

## Four Mechanisms for Finding (and Being Found by) Research Problems

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Sociology faces outward: it addresses problems in the external world and uses the tools of science to subdue them. We have no William James, who developed a viable psychology (in part) by reflecting on his own inner experience, nor a Piaget, who made many observations on which his seminal development theory was based from the comfort of his children's nursery. Thus, an opportunity to reflect on my own intellectual development — to play Piaget to my own Jacqueline, Laurent and Lucienne — is a treat and a challenge.

I shall do the best I can, but I would warn any student who hopes to draw guidance from this account to proceed with caution, for two reasons. First, my experience is at best only relevant to one type of scholar. Isaiah Berlin (1953) famously elaborated upon Archilochus's distinction between the fox (who knows many things) and the hedgehog (who knows one big, important thing). I am far at the vulpine end of the fox-to-hedgehog continuum, at times to the point of intellectual ADHD. I'm not sure I'd recommend this approach. When it works, others take up a question or idea and push it forward on their own, saving one the trouble. When it doesn't, promising lines of research wither on the vine; and, in any case, one spend much more on intellectual start-up costs than one's more erinaceous peers.

The second reason applies to any essay of this kind. Neuroscientists who study people with brain injuries have found that they can produce elaborate and plausible accounts of their behavior even when they are unaware of its causes (Gazzaniga, 1989). The reader is warned that the human talent for producing dubiously coherent accounts reaches its pinnacle in autobiographical reconstruction. To constrain my capacity for unintentional confabulation, I have assembled a data set consisting of my published papers, books, and book chapters, to which I shall refer throughout.

## 1 Four Mechanisms

Four mechanisms have led me to research problems: experiential; semi-stochastic; stochastic; and crecive. Experiential problems come about when you seek explanations for puzzles that life presents. Stochastic problems come out of the blue, often but not always at the initiative of others. Semi-stochastic problems emerge directly from the last thing you were thinking about, with no direct history before that. Crecive problems are itches that demand scratching: theoretical or methodological dilemmas that one has encountered in multiple research projects and can no longer avoid. After describing the mechanisms at greater length and discussing how they have figured into my own work, I will test a few hypotheses about the relationship between mechanism and reception on a data set comprising 96 of my own books, book chapters, and journal articles.

### 1.1 *Experiential*

This mechanism was most influential at the beginning of my career; or at least it seems that way, probably because most of my experience since graduate school has been as a professional sociologist (this is not as sad as it sounds), which makes it difficult to distinguish this mechanism from the others. I classify 5.3 percent of my publications as experientially based. One of several possible accounts of why I became a sociologist begins in Nashville, Tennessee, where good fortune landed me after college, having used my Sociology BA to get a job as a research assistant to Richard Peterson, then in the midst of his seminal work on the production of culture. For a semi-pro rock critic and aspiring songwriter like me, getting to work with a gifted and experienced scholar who was as into music as I, not to mention meeting song publishers, recording artists, and charter members of the Grand Ole Opry, pretty much sealed my fate, and led to my first three publications and an extended interest in the production of culture.

An equally plausible account of my sociological origins begins in a fifth-grade classroom in Swarthmore, Pennsylvania, where I undertook observation during my first year in my college's teacher certification program. At the time, my school was part of a district that served children from two very different communities: Swarthmore, a prosperous suburb populated by high-income professionals that boasted the highest incidence of BMWs in the state; and Morton, a blue-collar town, equally monoracially white, where most employed parents worked making helicopters in the Boeing-Vertol plant or in small businesses that served the former. As it happened, this made my classroom a privileged site for looking at class differences in

education: It consisted of two groups of children with different local identities, strongly differentiated by social class, more or less randomized by gender, and racially and linguistically homogenous.

The first thing I learned was that some kids were smarter than others: They were more attentive, they answered questions more eagerly and more accurately, they had better vocabularies, and they just *seemed* smarter. The second thing I learned was that the kids who seemed smart were almost all from Swarthmore and the kids who seemed less able were almost all from Morton. The third thing I learned, after observing for a while and getting to know the kids, was that the first and second things I learned were wrong. The little Swarthmore boy with the constantly upraised hand was more anxious than gifted, and the silent and sullen Morton girl turned out to be the most cognitively sophisticated and articulate in the class, though a difficult family situation kept her from concentrating on academics. How was it, I wondered, that social class predicted my initial impressions of intelligence so perfectly, while predicting the underlying construct (as revealed by longer-term observation) so poorly? Could it be that something about the home-school fit made middle-class kids more able and motivated to *perform* being smart, even in the absence of underlying differences in intellectual acuity?

