, proposes an alternative theory by arguing that the “story” is to be sought in the “counterfactual” character of theoretical models: “They are counterfactuals in that they begin, as they say, with assumptions, which are known to be unrealistic – counter to the facts.”65 This notion of “counterfactuality” can be described as another variation of tautology, but Morgan aligns it with a more pointedly literary analysis. She defines the structure as a static “metaphor” in a historical and time-based story and argues that a model needs both at the same time to be “used” in the real world. In a counterfactual model, the changes in the narrative are initiated by a what-if question – “what happens if...?” – proposing an event which brings about a chain of reactions in the story. By “using” the model, Morgan means “using it to answer questions that and this involves telling stories.”66 She relates this concept to the rhetorical school of economic methodology championed by Deidre McCloskey.67 After the publication of her Rhetoric of Economics

63 R. M. Dancy, “Model Behavior”, Journal of Philosophy 75, no. 11 (November, 1978), 677-679. A Similar critique is found in Emmanuelle Benicourt and Bernard Guerrien, “Is Anything Worth Keeping in Microeconomics?” Review of Radical Political Economics 40, no. 3 (Summer 2008), 317-23. Margaret Schabas feels the same way, when she writes that the economist’s “fairy story falls far short of capturing the complexities of the real world” [Margaret Schabas, “An Assessment of the Scientific Standing of Economics”, PSA: Proceedings at the Biennial Meeting of the Philosophy of Science Association 1986, Volume One: Contributed Papers (1986), 302]. She also observes that Gibbard and Varian provide a “less critical appraisal” of neoclassical economics, which is held to be the most daringly “unreal” branch of the discipline (ibid, 300).
65 Ibid, 363.
66 Ibid, 369.
in 1985, the deployment of literary strategies like metaphor and analogy was accepted by many economists and literary theorists as an integral part of economic analysis. But Morgan’s insistence on the practical use of models in real world points to another trend in academic circles – to extend the logic and the modalities of economic reasoning in other areas of life than that of the economic. Over the last century, the participation of professional economists in governance of economic lives of actual people has increased remarkably, making possible what some consider “the move of economics from the journals, textbooks and lecture theatres into “the real economy.” The real economy, at the same time, is being reshaped, produced, and engineered by these practical interventions together with their theoretical counterparts: “The issue that needs to be tackled in relation to economies and economics is not just about “knowing” the world, accurately or not. It is also about producing it.”

In recent time, a group of economists and sociologists led by Michel Callon have introduced the notion of “performativity” in studying economics not merely as a “form of knowledge that depicts an already existing state of affairs,” but as a “set of instruments and practices that contribute to the construction of economic settings, actors, and institutions.”

In the next section, I shall discuss the basic contours of the theory of performativity in Callon’s work and try to find out how it may help to underscore the role of modeling in economic performances.

THE ECONOMIC PERFORMATIVE

There is a growing awareness among economists, Callon has observed, about the intermingling of economics and the “economy” as a site of predetermined, yet dynamic, rationalities and institutions.


70 Ibid.

71 Ibid, 4.
This raises a fundamental question about the relation of a discipline with the object of its study: “Is it reasonable to consider that a scientific theory can alter the nature of the object that it describes?” While some economists believe that the purpose of their discipline is to describe the reality of an economic situation by devising concepts and theories from an “objective distance,” others advocate for a more participatory approach, viewing the economist as an innovator, engaged in shaping and reshaping the world by initiating technologies of intervention. Callon has introduced the notion of performativity to dissolve the difference between these two extreme positions. According to him, a discourse is performative, “if it contributes to the construction of the reality that it describes.” To understand the performativity of economics, one has to turn to a discourse analysis that focuses on “rhetoric” implying “relationships of entanglement between propositions and their referents.”

The inspiration of an analysis of performativity based on rhetoric comes from J. L. Austin’s definition of “performative utterance” as “a specific kind of statement or expression that establishes its referent through the very act of uttering.” Callon informs that Austin’s contributions to linguistics make it possible to imagine a distinction between “constative” statements that seek to describe an “outside object” already in existence, and the performative utterances, which produce the reality of the object by simply describing it to exist. “By proposing this distinction and by showing the diversity and large number of purely performative statements,” Callon argues, “Austin wanted to criticize the idea that the function of language is essentially representative.”

The idea of the performativity of language is associated with pragmatism – a philosophical tradition started by the mathematician Charles Pierce, who was deeply invested in the concepts of reality and scientific truth. Pierce believed that repeated measurements of a phenomenon would drive away the errors and reveal the truth of nature. “Pierce did not think that truth is correspondence to the facts,” Hacking explains, but to an assortment of “stable conclusions” obtained through experiments by a

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73 Ibid, 316.
74 Ibid.
75 Donald Mackenzie, Fabian Muniesa, and Lucia Siu, “Introduction”, 2-3. Mackenzie, Muniesa, and Siu give examples of such utterances: “In saying, for instance, “I apologize,” I am not reporting on an already existing state of affairs. I am bringing that state of affairs into being: to say “I apologize” is to make an apology. “I apologize” is, this, a performative utterance” (ibid, 3). The judge’s verdict of “sentencing” somebody to imprisonment or the vow of marriage at the alter are also instances of performative utterances [Callon, “What Does It Mean to Say That Economics Is Performative?”, 317].
The pragmatists of later generations like John Dewey held a different view on these concerns: “Dewey gave us the idea that truth is warranted acceptability.” It is reached by addressing to our immediate needs and desires – by way of doing things, not thinking idly. Although the emphasis Pierce put on finding a stable conclusion after repeated experiments was taken to be a false hope, his idea of a community of doers was preserved in the process of attaining acceptability. Richard Rorty, one of the most ardent followers of Dewey, was attracted to this idea of instability or relativity of truth. For him, as Hacking puts it, reason is “whatever goes in the conversation of our days, and that is good enough.” Based on Dewey’s idea that the world and its representations were social constructs – society being a universal expression of the community – Rorty defined reality as “whatever we agree on.” Austin’s critique of the representative function of language followed from these developments in the pragmatist school, which forced him to argue that “there is no language; there are only acts of language.” Every statement, even the constative ones, therefore, constitutes the “context” of its functioning.

