

Distributed agency within intersecting ecological, social, and scientific processes*

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Societies emerge as changing alignments of social groups, segments, and classes, without either fixed boundaries or stable internal constitutions... Therefore, instead of assuming transgenerational continuity, institutional stability, and normative consensus, we must treat these as problematic. We need to understand such characteristics historically, to note the conditions for their emergence, maintenance and abrogation. (Wolf 1982, p. 387)

Introduction

The anthropologist, Eric Wolf, proposes a conceptual inversion. Whenever theory has built on the dynamic unity and coherency of structures or units—in Wolf’s case societies or cultures—consider, instead, what would follow if those units were to be explained as contingent outcomes of “intersecting processes.”¹ This broad “Wolfian” heuristic informs this essay’s extensions of Developmental Systems Theory (DST) to cases in the sociology of mental illness, social-environmental studies, and social studies of science.² I link the three cases in a project of reconceptualizing human agents, in particular agents who are establishing knowledge and engaging in change. I show that viewing agents in terms of intersecting processes is also equivalent to teasing open their “heterogeneous construction,” that is, their contingent and on-going mobilizing of webs of diverse materials, tools, people, and other resources.

The importance for DST of reconceptualizing agency is indicated by a the section of Susan Oyama’s The Ontogeny of Information on “Subjects and Objects.” Oyama describes our primary experience of ourselves as subjects maturing from dependence and passivity to independence and control—what I call “concentrated” agency. We come to experience temporal continuity, casual potency, and are able to impart order according to prior knowledge and plan. This experience, however, “exaggerates our role as detached subjects and denies our object-like status” (Oyama 1985, 76). Accordingly, when we try to explain development, interaction, and perception, we tend to posit another subject inside

ourselves—mental modules, optimizing or rational actors, or, most notably, genes. Similarly, to explain the order of the world people have traditionally posited a subject outside it, God, or, more recently, “the-forces-of-natural-selection.”

In order to develop better explanations of development, interaction, and perception, we need, Oyama implies, metaphors and concepts that do not rely on the dynamic unity and coherency of agents, or on superintending agents within or outside those agents. And, to the extent that such patterns of thought persist because of their resonance with the experience agents have of their relations and actions in the material and social world, we need different experience. Or, better, we need to highlight submerged experience of ourselves as “object-like” or “distributed,” that is, as agents dependent on other people and many, diverse resources beyond the boundaries of our physical or mental selves. After all, the primary experience of becoming an autonomous subject is not “raw” experience, let alone uniform and universal experience (Lebra 1984 cited in Kondo 1990, p. 32), but experience mediated through particular social discourse.

There are circles here to be wrestled with. New concepts and metaphors might emerge if we experienced ourselves differently, but what counts as our primary experience is mediated by prevailing conceptual schemes and shared metaphors. And in current Western social discourse, these highlight our autonomy as subjects. Conversely, when some of us seek to theorize Developmental Systems or, in my case, to highlight distributed agency, we foresake the facilitation afforded by prevailing concepts and metaphors of concentrated agency. To so distance ourselves from the dominant discourse, however, requires a strong sense of “independence” and “causal potency” in attempting to impose an order—on one’s world and on one’s audiences.³ With these tensions acknowledged, but not resolved, let me move to the three cases.

Case I. The development of severe depression in a sample of working class women

A body of research initiated by the British sociologists Brown and Harris in the 1960s, has interpreted the social origins of mental illnesses in a way that undercuts the persistent dichotomization of genes vs. environment. This “life events and difficulties” research, which is not well known in the United States, allows one to conclude that apportioning behavior to genes or environment is, at least for those seeking to reduce the incidence of mental illness, at best, not very informative or helpful.⁴ To see how this follows, let me sketch their explanation of acute

depression in working-class women in London (Brown & Harris 1978, 1989). I will also work in the extensions of their findings and generalized narrative contributed by Bowlby, a psychologist who focused on the long term effects of different patterns of attachment of infants and young children to their mothers (Bowlby 1988).

Four factors are identified by Brown and Harris as statistically more common in women with severe depression: a severe, adverse event in the year prior to the onset of depression; the lack of a supportive partner; persistently difficult living conditions; and the loss of, or prolonged separation from, the mother when the woman was a child (under the age of eleven). Bowlby interprets this last factor in terms of his and others' observations of secure versus anxious attachment of young children to caregivers. In a situation of secure attachment the caregiver, usually the mother, is, in the child's early years, "readily available, sensitive to her child's signals, and lovingly responsive when [the child] seeks protection and/or comfort and/or assistance" (Bowlby 1988, p. 167). The child more boldly explores the world, confident that support when needed will be available from others. Anxious attachment, on the other hand, corresponds to inconsistency in, or lack of, supportive responses. The child is anxious in its explorations of the world, which can, in turn, evoke erratic responses from caregivers, and the subsequent attempt by the child to get by without the support of others.

The top three strands of figure 1 (class, family, psychology) combine the observations above to explain the onset of serious depression.⁵ The factors are not separate contributing causes, like spokes on a wheel, but take their place in the multistranded life course of the individual. Each line should be interpreted as one contributing causal link in the construction of the behavior. The lines are dashed, however, to moderate any determinism implied in presenting a smoothed out or averaged schema; the links, while common, do not apply to all women at all times, and are contingent on background conditions not shown in the diagram. For example, in a society in which women are expected to be the primary caregivers for children (a background condition), the loss of a mother increases the chances of, or is linked to, the child's lacking consistent, reliable support for at least some period. Given the dominance of men over women and the social ideal of a heterosexual nuclear family, an adolescent girl in a disrupted family or custodial institution would be likely to see a marriage or partnership with a man as a positive alternative, even though early marriages tend to break up more easily. In a society of restricted class mobility, working-class origins tend to lead to working-class adulthood, in which living conditions are more difficult, especially if a woman has children to look after and provide for on her own. In many

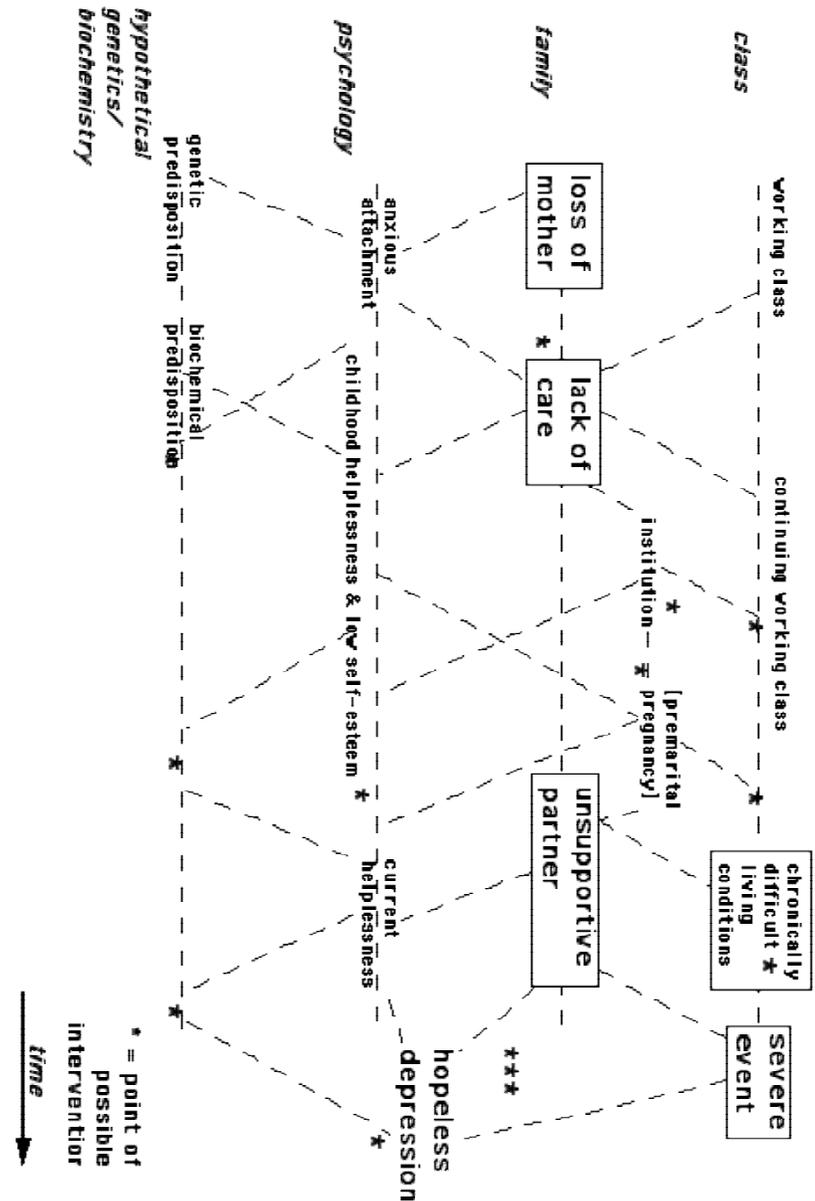


Figure 1. Pathways to severe depression in a study of working class women. The dashed lines indicate that each strand tends to build on what has happened earlier in the different strands. See text for discussion and note 5 for sources

such ways these family, class, and psychological strands of the woman's life build on each other. Let us also note that, as an unavoidable side effect, the pathways to an individual's depression intersect with and influence other phenomena, such as the state's changing role in providing welfare and custodial institutions, and these other phenomena continue even after the end point, namely, depression, has been arrived at.

--Insert figure 1 here--

Suppose now, quite hypothetically, that certain genes, expressed in the body's chemistry, increase a child's susceptibility to anxiousness in attachment compared to other children, even those within the same family. Suppose also that this inborn biochemistry, or the subsequent biochemical changes corresponding to the anxiety, rendered the child more susceptible to the biochemical shifts that are associated with depression. (This hypothetical situation is given by the bottom strand of fig. 1.) It is conceivable that early genetic or biochemical diagnosis followed by lifelong treatment with prophylactic antidepressants could reduce the chances of onset of severe depression. This might be true without any other action to ameliorate the effects of loss of mother, working-class living conditions, and so on. There are, however, many other readily conceivable engagements to reduce the chances of onset of depression, for example, counseling adolescent girls with low self-esteem, quickly acting to ensure a reliable caregiver when a mother dies or is hospitalized, making custodial institutions or foster care arrangements more humane, increasing the availability of contraceptives for adolescents, increasing state support for single mothers, and so on. If the goal is reduction in depression for working-class women, the unchangeability of the hypothetical inherited genes says nothing about the most effective, economical, or otherwise socially desirable engagement—or combinations of engagements—to pursue. Notice also that many of these engagements have their downstream effect on depression via pathways that cross between the different strands. For example, if self-esteem counseling were somewhat effective then fewer unwanted pregnancies and unsupportive partnerships might be initiated; both effects could, in turn, reduce the incidence of single parenthood and difficult living conditions.

These sequences of multiple causes, building on each other over the individual's life history, permit a number of conclusions about the nature-nurture debate:

1. Neither the unchangeability of genes nor the reliability of some gene- or biochemistry-based intervention, such as the hypothetical prophylactic antidepressants, would prove that the genes are the most significant cause of the acute depression that has been occurring in the absence of such treatment.

2. Critics of genetic explanations could dismiss the attribution of an individual's behavior to genes (or 50% or 80% to genes) as a technically meaningless partitioning of causes without placing themselves at the other pole from genetic determination.⁶ That is, they would not have to make the counterclaim that the environment determines behavior or that, if the right environment were found, any desired behavior could be elicited. The Brown-Harris-Bowlby (BHB) account addresses malleability or immalleability of behavioral outcomes without ruling out genetic contributions.

3. Similarly, critics would not need to rest their case on demonstrations that behavioral genetics has been or still is methodologically flawed (Lewontin et al. 1984), on textual deconstructions of the categories and rhetoric employed (Lewontin 1979), or on attributions of political bias to the supporters of behavioral geneticists. These are all interesting, but, in light of the BHB account of the behavior, not necessary for a conceptual critique of genetic determinism.

Over and above these conclusions, the BHB account of the origins of acute depression in working-class women also displays the following features that I associate with the idea that something is “heterogeneously constructed,” or an outcome of “intersecting processes.” (Most of these have equivalents in DST.)

- a) Without any superintending constructor or outcome-directed agent,
- b) many heterogeneous components are linked together, which implies that
- c) the outcome has multiple contributing causes, and thus
- d) there are multiple points of intervention or engagement that could modify the course of development. In short,
- e) causality and agency are distributed, not localized. Moreover,
- f) construction is a process, that is, the components are linked over time,
- g) building on what has already been constructed, so that
- h) it is not the components, but the components in linkage that constitute the causes. Points c) and f–h) together ensure that
- i) it is difficult to partition relative importance or responsibility for an outcome among the different types of cause (e.g., 80% genetic vs. 20% environmental). Generally,
- j) there are alternative routes to the same end, and
- k) construction is “polypotent” (Sclove 1995), that is, things involved in one construction process are implicated in many others. Engaging in a construction process, even in very focused interventions, will have side effects. Finally, points f) and k) mean that
- l) construction never stops; completed outcomes are less end points than snapshots taken of ongoing, intersecting processes.

I am aware that there may be objections to the case I have chosen to make the preceding points. In discussing depression among working class women, rather than in other groups, I could be seen as perpetuating a male, professional class perspective. However, the politics of the case can be viewed quite differently. Although depressed working class women are the focus, the intersecting processes account brings a range of other agents into the picture. While the account does not identify ways to cure the women studied, other girls and women that follow them might seek support from, or find themselves supported by—to pick up on the potential engagements mentioned earlier—counsellors, hospital social workers, people reforming custodial institutions, family planning workers, social policy makers, and so on. Moreover, these agents can view their engagement as linked with others, not as a solution on its own. For example, when women's movement activists create women's refuges as a step away from living in unsupportive households, this makes it possible for therapists who specialize in the psychological dynamics of the woman in her family to consider referring women to refuges as a critical disruption to the family's dynamic. The politics of highlighting different kinds of causes and their interlinkages can be seen as promoting such exchange among the distributed set of agents and contributing to the potential re-formation of the social worlds intersecting around the development of any given focal individual or outcome.