This view is also shared by Deidre McCloskey’s rhetorical analysis of economic arguments: “All the conversational devices of economics, whether words or numbers, may be viewed as figures of speech.” The debt to Rorty in this statement is evident. Arjo Klamer, another proponent of rhetorical analysis, argues with the same spirit, “Economics is a conversation, or better a bunch of conversations, and economists are economists because they are in conversation with other economists.” Klamer clarifies that he is using the term not in its colloquial sense, but following its use in Rorty’s Philosophy and the Mirror of Nature (1979), denoting association with people or an object of study. The objective also remains the same: “A rhetoric of economics would be a way of showing how the science accomplishes its results.” Contemporary neoclassical economics, however, Philip Mirowski observes, accumulated its strength by adhering to “the trappings of a

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77 Hacking, Representing and Intervening, 60.
78 Ibid, 61.
79 Ibid, 62.
80 Ibid.
84 Ibid, 19. McCloskey also cites Rorty as an authority on rhetoric and its importance in everyday negotiations: “Richard Rorty had it more right: “It is pictures rather than propositions, metaphors rather than statements, which determine most of our philosophical [and economic] convictions”” [McCloskey, The Rhetoric of Economics, 40].
Cartesian world view” that facilitated a decline of literariness and the advent of axiomatization. The irony of a rhetorical analysis, he explains, lies in the contradictory impulses of disavowing this history of exclusion and exposing the metaphorical nature of economics. “The point of thinking about economic conversations,” claims McCloskey rather innocently, “is to help the field mature, not to attack it.”

Her unwillingness to critique the discipline for its blatant ahistoricism, however, prevents McCloskey from seeing how the avalanche of metaphors in economics was resulted by the increasing use of mathematics as a mode of transfer of analytical frameworks from one scientific discipline to another. It was by the same logic, “utility,” the cornerstone of neoclassical economics, was analogically equated with “potential energy of mid-nineteenth-century physics.”

Another disadvantage of the rhetorical analysis proposed by McCloskey and Klamer is its insistence on the representational idioms of theorization. McCloskey’s idea of models points to her fascination with the metaphor and its capacity of representation:

Every step in economic reasoning, even the reasoning of the official rhetoric, is metaphoric. The world is said to be “like” a complex model, and its measurements are said to be like the easily measured proxy variable to hand. The complex model is said to be like a simpler model for actual thinking, which is in turn like an even simpler model for calculation.

Her conception of “models as metaphors” invokes the tautological framework of reasoning, where even the “world” is incorporated within the analysis as nothing more real than a “complex model.” The linkages between structuring a model and using it in a real situation are not paid much attention. As a result, it maintains a silence as to how the equation between the world and a complex model is inscribed, and how its actual measurements are assumed to have similar application like those of certain “proxy variables.” In a sense, it inherits the self-inflicted tautology of mathematical economics, and leaves it unquestioned, in spite of its desire to improve the discipline by pointing to

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its literariness. Callon tries to dodge this trap by referring to the entwinement of action and description in any scientific statement:

When I say “this thread breaks”, I am referring to all the actions that cause my statement to be true, to actually happen (or not). It is because the statement describes a singular course of action still to happen – and not a preexisting word [sic] out there – that it is performative.⁹⁰

By not privileging description over action, or vice-versa, Callon avoids the epistemic predicament of representation and proposes a theory of intervention that would materialize through sociotechnical agencements. Agencement, he informs, is a French word, whose exact translation would be “assemblage” or “arrangement”: “It conveys the idea of a combination of heterogeneous elements that have been carefully adjusted to one another.”⁹¹ Callon favors the original French word to its translation, as the English counterparts might designate a “sort of divide between human agents (those who arrange or assemble) and things that have been arranged.”⁹² Derived from the works of Deleuze and Guattari, the concept of agencement provides Callon a scope to break away from the binding notions of human agency and intentionality, and an opportunity to explore the sociotechnical arrangements of theories, practices, and technologies “endowed with the capacity of acting in different ways depending on their configuration.”⁹³ The economy and Economics come together in an agencement, enmeshed and absorbed in each other, and can never be broken apart, not even by analysis: “there is nothing left outside agencements: there is no need for further explanation, because the construction of its meaning is part of an agencement.”⁹⁴

We may remember, the notion of assemblage or bringing together several apparently disconnected domains of analysis in an artificial depiction of the world is the prerogative of Lévi-Strauss’ concept of model as well. Even in Bauman’s description of economic models, the observed facts and the instruments of formal analysis are integrated in a structure of built-in-justification. Both views are considerably different from the one proposed by Callon. For one, Lévi-Strauss’ model of structural anthropology suggests explicitly the existence of an outside world of concrete social relationships and institutions, whose meaning – the structural principle of “ordering” of the collected data – is in hiding from direct perception and can be approached only by exploring the unconscious categories

⁹¹ Ibid.
⁹² Ibid, 320.
⁹³ Ibid.
⁹⁴ Ibid.
that escape the preliminary enquiries at the field. Bauman’s account of built-in-justification, although speaking of integration of practices, theories, and policies in the same structure, presumes the same distinction between the reality of the model and that of the world, the former being a “stylized” expression of the latter. Both descriptions consider modeling as a technique of representation first, and by way of that, a technology of intervention. For Callon, it is unacceptable for two reasons. By introducing the notion of performativity, he succeeds to undermine the representational capacity of any form of economic argumentation (including models): the task of economics is to perform the world, not to emulate it. Secondly, by conceptualizing sociotechnical agencements as the “material” site of economic actions, he closes off the possibility of any distinction between theory and practice, agency and object, technique and technology. He does not even concede the pragmatic formulation popularized by McCloskey and Kramer: to represent is to intervene, and vice versa. That view too, Callon would argue, corroborates the distinction between representation and intervention, and consequently, between the model and the world, although perhaps unintentionally.

The theory of performativity also distances itself from the overtly “textual” disposition of rhetorical analyses. An agencement includes “material” elements, which cannot be reduced to “a pure world of words and interlocutors,” Callon argues. He draws an analogy with machines and their operating instructions: Without the material device the operating instructions are meaningless.... Likewise, the machine without the instructions is likely to be opaque, unstable, and passive.” The metaphorical example of the assemblage of the machine and its instructions comes in help to establish “why the concept of performativity has led to the replacement of the concept of truth (and nontruth) by that of success or failure.” The machine will not activate, if the instructions are not followed properly; the instructions will be ineffective, if there is something wrong with the machine. However, if we take this analogy seriously, it seems to question the validity of the theory of replacement of truth. Even if the instructions do not represent the machine, and therefore, do not have the responsibility to refer to its reality, they do have to be true themselves in order to help the machine actualize, to turn it on. If there were any technical or printing mistakes in the instructions, would the machine be “operative” at all? It is always assumed by the user that the instructions have a truth-claim within the domain of the machine’s existence. And that claim is ensured, guaranteed, and authorized by the manufactures of the machine. Of course, the machine does not have similar truth-claims regarding