Case II. The history of soil erosion in a region of Oaxaca, Mexico

In the mid 1980s resource economist Raúl García-Barrios, and his ecologist brother, Luis, studied severe soil erosion in a mountainous agricultural region near San Andrés in Oaxaca, Mexico, and traced it to the undermining of traditional political authority after the Mexican revolution (García-Barrios and García-Barrios 1990). The soil erosion of the twentieth century is not the first time this has occurred in this region of Oaxaca. After the Spanish conquest, when the indigenous population collapsed from disease, the communities moved down from the highlands, abandoning terraced lands, which then eroded. The Indians adopted labor-saving practices from the Spanish, such as cultivating wheat and using plows. As the population recovered during the eighteenth and nineteenth centuries, collective institutions evolved that reestablished and maintained terraces and stabilized the soil dynamics. Erosion was reduced and soil accumulation was perhaps stimulated. This type of landscape transformation also needed continuous and proper maintenance, since it introduced the potential for severe slope instability. The collective institutions revolved around first the Church and then, after independence

from Spain, the rich Indians, caciques, mobilizing peasant labor for key activities. These activities, in addition to maintaining terraces, included sowing corn in work teams, and maintaining a diversity of maize varieties and cultivation techniques. The caciques benefited from what was produced, but were expected to look after the peasants in hard times—a form of moral economy (Scott 1976). Given that the peasants felt security in proportion to the wealth and prestige of their cacique and given the prestige attached directly to each person's role in the collective labor, the labor tended to be very efficient. In addition, peasants were kept indebted to caciques, and could not readily break their unequal relationship. The caciques, moreover, insulated this relationship from change by resisting potential labor saving technologies and ties to outside markets.

The Mexican revolution, however, ruptured the moral economy and exploitative relationships by taking away the power of the caciques. Many peasants migrated to industrial areas, returning periodically with cash or sending it back, so that rural transactions and prestige became monetarized. With the monetarization and loss of labor, the collective institutions collapsed and terraces began to erode. National food pricing policies favored urban consumers, which meant that in Oaxaca corn was grown only for subsistence needs. New labor-saving activities, such as goat herding, which contributes in its own way to erosion, were taken up without new local institutions to regulate them.

Although this synopsis of the García-Barrios brothers' account is brief and, like the first case, smoothed out, it allows me to reiterate and elaborate on the intersecting processes viewpoint in the context of social-environmental studies:

1. Differentiation among unequal agents: Sustainable maize production depended on a moral economy of cacique and peasants, and the inequality among these agents resulted from a long process of social and economic differentiation. Similarly, the demise of this agro-ecology involved the unequal power of the State over local caciques, of urban industrialists over rural interests, and of workers who remitted cash to their communities over those who continued agricultural labor.

2. Heterogeneous components and inseparable processes: As highlighted in figure 2, the situation has involved intersecting processes operating at different spatial and temporal scales, involving elements as diverse as the local climate and geo-morphology, social norms, work relations, and national political economic policy. The processes are interlinked in the production of any outcome and in their own on-going transformation. Each is implicated in the others, even by exclusion (Smith 1984), such as when caciques kept maize production during the nineteenth century insulated from external markets. No one kind of thing, no single

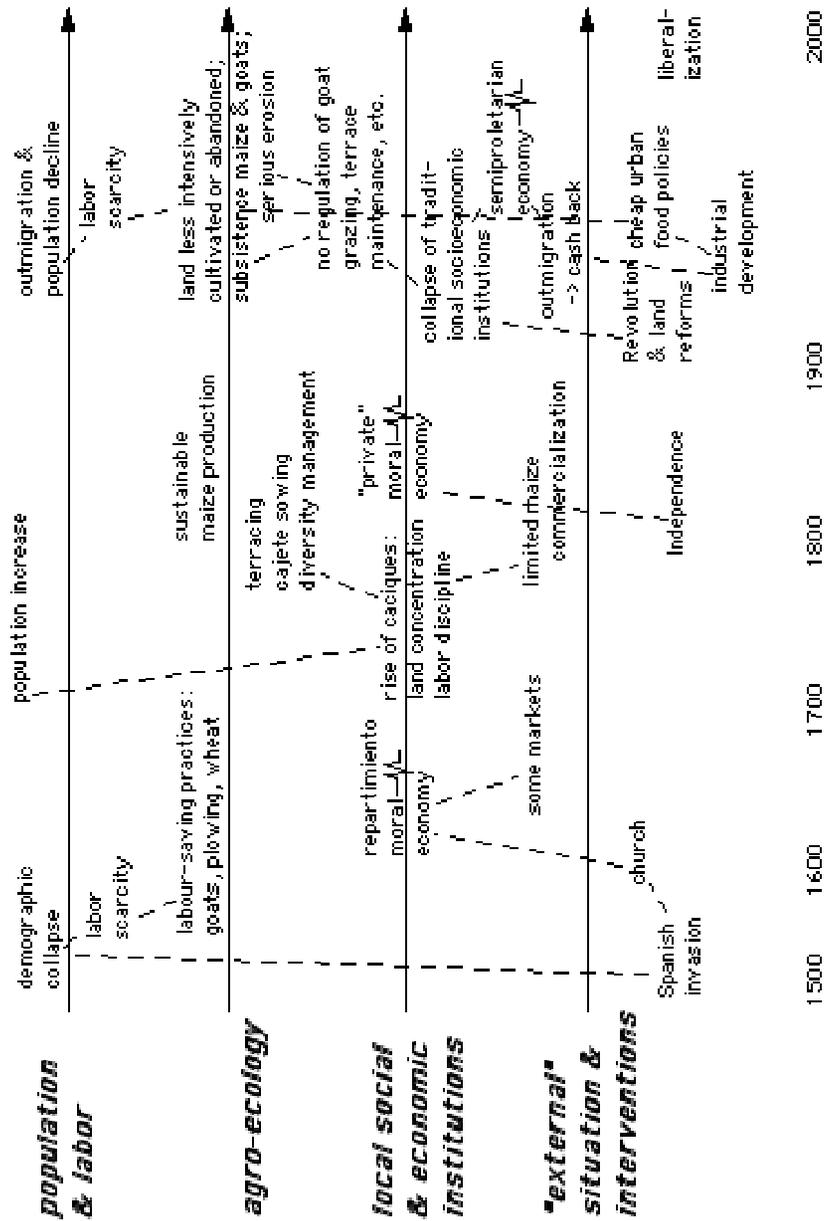


Figure 2. Intersecting processes leading to soil erosion in San Andrés, Oaxaca. The dotted lines indicate connections across the different strands of the schema. See text for discussion.

strand on its own, could be sufficient to explain the currently eroded hillsides. In this sense, an intersecting processes account contrasts with competing explanations that center on a single dynamic or process, e.g., climate change in erosive landscapes; population growth or decline as the motor of social, technical, or environmental change; increasing capitalist exploitation of natural resources; modernization of production methods; or peasant marginalization in a dual economy (Peet and Watts 1996).⁷

3. Historical contingency of processes: The role of the Mexican revolution in the collapse of nineteenth-century agro-ecology reveals the contingency that is characteristic of history. The significance of such contingency rests not on the event of the revolution itself, but on the different processes, each having a history, with which the revolution intersected.

4. Structuredness: Although there is no reduction to macro- or structural determination in the above account, the focus is not on local, individual-individual transactions. Regularities, e.g., the terraces and the moral economy, persist long enough for agents to recognize or abide by them. That is, structuredness is discernable in the intersecting processes.