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95 Ibid.
96 Ibid, 319.
97 Ibid, 320.
the instructions’ success. If the machine is already damaged, the instructions will be “meaningless,” but the inability to attain meaning does not involve questions of truth and falsehood; the functional dichotomy between success and failure will suffice to explain the operational crisis. The machine in itself is incapable of invoking the question of truth, but the instructions do contain a possibility of verification in terms of truth and falsehood. Obviously, there is an “asymmetry” between the two elements in the analogy of sociotechnical arrangement, which Callon chooses to overlook. On the contrary, he extends the analogy to the assemblages of economic actions and equations, where the world is “implied by the equation” exactly in the way the machine is actualized by its instructions. When the economy is compared with the machine, and economics with its operative instructions, it calls attention to the functionalist role of economic policies and prescriptions in realizing the world of their implication, but once it is admitted that the “instructions” in the original metaphor have to be authorized as true for implying and contributing to the machine’s operation, the risk of running into a similar asymmetry presents itself. Callon, however, defines the process of actualization of the world as a “long sequence of trial and error, reconfigurations and reformulations.” The process is made possible by the performativity of the trials and the statements – the sociotechnical agencement – and when they are dissociated from the world in which they are supposed to function, the trials and tribulations of adjustments become “inconceivable.” In Callon’s account, the failure of the system happens, when certain events show the tendency to be incompatible with the formula and its world. “Financial crisis,” Callon asserts confidently, “is a crisis for the formula.”

Strangely enough, this realization echoes the alarm expressed by Eichner when he has announced that the discipline of economics is in “crisis” because economic assumptions are far from reality and they cannot provide acceptable solutions to real-life problems. For Eichner, however, in question is the “effectiveness” of the discipline itself – its equations and models, its assumptions and analytical frameworks – because of its inability to intervene in the outside world. Callon would describe this

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98 Ibid. Callon cites an example from Donald McKenzie’s chapter in the same book: Donald McKenzie, “Is Economics Performative?: Option Theory and the Construction of Derivative Markets” in Donald Mackenzie, Fabian Muniesa, and Lucia Siu (eds.), Do Economists Make Markets?: On the Performativity of Economics (Princeton and Oxford: Princeton University Press, 2007), 54-86. Here McKenzie discusses the performative character of the Black-Scholes model in option theory of financial economics. “[In this paper] Donald McKenzie shows,” informs Callon, “for instance, that Black and Sholes’s famous formula, so basically simple, has meaning and effect only in its own world.... One world implied by the equation – without which the equation would not function and which would not function without the equation – is a world in which prices can be observed to follow a random walk” (Callon, “What Does It Mean to Say That Economics Is Performative?”, 320).
99 Ibid, 320.
101 Ibid.
situation not as an evidence of incompetence of the discipline, since the discipline does not exist in isolation with the world that it seeks to intervene, but as the proof of the dynamic functionality of the *agencement* – the sociotechnical assortment of theories, practices, and individuals – that performs the economic. The two ostensibly contrasting positions converge also on the emphasis they put on verification – Eichner on empirical confirmation of a theory in terms of truth and falsehood, Callon on functional estimation of the *agencement* in terms of success and failure.

The concept of sociotechnical *agencement* can also be viewed as a reaction against the restrictive understanding of “agency” in economic analyses confined to the figure of an individual rational economic agent:

> Agency as a capacity to act and to give meaning to action can neither be contained in a human being nor localized in the institutions, norms, values, and discursive or symbolic systems assumed to produce effects on individuals. Action, including its reflexive dimension that produces meaning, takes place in hybrid collectives comprising human beings as well as material and technical devices, texts, etc.\(^2\)

In this formulation, agency is distributed symmetrically between humans, texts, and technologies in sociotechnical *agencements* of beings and things, all acting in equal capacity to perform the economy through reformulations and reconfigurations of the said arrangements. The assemblages of human and non-human actors facilitate expansion of the domain of economic reflexivity. A study of sociotechnical *agencements*, Callon insists, cannot be restricted only to performative utterances by academic economists; the practitioners of the economy, the economic actors, should also be thought capable of producing formulas and producing the world. The “conservative” distinction between theory and practice should be abdicated, since it becomes impossible to distinguish one from the other. Callon calls this expanded understanding of the discipline “economics at large,” which refers to a network of entanglements among actors in an *agencement* endowed with a spirit of “collective innovation.”\(^3\) This is the domain of the “vernacular” – the twilight zone between theory and practice – bearing “a clear illustration of the importance of the concept of economic at large” and drawing “it strength

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\(^3\) Callon, “What Does It Mean to Say That Economics Is Performative?”, 335.
from its previous and continuing capacity to make the world that it describes, and that makes it true, exist.”

Is this a happy ending then, at least for the discipline? As it turns out, all the empirical criticisms are dissolved in the self-explanatory framework of *agencements* – in the “symmetrical” arrangement of its human and non-human elements; and the possibility of a critique of economic reason is foreclosed with the assumption that the histories of struggle, competition, and contestation between different research programs are also part of the same sociotechnical *agencement*: “The history of these struggles is incorporated into markets, just as a living organism retains traces of its evolution.” The impossibility of critique in Callon’s notion of performativity, Philip Mirowski and Edward Nik-Khah claim, follows from its inspiration, the Actor-Network-Theory of Bruno Latour. Callon has written the earliest manifesto of the Actor-Network-Theory with Latour in 1981 and has always held the position that the “social” as an analytical category has run its course as the source of explanation of scientific knowledge. Callon’s theory of performativity, Mirowski and Nik-Khah observe, is an attempt to extend their critique of social-constructivism of natural sciences in the arena of economics, a social science by definition. The problem with this concept, they argue, lies in his willful and persistent confusion of economics with various other activities like “accounting and marketing” – what Callon describes as “economics at large.” The rest of the inconsistencies result from the inattention to “the details of what does and does not count as legitimate “economics” among the agents.” In that sense, they are targeting the latent asymmetries in the *agencement* of economics and the economy, and looking for a resolution in a theory which would take stock of the historical processes of production of these asymmetries. They are right in affirming that, even if knowledge is not produced only by structures of “social relations,” these asymmetries and the procedural logic of “authorizing” what is considered a legitimate economic practice are indeed contingent on what is perceived as the “social.”

104 Ibid, 342.
105 Ibid, 335.
109 Ibid.
A particularly useful way of looking at these procedures of legitimization is to bring back the question of modeling into the picture. Even if the models are not supposed to have a special position within a sociotechnical agencement, a study of modeling can bring into focus the latent asymmetries in the same agencement by underscoring the materiality of practices that are considered legitimate within the corpus of expert knowledge. The shift from the concept of model to the practice of modeling, therefore, leads to repositioning of a critical approach within the apparently ahistorical and obstinately anti-social theory of agencement. But before demonstrating how the practice of modeling may help excavate the asymmetries in a sociotechnical agencement, we need to know what possible reformulations have taken place in the field of “social theory” in the last few decades.

ASSOCIATIONS AND ASYMMETRIES

In the twentieth century, when the universities all over the world started introducing courses in social sciences, the boundaries between different disciplines that dealt with the “social” as an abstract qualifier of human relations and institutions were fortified with unforeseen energy. Timothy Mitchell argues that each of these disciplines “inherited the assumption that a singular logic provided the unseen dynamic of social life” and, for each discipline, the approach to explain this logic was different from the others.\(^{110}\) However, the consensus on the “singularity” of a social qualifier, Mitchell points out, was soon challenged in some sub-divisions of these disciplines, and critical studies by the social historians, cultural anthropologists, and specialists in area studies like gender did crop up to demonstrate the limit of the logic. These critical studies, although aware of the constructiveness of the most of the categories of analysis like class, race, nation, gender, modernity and how these constructions were “less universal, stable, pure, singular, and transparent than [were] usually assumed,” maintained a distance from the world of “systematizing social sciences, both those operating at the individualist level of hypothetico-deductive methods, and those at the structural level of historical systems and processes.”\(^{111}\) Because of this distance, some of the old disciplines and dichotomies remain untroubled. Mitchell mentions particularly of the category of economy. Most of these critical studies treat it as the fulcrum around which the other social imaginaries circulate. They


\(^{111}\) Ibid, 2. History of economics is one discipline where both the hypothetico-deductive and the historical methods come together and interact.
need the ostensible stability of the economic against which the “cultural gains its distinctiveness.”

On the other hand, there are some studies which continue interactions with the economic discipline but critique its tendency to emphasize its autonomy from the social whenever there is chance. Mitchell identifies a problem with these studies – their obsession with the point of representation. He recognizes the strength in Callon’s argument in its reluctance to adopt a framework based on (mis)representation. Primarily an attribute of those sociological critiques of economics, it assumes a clear demarcation between the technologies of intervention and the material world outside to it. Callon’s theory of sociotechnical agencement, however, does not admit this distinction.

As we have just seen, the critical tendency to question the legitimacy of the social has been in vogue for quite some time. “In recent time,” writes Patrick Joyce, “the emphasis on practice, the material world, and embodied being, and the employment of categories like ‘sociality’, ‘network’, ‘reflexivity’, ‘movement’, and so on, alike bear witness to a new questioning and a dissatisfaction with what has gone before.” With the decline of an organicist notion of the social after the first world war, a structural or functional understanding of the abstract social space was initiated. However, in the last few decades, through an array of multidisciplinary engagements, the category was demystified and studied in relation with various forms of power and governmental interventions. From Joyce’s own work, we get to how the accumulation of technical knowledge of governance in the fields of city planning, statistics, and sanitary techniques led to formation of a social space of urbanity in nineteenth century England. Mary Poovey has also shown through her reading of Victorian literature how the emergence of the governmental category of population and the production of an abstract social body through interactions of scientific innovations and political ideals like liberal self-government were intricately linked with each other. What is interesting in both accounts is how the production of the social space was coterminous with that of modern scientific discourse and

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112 Ibid.
114 “The tendency to think of social structures, relationships, and processes as abstractions which yet become in one sense or another real entities, relatively autonomous of other sorts of human activity, and systematic in their operation, has for some time been in question” [Patrick Joyce, “Introduction” in Patrick Joyce (ed.), The Social in Question: New Bearings in History and the Social Sciences (London and New York: Routledge, 2002), 1].
115 Ibid.
expertise. As Joyce puts it succinctly after Poovey, “The human capacity to imagine order is seen to be at the foundation of society itself.”\textsuperscript{118} This juxtaposition of the epistemic and the political concerns has been the subject of Bruno Latour’s investigations for the last few years. In this section, I shall engage with his arguments in detail and show that the possibility of staging asymmetries within a social arrangement has always been there in his writings. At the same time, I shall examine how the mediations between epistemic and political authorities take the form of a translation in his theories.

A summary of Latour’s arguments and his reformulation of the term “social” is available in his \textit{Reassembling the Social}. In this book, he highlights a misuse of the term by some social scientists who ascribe an undeserving double meaning to it: “first, a movement during a process of assembling; and second, a specific type of ingredient that is supposed to differ from other materials [in the assemblage].”\textsuperscript{119}

Latour aspires to challenge the customary practice of treating the social situated on a podium above and separated from all other domains of analysis, and to question the “project of providing a ‘social explanation’ of some other state of affairs.”\textsuperscript{120} It requires, he informs, a complete reshuffling of the orientation of the discipline of sociology – the science of the social. The earlier interpretations of the name entailed restricted understandings of both the social and the science. With time, the products of science and technology experienced great expansions and the meaning of the term “society” went through radical transformations; but the objects and methods of sociological inquiries remained the same. The time has come, Latour assures, to devise an alternative definition of the discipline without discarding its name and staying committed to its intention, that is, to provide a scientific exposition of the social.\textsuperscript{121}

Faced with the questions “what is a society?” and “what does the word “social” mean?” sociologists may offer two distinct sets of explanations. While the former insists on the existence of a “specific sort of phenomenon” named the “social,” separated from and perhaps superior to other domains of

\textsuperscript{118} Patrick Joyce, “Introduction”, 5. Poovey also extends this formulation in her \textit{A History of the Modern Fact} where she shows how the innovations in the field of double-entry bookkeeping helped imagining distinction between the factual and the fictional in the fields of natural philosophy and political economy in the eighteenth and the nineteenth centuries [Mary Poovey, \textit{A History of the Modern Fact: Problems of Knowledge in the Sciences of Wealth and Society} (Chicago: University of Chicago Press, 1998)].


\textsuperscript{120} \textit{Ibid}.

\textsuperscript{121} \textit{Ibid}, 3.
analysis like the economic or the biological, the latter contradicts its claim of specificity and seeks to explore the processes of its own construction. Latour argues that the objective of the second line of thought – championed by himself and the other proponents of the Actor-Network-Theory – is to point out the abject circularity of reason at work in the former framework popularly known as social constructivism: “the social could explain the social.” By this logic, even if it is admitted that forces and realities tagged with the label “economic” may pertain to autonomous theoretical mobilizations, they are finally explained by the fact of being embedded in a social context. This theory of “social embeddedness” entails a seemingly highhanded assertion on part of the experts:

since ordinary agents are always ‘inside’ a social world that encompasses them, they can at best be ‘informants’ about this world and, at worst, be blinded to its existence, whose full effect is only visible to the social scientist’s more disciplined eyes.