5. Distributed agency: The agency implied in the account of the García-Barrios brothers was distributed, not centered in one class or place. In the nineteenth-century moral economy caciques exploited peasants, but in a relationship of reciprocal norms and obligations. Moreover, the local moral economy was not autonomous; the national political economy was implicated, by its exclusion, in the actions of the caciques that maintained labor-intensive and self-sufficient production. Although the Mexican revolution initiated the breakdown in the moral economy, the ensuing process involved not only political and economic change from above, but also from below and between—semi-proletarian peasants brought their money back to the rural community and reshaped its transactions, institutions, and social psychology.

6. Intermediate complexity: The García-Barrios brothers include heterogeneous elements in their account, but, as my synopsis and figure 2 indicate, different strands can be teased out. The strands, however, are cross-linked; they are not torn apart. In this sense, the account has an intermediate complexity—neither highly reduced, nor overwhelmingly detailed. By acknowledging complexity, the account steps away from debates centered around simple oppositions, e.g., ecology-geomorphology vs. economy-society. Similarly, by placing explanatory focus on the ongoing processes involved in the historically contingent intersections, the account discounts the grand discontinuities and transitions that are often invoked, e.g., peasant to capitalist agriculture, or feudalism to industrialism to Fordism to flexible specialization.⁸

7. Multiple, smaller engagements: Distributed agency, intermediate complexity, and the other features of intersecting processes have implications, not only for how environmental degradation is conceptualized, but also for how one responds to it in practice. Intersecting processes accounts do not support government or social movement policies based on simple themes, such as economic modernization by market liberalization, or sustainable development through promotion of traditional agricultural practices. They privilege multiple, smaller engagements, linked together within the intersecting processes.⁹

This shift in how policy is conceived requires a corresponding shift in scholarly practice. On the level of research organization, intersecting processes accounts highlight the need, in brief, for transdisciplinary work grounded but not localized in particular sites. They do not underwrite the customary, so-called interdisciplinary projects directed by natural scientists, nor the economic analyses based on the kinds of statistical data available in published censuses. In all these different ways, representing intersecting processes is inseparably bound up with engaging or intervening¹⁰ in a way that further extends the idea of distributed agency.

Case III. The simulated future of a salt affected agricultural region

The "Institute" is an economic and social research organization based in Melbourne, the major city of the southern Australian state of Victoria. The Kerang region, 240 kilometers north of Melbourne, is an agricultural region where farmers irrigate some pasture, which is grazed by beef or dairy cattle and sheep, and irrigate some crops. Soil salinization has been a chronic problem; during the middle 1970s, after some very wet years, the problem was acute. The rise in salinity, following a decline in beef prices, threatened the economic viability of the region. The "Ministry" of the state government overseeing water resource issues commissioned the Institute in late 1977 to study the economic future of the region. An agricultural economist from the Ministry and the principal investigator from the Institute formulated a project to evaluate different government policies, such as funding regional drainage systems, reallocating water rights, and raising water charges. This evaluation would take into account possible changes in farming practices, such as improvements in irrigation layout, drainage, and water management, and changes in the mix of farm enterprises. The analysis was to be repeated for different macroeconomic scenarios as projected by the Institute's national forecasting models.

The central part of the project was the construction of what came to be known as the Kerang Farm Model (KFM). Using an optimization technique called linear programming the KFM would determine for each of four composite representative farms the mix of farming activities that produced the most income. Different factors, such as water allocation, could be changed and the effect on the income and mix of activities ascertained. The division of labor in the project was as follows. The principal investigator, an econometrician, continued his work on the agricultural component of the Institute's forecasting model. The agricultural economist conducted extensive surveys of farm operations for forty farms and acted as liaison with two senior agricultural extension officers in the region who helped screen the production relationships and parameters used in the KFM. I was hired for fifteen months as a statistician and modeler to analyze the farm surveys and to construct and operate the KFM. The Ministry maintained oversight of the project through its agricultural economist and through regular meetings with the project team and an advisory committee.

The tangible products of the study included the survey and data analysis incorporated in one report to the Ministry, the KFM and economic analysis making up the second report, a technical monograph documenting the KFM, papers presented at two national conferences of agricultural economists, and a public meeting in the Kerang region to explain the results of the study (Ferguson et al. 1978, 1979; Taylor 1979). Although some refinements were omitted to meet the Ministry's deadline, the KFM was sufficiently flexible to allow evaluation of the required range of factors, yet not so complex so as to be unmanageable.

At the public meeting to present the study's findings some local agricultural extension officers raised objections to the study's having endorsed irrigation of pasture over irrigation of crops. This ran contrary to the advice they had been giving to farmers ever since the decline in beef prices. Subsequent reanalysis, incorporating generous increases in crop yields into the KFM's parameters, was completed rapidly. This showed the result favoring pasture irrigation was robust and could be attributed to beef prices having recovered by this time in the late 1970s. The Ministry, meanwhile, focused its attention simply on results indicating that water charges were not a primary limiting factor on farm enterprises or viability. These results eclipsed others concerning the larger range of options that the Institute had been commissioned to analyze, which suggests that justifying an increase in water charges had been the Ministry's primary concern all along.

This last outcome could engender or reinforce cynicism or fatalism about social impact studies commissioned by the authorities. However, if I

were able to show the ways in which particular aspects influenced the results, I would be identifying how the research could have been done differently. The possibility of identifying sites for possible modification of similar research informs the analysis to follow.

My entry point for analyzing the project will be around the modeling because that was the part that I, as a participant, observed more closely. I refer to myself in the third person as "the modeler" to express some distance between my position and actions in 1978–79 and my interpretive role today. I do not want to discount my observations and understandings as a participant, but it would be misleading to imply that during the Kerang study I had in mind a later analysis in terms of the sociology of science.

Building and Probing the Kerang Farm Model

Diverse components went into the KFM: data on soil quality, expected crop yields, range of farm sizes, technical assumptions used in the linear program, the status of the different agents in the project, the geographical distance between the Institute and the Kerang region, the computer packages available, the terms of reference set by the Ministry, and so on. Moreover, many of these components span the different realms of action of the various agents—from the modeler to the farmers—who are implicated in the building of the KFM. I need to put some order into this heterogeneity of components and assess their relative importance. Let me use my observations as the modeler to unpack parts of the processes of model building here.

Consider a central technical assumption in the KFM. The use of a linear program for economic analysis assumed that farmers operate to maximize one objective: in the KFM, this objective was income. Furthermore, the use of a linear program for policy formation assumed that if the optimal mix of farming activities according to the KFM were different from a farmers' existing mix, the farmer would change accordingly and immediately. Even though the economic future of the region obviously entailed the farmers' participation, the study did not investigate why and how farmers change, how directly and readily they respond to economic signs, or the extent to which any overriding economic rationality governed their actions.

The modeler questioned these assumptions. He expressed interest in techniques that incorporated more than one objective, but the principal investigator could not envisage modeling an alternative objective to income. In any case, software for multiobjective analysis was not available at the computer center used by the Institute. The modeler designed the

KFM to allow examination of the course over time of new investments needed, but when the project approached its deadline, this part of the model development was halted. The modeler learned of the existence of a sociological study on the factors influencing Kerang farmers to change their practices. This study had not, however, been released at that time and the principal investigator lent no institutional support to obtaining advance access to it. These and other issues were, he maintained, outside the economic specialization of the Institute and best left for others to deal with.