The version of social theory that Latour favors does not consider the role of agents to be merely restricted to providing information of the social contexts of other disciplines. The social is not a primary or fundamental principle which binds and sustains the other domains of analysis in an all-encompassing, totalized network of connections and relationships. It is one of the multiple dots in the same network, one among many ingredients in the associations made out of interfaces among different systems of knowledge. The only way to ensure a less specific but more reasonable analysis of the social is to offer an alternative definition. The social, according to Latour, should be viewed as a “trail of associations between heterogeneous elements.” Sociology, therefore, may be described as a discipline, which seeks to trace such associations of various degrees, of different natures and objects. Economics, for example, as a “social science” consists of several forms of associations that facilitate production, distribution, and consumption of goods and services. A sociology of economics should trace these heterogeneous associations without having to impose an order of singular interpretation. Latour is aware that this broader than usual definition of sociology may dilute its meaning. It can be used to describe and examine “any type of aggregate from chemical bonds to legal ties, from atomic forces to corporate bodies, from physiological to political assemblies.” But tracing the associations and their reconfigurations with changes in the incumbent state of affairs necessitates a conception of the social, which is perennially unstable and eclectic: “We

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122 Ibid.
123 Ibid, 4.
124 Ibid, 5.
125 Ibid.
are no longer sure about what ‘we’ means; we seem to be bound by ‘ties’ that don’t look like regular social ties.”

This definition has a strange condition, invoking at one time a wider spectrum of designation, and at another, a restricted domain of investigation. In Latour’s conception of the social, the great divide between the human social actors and the non-human natural objects is dissolved for good. Chemical bonds and communal alliances are given the same discursive status. Agency is no longer confined to the realm of the living; things act and associate too, often to the effect of re-association of the earlier networks. Actors cannot be distinguished from the networks they are embedded within. At the same time, the special status of the social is rejected to be redefined “only as a very peculiar movement of re-association and reassembling.” The similarities between Callon’s sociotechnical agencements and Latour’s actor-networks are obvious. More important is how they both highlight the importance of reconfiguration of the heterogeneous elements – the dynamics of an association, its tracks and trails. As I have argued in the last section, Callon’s notion of performativity does not take account of the asymmetries within an agencement, as they re-present themselves as a non-question to the all-inclusive paradigm of economic performation. Similarly, in Latour’s sociology of associations, the question of internal asymmetries becomes redundant. The task of the sociologist is strictly limited to tracing the associations in their moments of reassembling, but the act of reassembling itself remains inaccessible and elusive, beyond the domain of theorization.

Incidentally, the issue of authorial control – the aspect of normative reiteration in a performative act or statement, which dictates and inscribes these asymmetries – is discarded as a lost cause. Although, the earliest program of Actor-Network-Theory authored by Callon and Latour contained a different perspective. In 1981, Callon and Latour published an article to revaluate the relation between micro- and macro-social actors. They began with Hobbes’ theory of social contract in which, to overcome the “merciless state of nature” making every person wage war against the other, people “authorize” a person among them to speak on behalf of everybody, namely, the Leviathan:

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126 Ibid, 6.
127 Ibid, 7.
Thus ‘authorized’, the sovereign becomes the person who says what the others are, what they want and what they are worth, accountant of all debts, guarantor of all laws, recorder of property registers, supreme measurer of ranks, opinions, judgments and currency.\(^{129}\)

Callon and Latour believed that Hobbes’ account was the first attempt to map the correspondence between the sovereign and the multitude, though it invoked a paradoxical view on the sizes of social actors engaged in mutual contracts. The so-called macro-actors like the state or the corporate bodies are not necessarily of bigger or smaller size than that of the multitude, whose sum of wishes is supposed to materialize in the body of the sovereign:

The sovereign is not above the people, either by nature or by function, nor is he higher, or greater, or of different substance. He is the people itself in another state – as we speak of a gaseous or a solid state.\(^{130}\)

As the theory of social contract found no confirmation in either anthropological studies or historical enquiries, Callon and Latour proposed a different explanation of how the micro-actors became and behaved like macro-actors without having to modify their sizes and substances. “The contract,” they wrote, “however, is merely a specific instance of a more general phenomenon, that of translation.”\(^{131}\)

By translation, Callon and Latour meant an array of negotiations and transactions including intrigue and violence, by which a singular actor or force actualized authority of speaking and acting on behalf of others. The chemical metaphor used earlier of change of state from solid to gaseous or otherwise points to the materiality of the act of translation that the two theorists had in mind. Translation, they argued, helped them retain the spirit of Hobbes’ Leviathan, which offered a theory of representation focused on the sublimation of authorial control into an innocent world of contractual consensus. It was therefore necessary to suspend the distinctions between individuals and institutions on the basis of size or superiority; more instructive was the study of differences wrought by “power relations and the construction of networks.”\(^{132}\) After repeating Hobbes’ theory of isomorphic social actors, Callon and Latour specified that one had to imagine “actors as networks” to avoid the paradox of size:

\(^{129}\) Ibid, 278.
\(^{130}\) Ibid.
\(^{131}\) Ibid, 279.
\(^{132}\) Ibid, 280.
Two networks may have the same shape although one is limited to a point and the other extends all over the country, exactly like the sovereign can be one among the others and the personification of all the others.\textsuperscript{133}

The problem with contemporary sociology, they insisted, lay in its reluctance to desert the principle of size. Once the presumption of different sociological analyses of different actors according to their respective sizes was rejected, the possibility of exploring the modalities of translation would become available and the making of the Leviathan should come under scrutiny.

The next step was even more ingenious. Callon and Latour took up the example of baboons – an animal considered by many to possess a restricted capacity of socialization. They also confirmed that baboons were “social animals,” but the way they chose to define “social” was not in conformity with conventional understanding:

The word ‘social’ derives, we know, from ‘socius’, which is akin to ‘sequi’, to follow. First of all to follow, then to form an alliance or to enlist, then to have something in common, to share. Several act like a single entity, the social link is there.\textsuperscript{134}

The definition provided here was the precursor of the definition of social as “trail of associations” in Reassembling the Social. To “associate” – to follow, enlist, and share common values and territories – was then the basic attribute of any social animal, and the baboons did fit the description, as not only did they tend to enroll and form alliances with other baboons, but also took great care to stabilize those relationships. However, Callon and Latour observed with curiosity, there was no instance of a Leviathan or “durable macro-actor” among the baboons.\textsuperscript{135} But in human societies, the macro-actors were constructed every now and then, and that was achieved by forming associations that could last longer than the interactions that formed them.\textsuperscript{136} The authors gave examples of these associations, which ensured the birth of the Leviathan: “walls and written contracts, the ranks with uniforms and tattoos and reversible friendships with names and signs.”\textsuperscript{137} These were the durable materials, which a micro-actor required for being transformed into a macro-actor – a specialty of the human species. In

\textsuperscript{133} Ibid.
\textsuperscript{134} Ibid, 283.
\textsuperscript{135} Ibid.
\textsuperscript{136} Ibid. Authors” emphasis.
\textsuperscript{137} Ibid, 284.
other words, the instruments of authorial control dictated the norms of association, creating, in Callon and Latour’s own expression, “asymmetries.”