In affirming the technical assumptions in the KFM in response to the modeler's questioning, the principal investigator drew variously on his senior and permanent position at the Institute, the Institute's specialization in quantitative economic research, and the terms of reference and deadlines that the Ministry had set. These assumptions, in turn, had several consequences. They eliminated certain issues from investigation, e.g., farmer's objectives. They shaped the data that needed to be collected, e.g., obviating the need to investigate how farmers change. And they colored the relationships put into the model, e.g., the time course of investment became a secondary issue to locating the farming activities that optimized income. As an exercise in the authority of an experienced principal investigator over a young researcher, this was not at all extraordinary. Nevertheless, through such exchanges the principal investigator and the modeler were negotiating the different components of what would count as a representation of reality and a guide to policy formation.

Of course, there were parties other than the principal investigator and the modeler potentially involved in accepting or disputing the KFM. The farmers might have objected to the way their behavior was modeled. The KFM could also have been disputed by economists interested in multiobjective techniques, by sociologists interested in how people act, interact, and change, or by agricultural policymakers interested in having the study's results translated successfully into changes in the state of farming in the region. None of these potential disputes proved significant at the time. The farmers were separated from the formulation and operation of the KFM, and, conversely, the KFM was insulated from the farmers, by several considerations: by location (the modeling was performed in the city); through a chain of personnel (modeler–agricultural economist–senior agricultural extension officers–local agricultural extension officers–farmers); and by levels of abstraction and generalization. No one in the Institute, the principal investigator in particular, had training in multiobjective economic analysis or ready access to suitable computer software. There were no sociologists included in the project team or advisory committee. The Ministry, through the range of

options established in the terms of reference for the study, indicated that change would be initiated by government policy based on economic and engineering criteria. The farmers were, in effect, to be instruments, more than coparticipants, in determining the future of the region. In short, the Ministry did not dispute the KFM as a representation of reality; neither were any farmers, economists, or sociologists in a position to do so.

Six Heuristics Drawn from the Reconstruction of the Kerang Study

The description of the building of the KFM, although brief and clearly partial, is sufficient to introduce six propositions concerning the processes of science in the making and interpreting those processes. I begin with the observation that heterogeneous components from a range of realms of social action are being drawn on by the different agents involved in the KFM (proposition 1). Each of the other propositions follows more or less directly from the ones that have preceded it. These propositions are advanced heuristically, without expecting them to apply to all situations.

1. Science-in-the-making depends on heterogeneous webs, not unitary correspondence. From the description above, it is clear that diverse components were involved in building the KFM. Moreover, they were interconnected in practice, forming heterogeneous webs. The assumption that farmers were subordinate to economic rationality in the KFM facilitated the formulation of conclusions in the form of government policy options. The power of the government to enact its decisions rendered investigation of how farmers change less relevant, which shaped the data needing to be collected. Generalized agronomic data, rather than sociological insights, would suffice. This, in turn, conditioned the relationships that could appear in the model. Similarly, the modeler's mediated relationship with the modeled situation and his geographical separation from the region rendered it less relevant to model long-term options, such as selective reforestation and organic soil restoration. These possibilities, although potentially of economic and ecological benefit, would have required such things as experimental plots, publicity, education, advocacy, subsidized loans for tree planting from the government, and other institutional changes before they could be adopted. With so many contingent factors it was impossible even to estimate their costs. Omission of such options from the modeling, in turn, helped ensure that such aspects of the future reality would be less realizable, and the model's account more real. Figure 3 presents a schematic picture of diverse components interconnected in the making of the KFM.

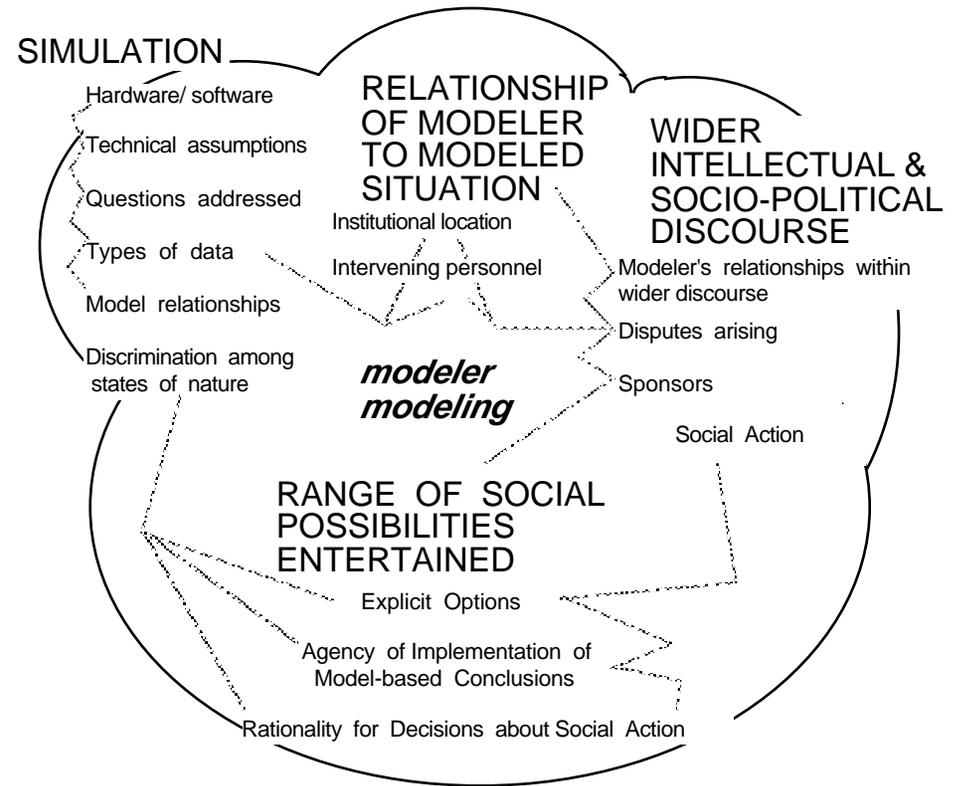


Figure 3. Different components of socio-environmental modeling. See text for discussion of their interconnections.

"Technical" considerations, such as the assumption of income optimization, and "social" considerations, such as the separation of the modeler from the farmers, had implications in practice for each other. "Local" interactions were connected with activities at a distance. For example, the modeler and the principal investigator decided not to pursue sociological inquiry into how farmers change, which meant that the content of and conduct of the survey of farms and farmers could remain unchanged. No one component in the web stood alone in supporting the KFM as a representation of reality; in the actual intersecting processes of building the model, technical components could not be detached from social ones, nor local ones from those that spanned levels.

In this sense I would say that science is constructed; science-in-the-making is an on-going process of building from diverse components, as in building a house from the ground up using concrete, bricks, cement, wood, nails, and so on. This is social construction, but not "merely" social construction. Moreover, the associations that social construction has with reflection are not apt here. It might be possible to say that the model reflected all the different social components, but it would be stretching the metaphor of reflection. The heterogeneity and interlinkage of the components make it difficult and uninformative to collapse science-in-the-making to a unitary idea of reflection of society in theory, or, similarly, to an issue of correspondence of theory to natural reality. In short, science, I would say, is heterogeneously constructed (Taylor 1995a).¹¹

2. Scientists represent-engage. In the process of building the model, the modeler, principal investigator, and other agents linked together technical and social components in order to make a model that worked for them. These scientific agents tended to make the different components reinforce, not undermine, each other, rendering both the model and the ongoing scientific activity more difficult for others to oppose or modify in practice (see proposition 1)¹². This insight goes beyond the observation that representations of natural reality support interventions in different realms of social action, or the claim that repeatable interventions provide the basis for scientific representations (Hacking 1983). Through the model's heterogeneous construction, representations and engagements were being formed simultaneously, and, moreover, jointly. "Interaction" between "technical" and "social" considerations fails to capture this relationship. Let me instead speak of scientists representing-engaging.

3. Scientists are practically imaginative agents. The idea of representing-engaging implies that scientific agents are mindful both of nature and of the social worlds in which they act, and that they project continuously between these realms. This attention to their social situatedness is not an accusation that scientists are corrupt, fallible, or lazily taking the path of least resistance. On the contrary, it is an affirmation of the view that all human activity is imaginative, that is, the result of a labor process that has to exist in the laborer's imagination before the process commences. Agents assess, not necessarily explicitly, the practical constraints and facilitations of possible actions in advance of their acting (Robinson 1984).¹³

Imagination in the sense I use it here is not at all like fantasy, in which worlds can be envisaged and mentally inhabited so as to escape from the practical difficulties of their realization. Achieving some result in the material world, in contrast to in fantasy, requires human agents to be

engaged with materials, tools, and, usually, other people. The KFM modeler had to engage with pasture growth, government sponsorship, an agricultural extension system, and so on. Moreover, materials, tools, and other people confront scientists with their recalcitrance. So scientists project themselves into possible engagements out in the world in order to imagine what will work easily for them and what will not. These constant projected confrontations with the components that personal and collective histories make available lie behind all the actions people take, including scientists' representing-engaging. Through them people build up knowledge—not necessarily consciously articulated—about their changing capabilities for acting in relation to the conditions in which they operate.

4. The agency of heterogeneously constructing agents is distributed. If we focus on agents' contingent and on-going mobilizing of webs of materials, tools, people, and other components, we can think of their psychology or agency as distributed, not concentrated mentally inside socially autonomous agents. That is, although agents work with mental representations of their worlds, the malleability of those representations should not be understood merely in internal mentalistic terms related to belief or rationality. Instead, we should inquire into the heterogeneity of resources that facilitate agents acting as if the world were like their representations of it. During the Kerang study, the principal investigator may well have believed deeply that economic decision making was of primary importance in people's lives. However, he was able to sustain this belief against possible challenges by many practical measures, such as not securing access to the sociological study on how farmers change, and concentrating on his econometric investigations rather than developing skills in multiobjective analysis.

5. Resources are causes. Up to this point in my description of the construction of the KFM, I have used the neutral term component to refer to the diverse things that scientists link into webs to support their theories and ongoing scientific activity. But there are many components linked together through the construction process that have little significance in explaining the development of theories and activity. The modeler, for example, used baking soda to clean his teeth at that time. But let me reserve the term resource for components that make a claim or a course of action more difficult for others to modify. Resources make a difference; that is, when resources are deployed they function as causes. In this light, the term resource cannot be used descriptively without also implying a claim about causes, and such claims invite analysis (see Taylor 1995a, appendix A).

6. Counterfactuals are valuable for exposing causes. With the exception just now of the baking soda, the components of the construction process I have chosen to mention were significant resources in the building of the KFM. Or so my account of the KFM would imply. But how can I support the causal claims that I have thus structured into my account of the KFM? For a start, let me note that, to support the causal claim that something made a difference logically requires an idea of what else could have been if the resource in question had been absent. There are many sources for ideas about what else could have been. Sociologists and historians of science listen to opposing parties in controversies (Collins 1981)—which include activists in movements for social change (Nelkin 1984)—undertake conceptual analysis or historical and cross-cultural comparisons (Harwood 2000), and give rein to their sociological imagination (Hughes 1971). Analyses of controversies have been popular; they provide the clearest, most concrete evidence of alternatives, because the agents themselves identify the resources they consider important.

There is no logical reason, however, why the resources explicitly exposed during a controversy constitute the full set used by a scientist. There are resources taken for granted and shared by opposing parties and, moreover, resources that must be mobilized even when there is no apparent controversy. In short, ideas of what else could have been should not be limited by whether anyone actually attempted to construct the alternative situation. For all these reasons, explicit use of counterfactuals may be needed in order to analyze a more inclusive array of resources used in the construction of science.

If we look back we can see that, although I began my account of the building of the KFM as a fairly neutral description, once I started to draw connections among the heterogenous components I began introducing counterfactuals. For example, in contrast to a single objective of maximizing income in the modeled farms, I mentioned the counterfactual possibility of multiobjective techniques. In explaining why this was not incorporated in the KFM, I mentioned that the principal investigator's training, his status relative to the modeler, software availability, and the Institute's specialization were invoked during the course of the study. These were constraints for anyone wanting to construct a multiobjective model. By identifying them I was implying that the principal investigator's training and so on were resources for constructing a model with a single objective function. In this general fashion, exploring the practical constraints on realizing counterfactual possibilities can, by a logic of inversion, expose the resources facilitating those who constructed what actually happened.

The emphasis on multiple, heterogeneous resources means that the relevant counterfactuals are multiple and particular. We could formulate an all-encompassing counterfactual, in which, for example, the Kerang study is replaced by a project that could not be used for top-down government policymaking. However, once we began to consider the practical implications of such a counterfactual, we would be challenged to identify specific sites for possible modification of the research. This would be all the more the case if we focused on the practical implications for the specific scientific agents involved. The modeler's ability to produce results based on sociologically realistic processes of change was constrained, as I observed earlier, by his distance from the farmers' realm of social action—distance given not only by location, but also by the chain of mediating personnel and degree of abstraction. The geographical and organizational distance was, in turn, related to the centralized character of government and intellectual activities in the one major city of each Australian state, something given by the previous 200 years of development. Towards the end of the project the modeler considered a move counter to that centralization, namely, to live and work in the Kerang region as an agricultural consultant. He was aware that this would raise practical issues such as purchase and maintenance of a car, long-distance access to computer facilities and libraries, keeping abreast of discussions about the wider state of the rural economy, and other considerations of a more personal nature. The modeler's decision not to move meant the representation of the Kerang region he was able to produce facilitated the making of policy based on simple economic grounds. This outcome did not flow from a political or intellectual commitment to the economically-based technocratic rationality; many practical, not only intellectual or ideological, considerations would have been entailed in producing a different result.

Conclusion -- Persisting tensions between concentrated and distributed agency

For the three cases in this essay I have provided overviews, in which the complexities have been smoothed or "disciplined." The emphasis, however, on heterogeneous resources and on intersections of processes at different scales and types highlights the range of agents whose different engagements jointly might contribute to modifying the focal outcomes and the social worlds intersecting around the development of those outcomes. As a result, the overviews do not privilege interventions from a superintending or master position.