The creation of asymmetries in this account was identified with growth of durable relations stocked in black boxes. “A black box contains,” Callon and Latour wrote, “that which no longer needs to be reconsidered, those things whose contents have become a matter of indifference.” The assumed durability of these relations or associations ensued from the impression that a macro-actor tried to present – that the boxes were tightly closed and fastened. With the increase of associations stocked in the boxes, the macro-actor tended to grow in relative size, despite the condition of isomorphism. The studies of micro- and macro-actors, therefore, should direct their “attention not to the social but towards the processes by which an actor creates lasting asymmetries.”

The investigations should not privilege one type of association over another (like the human relations over the non-human ones), as the inclusion of each element in a black box was equally important for a micro-actor to turn into a macro-actor with durable impact. Hence, the relative importance of social relations to associations with apparently inert objects and technologies must be rejected for a more symmetrical analysis. In a way, it summarizes the political project of Actor-Network-Theory: to resist the asymmetries created by the monstrous actions of the Leviathan, the analytical apparatus must assume a symmetrical form that would abstain from any singular metaphor to describe its paraphernalia:

The Leviathan is such a monster that its essential being cannot be stabilized in any of the great metaphors we usually employ. It is at the same time machine, market, code, body, and war.

However, the so-called symmetry between human and non-human actor-networks, Latour cautions us later, does not presume the celebrated dichotomy between subjects and objects. The division is itself a myth created for convenience. The objective of the Actor-Network-Theory is not to establish a symmetrical relation between the two domains of analysis, as the division does not exist in the first

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139 Ibid.
140 Ibid, 285-86.
141 Ibid, 294.
142 Latour, Reassembling the Social, 75.
place: “To be symmetric, for us, simply means not to impose a priori some spurious asymmetry among human intentional action and a material world of causal relations.”

Clearly, there are two forms of asymmetries being discussed and derided in these texts. The first one is created by inclusion of more elements of durable associations stocked in the black boxes by some actors than others, and the second one is the result of an uneven distribution of attention by social scientists to human and non-human agencies. A conflation of the political and the analytical registers occurs, when it is argued that an *a priori* imposition of asymmetry among human actions and material world would expand the asymmetries created by normative conditions of associations. The strength of the Actor-Network-Theory premised on the asymmetric distribution of agency among human and non-human actors lies precisely in the realization of the power relations vested in analytical or discursive interventions. Callon and Latour, however, were more interested in analyzing asymmetries between different associations, enrolling “a little more than relationships, alliances and friendships” – the durable elements like walls, uniforms and contracts – and turning into Leviathan monstrosities. The increase of the relative size of certain micro-actors, as we know from the comparative analysis of baboon and human societies, would not have taken place, if these elements were not incorporated in the black boxes. The humans were the only species, among whom the Leviathan exists, and that is because of the inclusion of durable non-human elements in these black boxes, elements other than human relationships and alliances.

This observation raises an interesting point. If the fundamental difference between an animal and a human society is understood in terms of the existence of the Leviathan, and if the Leviathan is not supposed to have existed without the insertion of durable non-human elements in the black boxes, the notion of humanity itself becomes conditional on its association with the non-humans. The non-human elements in an association, therefore, become the essential counterpart of humanity, without which it cannot be theorized. To produce social theory, one must talk about the humans and non-humans in the same breadth; otherwise theories lose their impact and criticality. Do the non-humans find their essential counterparts among humans? Not necessarily. If the baboons ever get to include durable elements like written contracts and uniforms in their black boxes, they will have their own Leviathans, eliminating the last traces of distinction with the humans. Undoubtedly, the non-human elements in the associations help transformation of micro-actors into macro-actors more decisively.

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144 Callon and Latour, “Unscrewing the Big Leviathan”, 284. Authors” emphasis.
than the human elements like friendships and alliances do. It seems that, although all elements in the black boxes are equally indispensable to bestow authority in the Leviathan, some elements are more equally indispensable than others.

This possibility of asymmetries within the networks, however, was not acknowledged by Callon and Latour in the present article. Their later works seem even more indifferent to internal asymmetries: a parallel example is found in Callon’s conception of sociotechnical *agencements*, where the asymmetry between economic theory as a manual of the economic machine – the economy itself – remains unexplored. Considering that, this essay by Callon and Latour is quite special, especially in its keenness to link the issue of authority with networks, where the durability of material assemblages ensures the calculus of power:

[Hobbes] omits to say that what makes the sovereign formidable and the contract solemn are the palace from which he speaks, the well-equipped armies that surround him, the scribes and the recording equipment that serve him.\(^{145}\)

The formidable of authorial control and the historical conditions of their durability are solidified in the act of translation – the cache of negotiations and persuasions that mould others’ wills into that of the Leviathan, rendering illegitimate any other desire in the process. “Hobbes restricted this process of translation,” Callon and Latour wrote, “to what we now call ‘political representation’.”\(^{146}\) It is true that the way it was defined by the two authors, translation surpasses the restrictive responsibility of interpreting political will of the multitude. The economic and sociological machines also translate the desires of the multitude, apart from calculating and calibrating their worth. At the same time, it has become impossible to distinguish among these domains of translation. Transgressions, overlaps, and analyses are intertwined. Translatability of one of these domains into another – the potential of one of them becoming the other by re-assembling durable and non-durable materials – has also emerged as a strong element of both discursive and political interventions. In the next section of this chapter, I plan to explore how translation or mediation among different domains of associations – economic, political, social, and natural – fortifies the normative technologies of intervention.


\(^{146}\) *Ibid*, 296.
MODELS OF TRANSLATION, TRANSLATION AS MODELING

In the last section, we have encountered a general description of the practice of translation which is a political act and though which occurs the authorial consolidation of the will of the multitude. In Latour and Callon’s account, however, one point is missing: the specific use of the term “translation” in the colonial context, especially in its role in the pedagogical enterprises adopted by the colonizers. How far is it possible to extend or contrive the meaning of the term informed by “specific conditions” in the colony? Does that mean that colony can only appear as a special case in the theory of translation and the subsequent reformulation of the social as a “trail of associations” emerging out of these acts of translation? In the final section of this chapter, I shall argue the opposite. My contention is that the colony is not a special case in a general theory of translation and it occupies a central position in this reformulation of the social. By not recognizing this centrality, Latour and Callon also fail to situate the point of “asymmetry” in a more productive way and miss out on the opportunity to produce a more historically grounded critique of social constructivism. In this context, I shall bring back the issue of modeling at the heart of this problematic and try to define the vernacular as the space where the social is produced and demarcated through interactions between various modalities of translation.