I find the politics of distributed agency congenial, but recognise that a central question has been left open—what would lead any agent to engage so as to change the intersecting processes? Actually, no-one can simply continue to mobilize the same resources as previously, because the contingent intersection of different processes ensures on-going change and restructuring. So the open question becomes what would lead any agent to try self-consciously to steer the restructuring in certain directions over others? One kind of answer would return us to concentrated agency, in that we could point to the agent's goal, such as preventing illness, soil erosion, and the production of models whose results can be manipulated by policy makers, or convincing readers of the virtues of an intersecting processes framework. A different kind of answer, more consistent with the spirit of this essay, would stem from investigating the intersecting processes that have formed the particular agents in question (analogous to the first case, but without the focal outcome of a mental *illness*). Indeed, the Wolfian heuristic would have us subsume the first kind of answer into the second. That is, "goals" become discursive shorthand for the particular intersecting processes of different agents. At the same time, to the extent that agents need to explain their actions to others—and to themselves—when they attempt to mobilize different resources or organize them in new directions, such discursive themes may be valuable resources.

The image that emerges is one in which agents are always "vibrating" among their experiences of concentrated and distributed agency. The challenge becomes to acknowledge the discursive impact of simple themes, but to strengthen the vibrations in the direction of agents attending to their dependency on other people and many, diverse resources beyond the boundaries of their physical or mental selves (Taylor 1999a).¹⁴ The intersecting processes/heterogeneous construction framework introduced in this essay clearly highlights distributed agency, but it would be better for my case if I could move beyond text and argument, to lead my audience into positive experiences of their distributed agency in establishing knowledge and engaging in change (Taylor 1990). To this end, the workshop processes developed by the Institute of Cultural Affairs (ICA) have become my model.¹⁵ I could try to evoke the experience of ICA processes, but I am going to leave it for interested readers to gain this experience first hand.

Coda -- Evolution in a context of unruly ecological complexity

Given this open-ended conclusion, my argument for pursuing and promoting the experience of our distributed agency cannot on its own be expected to move readers to change their work and ideas. Moreover, I

have not explicitly bridged the gap between the the immediate focus of DST on development and evolution and the areas addressed in this essay. I offer this coda, therefore, on theorizing ecological complexity to nudge DSTheorists in the direction of exploring the kinds of intersecting processes I have highlighted. Indeed, my own interest in intersecting processes grew from a search for ways to theorize the complexity of ecological dynamics. Along the way I also observed that the structure and dynamics of this ecological context have not been well integrated into developmental and evolutionary theory.¹⁶ The challenge of doing so needs eventually to be addressed—after all, all development and evolution occurs within a dynamic ecological context (Taylor 2001a).

During the last decade or so a reassertion of historical contingency, non-equilibrium formulations, local context and individual detail has subdued the ambitions many ecological theorists had in the 1960s and 70s for identifying general principles (Taylor 1992, Kingsland 1995). Ecologists have become increasingly aware that situations may vary according to historical trajectories that have led to them; that particularities of place and connections among places matter; that time and place is a matter of scale that differs among species; that variation among individuals can qualitatively alter the ecological process; that this variation is a result of on-going differentiation occurring within populations (which are specifically located and inter-connected); and that interactions among the species under study can be artifacts of the indirect effects of other "hidden" species.

In patch dynamic studies, for example, the scale and frequency of disturbances that create open "patches" is now emphasized as much as species interactions in the periods between disturbances (Pickett & White 1985). Studies of succession and of the immigration and extinction dynamics for habitat patches pay attention to the particulars of species dispersal and the habitat being colonized, and how these determine successful colonization for different species (Gray et al. 1987). On a larger scale such a shift in focus is supported by biogeographic comparisons which show that continental floras and faunas are not necessarily in equilibrium with the extant environmental conditions (Haila & Järvinen 1990). From a different angle, models that distinguish among individual organisms (in their characteristics and spatial location) have been shown to generate certain observed ecological patterns, such as patterns of change in size distribution of individuals in a population over time, where large scale, aggregated models have not (Huston et al. 1988, Lomnicki 1988). And, the effects mediated through the populations not immediately in focus, or, more generally, through "hidden variables," upset the methodology of

observing the direct interactions among populations and confound many principles derived on that basis (Strauss 1991; Taylor 2001b).

To incorporate this new, or perhaps resurgent, emphasis, it has been suggested that ecology be conceived as an “historical” science (Ricklefs & Schluter 1993). Like the fields of epidemiology, psychoanalysis, structural geology, paleontology and history proper, ecology faces the challenge of historical explanation: how to assemble a composite of past conditions sufficient for the subsequent outcomes to have followed, while, at the same time, not obscuring the provisional quality such accounts have, their being subject to competition from other plausibly sufficient accounts (Taylor 1987). The phrase “a composite of past conditions” could conjure up pure historical contingency, but I do not mean this. Like the accounts of intersecting processes in this essay, historicity in ecological thought should preserve a place for regularities or structuredness of ecological patterns and processes. To say that ecological structure has a history is to say that it changes in structure and is subject to contingent events, while at the same time it constrains and facilitates the living activity that constitute any ecological phenomenon in its particular place. The challenge facing ecology then is to theorize particularity and contingency intersecting with structure, and of that structure changing in structure, being internally differentiated and, because of overlapping scales of different species' activities, having problematic boundaries—in short, to discipline, without suppressing this “unruly complexity” of ecological processes (Taylor 1992, 2001a, b).

It is within such unruly ecological complexity that organisms, for almost four billion years, have constructed their living and “evolved,” that is, given rise to descendants that differ from them. It ought not be assumed that the ecological context remains consistent, that is stable or repeatable, with respect to evolution occurring in populations of individuals or, as in DST, of life cycles of organisms and resources (Gray 2001). Although consistency of context may sometimes be the case, the relevant processes are not necessarily separable into “ecological” and “evolutionary” time scales (Taylor 2001a).¹⁷ The challenge then for DSTheorists is—as researchers conditioned by intersecting scientific and social processes—to make sense of the appearance of organisms as intersections of ecological, developmental, and evolutionary processes.

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Notes

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¹ Two comments on terminology: i) In other publications I use the term "system" or "strong system" to denote structures or units assumed to have dynamic unity and structure (e.g., Taylor 1988, 1992, 1998, 2000b), but, given the distinctions and arguments Oyama (this volume) makes about the connotations of system in DST, I have chosen not to cast the term in a critical light in this essay; ii) I use the term process in the sense of sequences of events that persist or are repeated sufficiently long for us to notice them and need to explain them. This contrasts with an essentialist sense of process as a basic underlying causal structure that allows people to explain events as instances of the process or as noisy deviations from it. "Maturation," "modernization," "population growth" are examples of the latter sense of process.

² Portions of this essay are adapted from other publications, with acknowledgement of the respective publishers: parts of Case I appeared in Taylor (1995a; University of Chicago Press); Case II, in Taylor (1999; Oxford University Press); Case III, in chapter 4A of Taylor (2000b; University of Chicago Press); and the coda, in Taylor (1997a; Taylor & Francis). Although I have developed these cases without explicit reference to DST, I have benefitted from conversations with this volume's editors since the middle 1980s. I welcome this opportunity to let readers consider the convergences between DST and ideas arrived at along some different paths.

³ Indeed, when we search for new concepts and metaphors, or more generally, use words and text to make arguments and seek to convince others, we privilege three related and persistent "meta-metaphors": "1) metaphors are root, fundamental, underlying things that shape the surface layers; 2) mental things—thoughts, expectations, what we see—shape our actions; and 3) culture or society get into these thoughts (and so we can be taught [or argued into] how to conceive/perceive the world" (Taylor 1997b, p. 222, note 37). These meta-metaphors discount our experience of thought being constructed in practical activity from diverse resources.

⁴ Although associations between life events and difficulties are also studied in the United States, conventional quantitative epidemiology still dominates that research (Brown and Harris 1989, p. x-xi, 3-45). Associations are thought of in statistical terms, that is, as if causality were a matter of adding up separate "effects" (see note 6 below and Oyama, this volume). In contrast, as the text and Figure 1 to follow indicate, Brown and Harris focus on the development of life histories and the contingencies involved.

⁵ Figure 1 is adapted from Bowlby (1988, p. 177). His schema is, in turn, adapted from Brown and Harris (1978, p. 265). The hypothetical genetics/biochemistry strand is my addition. Its significance will become clear in due course.

⁶ The nonpartitionability of different kinds of biological and social causes, given the interdependence of their effects, is demonstrated well by Lewontin (1974), when he argues that statistical partitioning of effects ("analysis of variance") does not constitute an analysis of causes. Of course, partitioning of biological and social causes does have ideological meaning (Lewontin et al. 1984).

⁷ The combination of differentiation, historical contingency and structuredness distinguishes this intersecting processes view of socio-environmental change from Vayda (1996). Although his approach shares many qualities with mine, he is more particularist and sceptical of theory based on social structures or structured processes.

⁸ Such discontinuities and transitions often rely on the sense of process that I want to avoid; see note 1, point ii).

⁹ In the Oaxacan case, the changes of past centuries cannot be undone, but a more fine-grained intersecting processes analysis focusing on recent decades would expose a range of potential engagements—from Non-Governmental Organizations promoting conservation of traditional cultivars to efforts to redirect international financial policies so as to support, rather than reduce, rural credit at the local level (DeJanvry & García-Barrios 1989).

¹⁰ See Case III, heuristic 2.

¹¹ If we think of construction in terms of sequences of diverse, multiple causes we can reject the terms of the realism-relativism dichotomy persisting in explanations of the course of science in similar ways to the argument against nature vs. nurture from case I. Let us read genes as underlying, "mind-independent" reality and the environment as social influences on science. If the outcome (mental illness, or, by analogy, some aspect of science, e.g., an established theory) is the result of many heterogeneous components linked in a process, in which each step builds on the outcomes of the previous steps, then it is difficult to partition relative importance or responsibility for an outcome among the different contributing causes, or components

in linkage. It becomes quite difficult to give meaning to determination by either nature or society, and not very helpful besides. Heterogeneous constructionism would, therefore, lead us to conclusions analogous to the three stated in case I for the nature-nurture debate:

1. Suppose there are fundamental principles of nature that are difficult or impossible to modify (a tenet of scientific realism). This does not imply that this deep reality predominantly or ultimately governs the actions of scientific agents, in particular, their success in establishing some representation of this reality. Moreover, the reliability of certain science-based interventions in the world (also important to most scientific realists) is interesting and worth explaining, but it does not justify the belief that sound scientific method is the most efficacious route to exposing any unreliable knowledge or eliminating problems in correspondence between theory and reality.

2. Critics of scientific realism do not need to claim that construction is entirely a matter of social influences, conventions or personal beliefs. Heterogeneous constructionism addresses the malleability or immalleability of scientific knowledge without entailing such relativism.

3. Sociology of science's analyses of methodology, interests, and rhetoric are illuminating, but not strictly necessary in the conceptual critique of scientific realism. More needs to be said to argue these propositions, but not here (Taylor 1995a, b); after all, analogies are meant to open discussions more than close arguments.

¹² The work of Latour (1994) and Pickering (1995) shares with this essay an emphasis on process and scientific practice. Latour explores a Whiteheadian process metaphysics for science studies. Heterogeneous construction might be read as Pickering's "mangle" and "impure dynamics," and the imagination of scientists as his "modeling." In contrast, however, to my goals of explanation and spanning levels of social practice, Pickering develops a position opposed to social construction and causes and he theorizes practice mostly in terms of experimental practice. That is, it is

mostly because scientists tinker with tangible objects, whose resistance requires accommodation, that their goals and interests are subject to ongoing revision. Studies of practice in that sense are reviewed by Golinski (1990); see also Pickering (1992).

¹³ Associating imagination and the labor-process is Marx's idea. See Capital, vol. 1, pt. 3, chap. 7, sec. 1, reprinted, e.g., in Tucker (1978, pp. 344–45). The convention in social studies of science has been to avoid reference to an agent's psychology for fear of shifting the terms of explanation from the social realm to an unobservable realm of the agent's mind. I find dubious both the equation of social with observable and the empiricist rejection of unobservables, but, in any case, notice that imagination relies on a distributed, not an internal, notion of mind and psychology. Furthermore, psychological or cognitive models of the scientist as social agent are implicit in every explanation of the outcome of scientific activity. For example, Latour (1987) depicts scientists building "networks" in response to the stimulus of others building competing networks, and assumes that scientists seek to accumulate resources, all of which results, if successful, in "centers of calculation," "obligatory passage points" (Callon 1985), and their becoming macroactors (Callon and Latour 1981). Like the psychology of pigeons in the accounts of behaviorists, the psychology implied is both strong and minimal—the scientists are governed only by this egocentric metric of resource accumulation; they are not assumed to have multiple projects in their lives and work. This, like most other models of psychology and rationality implicit in social studies of science, is quite restrictive, even when rationalized as a methodological tactic to highlight the flexibility of agents' actions and network building.

¹⁴ In this project I am inspired by DST-like work on the development of the self in relationship to others (Fogel 1993) and to the "intentional scaffolding" others provide (Hendriks-Jansen 1996).

¹⁵ ICA workshops elicit insight from a large range of participants in analyzing a situation and usually lead to plans that no one participant has envisaged beforehand and that the participants are invested in carrying out. This is achieved by a neutral facilitator leading participants through four phases—objective, reflective, interpretive, decisional—a structure best represented in "focused conversations" (Spencer 1989, Stanfield 1997). For an elaboration of the basic propositions of ICA facilitation and group process, see <http://omega.cc.umb.edu/~ptaylor/ICApropositions.html>, which is adapted from workshop materials of ICA Canada; see <http://www.icacan.ca/>.

¹⁶ In a sense ecological dynamics are implicit in any evolutionary theory, but with "genetic (transmission)," "developmental," "ecological" and "evolutionary" time scales theoretically separated (Taylor 2000a). See note 17.

¹⁷ Laland and Odling-Smee (this volume), who extend the important emphasis of Lewontin (this volume) on organisms constructing the environments, recognize that the persistence of a constructed environment ("ecological inheritance") conditions subsequent evolution in the constructing species and others. Notice, however, that Laland and Odling-Smee do not otherwise theorize the dynamics of environmental change or explore the significance of those dynamics for the theory of natural selection (Taylor 2000a).