The question of the social in colonial context, as Gyan Prakash has observed, was one of “externality”: “Unable to position itself as the political instance of the indigenous society, the colonial state operated as a Leviathan brought into existence by the conqueror’s sword.” Prakash is right to point out that the markers of modernity and capital in colonial India were instituted on despotic coercion, but his conclusion from there that the movement of the social was stalled because of the realization by the British of a historically mandatory incompleteness in the natives to form society seems quick and unsubstantiated. Prakash claims that the colonizers found no resemblance between the societies here and in Europe and deduced that what had existed before their advent were indigenous collectives such as castes, tribes, clans, etc. So, the colony became a site of anthropological investigations based on racial profiling and not of sociological studies – the “classic discipline of European modernity.” Prakash’s insistence on a clear distinction between

148 Ibid, 82.
149 Ibid.
these two forms of knowledge is reproduced in his conception of colonial modernity as a site of mediation between the colonized and the colonizers in the arena of “community,” as opposed to that of the civil society, “for the articulation of state and capital.”\footnote{Ibid, 83. “By community,” Prakash writes, “I mean an affiliation that invokes collective bonds and rights based on imagined ties of kinship, religion, culture, past and sentiments. The notion of collective interest affiliation distinguishes community from society, that is, an association of sovereign individual subjects based on laws and contracts” [ibid, 84]. It is not clear that whether this distinction was Prakash’s own or he is simply reproducing the British view on the subject. In any of the cases, this distinction looks surprisingly precise, uncontested, and mutually exclusive.} He then presents a detailed and interesting account of how colonial governmentality appropriated the logic of community by not only misrecognizing some of its elements but by “concretizing” these misrecognitions as “social facts” through governmental apparatuses like census.\footnote{Ibid, 86.} This valuable observation, however, as Timothy Mitchell has suggested, leaves out certain spaces untroubled and certain questions answered.\footnote{Timothy Mitchell, \textit{Rule of Experts}, 1-15.} Evidently, the mediation between the state and the so-called communities involved acts of translation which informed the formation of social facts by census and other enumerative apparatuses. Without interrogating these technologies to some extent and without clarifying the issue of participation by the natives in these processes, Prakash imagines a one-way traffic between the colonial state and the so-called communities in the form of imposition of certain norms from above. Even when there were displacements, the impetus was believed to have arisen only from below, once again leaving out the question of involvement by the state in these very displacements.

This idea of a monolithic colonial state and its resisting subjects is conducive to the standard problems of translation studies in the context of colonialism. Most of the works on translation practices in colonial India assume two positions: it is either described as a mechanism of imposing western values and ideas on the natives, or it is celebrated as the natives’ strategy to situate themselves in western texts as surplus meaning.\footnote{Tejaswini Niranjana writes in one of her essays that she sees translation as “that set of practices which, authorising and authorised by certain classical notions of representation and reality that underwrite teleological models of history, ultimately contribute to the rise of English education in India” [Tejaswini Niranjana, “Translation, Colonialism, and Rise of English”, \textit{Economic and Political Weekly} 25, no. 15 (April, 1990), 779]. Niranjana brings in focus how these practices helped construction of the colonial subject by imposing “overarching [European] concepts of reality, knowledge, and representation” (ibid, 773). She develops this understanding in her \textit{Siting Translation} in terms of “strategies of containment” which projected the colonized as “objects without history” [Tejaswini Niranjana, \textit{Siting Translation: History, Post-Structuralism, and the Colonial Context} (Berkeley: University of California Press, 1992)]. On the other hand, Saurabh Dube, writing on the accounts by Indian catechists and translation practices embedded in the politics of conversion in the early twentieth century, discovers traces of a “vernacular Christianity” which through its surplus producing procedures of translation recast the evangelical idioms and “simultaneously straddled and subverted” the close connections between colonial power and}
interlinked concepts with which these writings are obsessed are “representation” and “identity”: “because the colonial power controls representation and forces its subjects to use the colonial language, it is in a position to construct the forms of indigenous and subject identity.” As a result, a simple dichotomy is suggested between metropolitan knowledge and colonial resistance. All translations are assumed to oscillate between them until they settle comfortably in one of the pockets. If we observe closely, the real problem lies in the assumption of a symmetric distribution of agency between the colonial state apparatuses and the effects they produced on the native subjects. It is therefore necessary to introduce an alternative conceptual framework which would expose the politics of this symmetric distribution and question the received dichotomies based on representation and identity.

Here, at this juncture, I propose to go back to our earlier discussion of modeling practices in contemporary economics and re-invoke a moment from there – the “materiality” of modeling resulting from concerns regarding the various material specifications or constraints of the model and a commitment to properly demonstrate few universal principles – to study certain modalities of translation in colonial India. At the risk of sounding anachronistic, let me explain what particular mode of modeling will be perfect for this study, and for what reasons. By modeling, in this context, I mean “translation” of metaphors into a physical model – machinic, or diagrammatic. As we have seen, in any counterfactual model, physical or mathematical, the central concern is to situate a metaphor at the heart of the model and initiate its dynamic by extending an interactive narrative or “story.” In Morgan and Boumans’ account, the shift from a metaphor to a model involved a lot of considerations to be made, mostly between the constraints, the size, shape, and angles of the apparatuses, and the commitment to explain a particular theory – which is autonomous and true irrespective of the working of the model, and hence, has an asymmetric relationship with it. The

Christian knowledge [Saurabh Dube, “Colonial Registers of a Vernacular Christianity: Conversion to Translation”, Economic and Political Weekly 39, no. 2 (January, 2004), 169]. On the whole, there is a recognition of the importance of translation practices in shaping the political project of colonialism, and the consensus is that, by importing concepts unfamiliar to the soil of this country and, conversely, by injecting ideas which were exclusively pertinent to the conditions of the colony, these practices bring together different sensibilities at one place and produce hybrids which carry elements of both the worlds. However, Partha Chatterjee cautions against an unqualified use of the notion of hybrid in this context, as it churns out against its own logic a series of homogeneities that elude any kind of analytical distinction [Partha Chatterjee, “The Disciplines in Colonial Bengal” in Partha Chatterjee (ed.), Texts of Power: Emerging Disciplines in Colonial Bengal (London and Minneapolis: University of Minnesota Press, 1995), 20].

only reason one takes the trouble to make a model then, Morgan and Boumans argue, is that it has a capacity to “demonstrate” the complex theory in a concrete situation.\textsuperscript{155}

Taking these clues together, I shall argue that “modeling” or translation of metaphors into a model gives an opportunity to have a structural analysis of the modalities of translation in a colonial context. First of all, as Dipesh Chakrabarty suggests, in a colonial or postcolonial situation, the issue of translation is often subjected to that of applicability of certain concepts which were held by Western scholars as universally valid.\textsuperscript{156} The categories of political economy, Chakrabarty argues, “contained a certain degree of prejudice” which is substantiated by the non-rational and non-universal etymologies they had.\textsuperscript{157} Also, he points out, the forms of economic argumentation are mired in both prose and mathematical calculations, and the strength of an argument depends on finding a balance between the two. The objective of a postcolonial political economy, he explains, should be to deal with the “problems of categorical translation as part of the narrative of translation to capitalism.”\textsuperscript{158} The use of “modeling” as a conceptual framework, in fact, addresses all these concerns – applicability of concepts, literariness of argument, and embeddedness of practices and narratives of translation. The point of applicability is visibly a major point of consideration in all the decisions to build a model according to the correspondence between material constraints of the model and the commitment to theory. The ensuing modifications are also results of these correspondences. The literariness of economic arguments is also emphasized in the admission of a metaphorical relationship between theory and model. Finally, by adopting modeling as a mode of conceptualization, we can also draw attention to the myriad strategies and techniques of translation informed and codetermined by the material conditions of the shift from a metaphor to a model and the narratives of this transition.

One may still ask: how far is it advisable to import a conceptual framework from the debates in the contemporary world and extrapolate retrospectively on a historical situation some two hundred years ago? I have two answers for that. First, it will a mistake to imagine that this debate around the shift of a metaphor to a physical model is one of recent origin. It has its place in the history of liberal political economy almost since the moment of its inception. Morgan and Boumans describe the

\textsuperscript{155} Morgan and Boumans, “Secrets Hidden by Two-Dimensionality”, 387-93.
\textsuperscript{157} Ibid, 29, 30.
\textsuperscript{158} Ibid, 32.
analogy between cash flow and liquid flow as something “more than a metaphor; it was an informative and substantive analogy.” This analogy is still used most prominently in an organicist conceptualization of the economy. Kath Watson gives many examples of how in the age of finance capitalism, the analogy between circulation of money and that of blood makes frequent appearance in both popular media and standard textbooks: “In the contemporary world of finance, central banks serve as a kind of artificial heart, intervening to re-establish flow when circulation falters, and not always successfully.” But the first instance of this analogy can be found in Adam Smith’s Wealth of Nations where he diagnosed Great Britain of having blockages in blood circulation of its “body politic” due to its misapplication of mercantilist theories of monopoly trade in North American colonies:

In her present condition, Great Britain resembles one of those unwholesome bodies in which some of the vital parts are overgrown, and which upon that account, are liable to many dangerous disorders, scarce incident to those in which all the parts are more properly proportioned. A small stop in that great blood-vessel, which has been artificially swelled beyond its natural dimensions, and through which an unnatural proportion of the industry and commerce of the country has been forced to circulate, is very likely to bring on the most dangerous disorder upon the whole body politic.

The imagery of swelling of a blood-vessel beyond its “natural” dimension must have been effectively horrific for any country planning to adopt a policy of monopoly trade. But more importantly, it indicates the centrality of metaphor even in the oldest and the most celebrated doctrine of classical political economy. It will not be far-fetched to think that when the British brought their political economy to the colony, they also did not forget to bring these inbuilt metaphors and the analogical illustrations with it. Once they arrived with these “universal” theories in a land of unknown dimension, they were faced with certain constraints emanating from the physical and material peculiarities of the country. Translation of these theories in a colony replete with such constraints also brought about certain displacements in their commitment to the principles of the discipline. In the second part of this dissertation, I shall elucidate how the correspondence between these constraints and commitments created the space of the vernacular in Colonial India and produced

159 Morgan and Boumans, “Secrets Hidden by Two-Dimensionality”, 387.
multiple conceptions of the social in different moments of its so-called application. The colony, in this formulation, was not a passive recipient of Western values and theories as denoted unfairly in the term “application,” but contributed quite energetically to various disfigurations that followed from the interactions between these constraints and commitments.

Most of these interactions, as we shall see later, took place in the pedagogical sites in colonial India where the experiences of teaching political economy as a discipline in the schools and colleges raised questions about its suitability to a foreign condition and the justification of an Indian political economy that should be prepared to familiarize the native students to its “difficult” principles. This mode of familiarization, we already know from Morgan and Boumans, is integral to modeling because of its capacity to “demonstrate.” In this sense too, the practice of modeling seems to be a fitting conceptual analogy of colonization. Demonstration – or, as Timothy Mitchell calls it, “exhibition” – was one of the chief modalities of colonial power. From accounts of visiting world exhibitions in nineteenth-century Europe by Egyptian and other Arab travelers, Mitchell shows how the replicas of their colonial conquests, orderly arranged and mapped within the exhibition space, helped the colonizers to impose an order of meaning on the colonial space of difference. A similar imposition and dissemination of order happened in case of the colonial pedagogical sites where the “local” illustrations of supposedly “universal” principles generated different strategies of “modeling the social.”

Thus, by using modeling as a conceptual framework to study the making of the social in the colony, we are able to redefine translation, not merely as linguistic transference or mediations between different domains of knowledge production, but as a form of modeling – shifting of a metaphor into a physical model through correspondences between various constraints and commitments. This definition, though not fundamentally different from the one suggested by Latour and Callon, supplements their theory by locating “asymmetry” in the relationship between the metaphor and the model in the procedures of socialization. Once this asymmetry is recognized, the old and linear histories of any discipline start to disintegrate. To make this point more emphatically, in the next chapter, I shall revisit a particular moment in the history of political economy in the West and show how the conceptual framework of modeling can help us unfold a novel genealogy of economic

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162 Timothy Mitchell, *Colonizing Egypt* (Berkeley: University of California Press, 1991). From accounts of visiting world exhibitions in nineteenth-century Europe by Egyptian and other Arab travelers, Mitchell shows how the replicas of their colonial conquests, orderly arranged and mapped within the exhibition space, helped the colonizers to impose an order of meaning on the projected sites of difference.
expertise in the nineteenth century and question the conservatism of the standard histories of the discipline.