In my first year of graduate school, I described my experience to one of my professors, Mike Useem, who mentioned that a young French sociologist, Pierre Bourdieu, had been doing interesting work, as yet untranslated, that I would find instructive. I read everything I could find by Bourdieu, as well as Bernstein on language, Bowles and Gintis on school structure, and Collins on credentialism and educational expansion, and ended up writing a dissertation on social class, aesthetic taste, and school success that generated some of my earliest papers (e.g., DiMaggio, 1982). I have continued to work on related topics, though with declining frequency, ever since.

## 1.2 Semi-Stochastic

Much of the time (45.6 percent of papers) my intellectual progress has been semi-stochastic, in that each project is influenced by a previous one, while lacking direct connection to any that preceded it. At times, a project or paper leads in this way to several others, which may therefore give an impression of planfulness; but more often, this pattern produces a multi-tendrilled tree, in which any given project may generate several new directions. As a result of my interest in class, culture, and education, and drawing on earlier work with Richard Peterson on how the center of gravity for the country music audience had shifted from southerners to the white working class (Peterson & DiMaggio, 1975), I collaborated with Michael Useem on a meta-analysis of the U.S. audience for the arts, supported by the National Endowment for the Arts. The Endowment asked that we go beyond synthesizing findings of audience research by arts organizations to studying why the research was undertaken and how the results were used (Useem & DiMaggio, 1978). Answers to the first question (managers perceived that opportunities for government funding would increase and that such opportunities would require them to diversify, or at least to pretend to care about, the composition of their audiences) led me to research on organization-environment relations. Answers to the second (the results were used symbolically, used to stimulate conversation, or not used at all, and their perceived utility was unrelated to the research quality) led me to Carnegie School research on limited rationality and organized anarchies. Together, working on these papers cultivated a career-long engagement with organization theory, a field in which I had previously had little interest.

The branches that grew out of this project were several and diverse. Taking an organizational perspective on audiences encouraged me to think about alignment between class and aesthetic taste as itself a product of organizational systems, which led to historical work on the origins of high culture (DiMaggio, 1982) and on developments in public cultural policy that seemed responsible for the managerial jitters Useem and I had noted (DiMaggio, 1991). It also led me to study the managers themselves to understand how institutional change occurred through demographic succession and changing recruitment channels (DiMaggio, 1987). Because several fields I studied were rife with contention over these new directions, it made sense to use network analysis to examine the structure of the managers' professional affiliations; this in turn led to an engagement with network analysis (which I had studied in graduate school but had not yet used) and, more broadly, with clustering and subgroup-detection algorithms, that has continued through the present (DiMaggio, 1986).

### 1.3 Stochastic

Some projects or papers (32.2 percent) come to one out of the blue, with limited relation to anything that has preceded them. At times, the source is an organizational role. My involvement with Yale's Program on Non-Profit Organizations led to a series of papers on the nonprofit sector (e.g. DiMaggio & Anheier, 1990); and my engagement with Princeton's Center for Cultural Policy Studies to several papers on basic data resources for studying cultural participation and nonprofit arts organizations (Kaple, Rivkin-Fish, Louch, Morris & DiMaggio, 1998) — not necessarily at the top of my personal agenda, but important tasks that our Center was best positioned to carry out. My participation in the General Social Survey led to work on a module of questions on economic behavior, which generated a paper on a new topic (albeit one of longstanding interest), the role of personal networks in consumer and housing markets (DiMaggio & Louch, 1996).

Many papers (55.3 percent) that I classify as stochastic are among the 39.6 percent of publications that resulted from invitations rather than emerging out of my own research programs.<sup>1</sup> Some of these stem from opportunities that are too interesting to decline. In general, one should decline them anyway, but at times the temptation is too great and the results are fruitful. A Social Science Research Council working group on New York's centrality in the urban network led to my only publication in urban sociology and the only one on power (DiMaggio, 1993). A fortuitous conversation with Filiz Garip, then writing her dissertation at Princeton, led to our joint realization that apparently different problems we were each tussling with might have a common solution, resulting in several papers about network externalities and economic inequality (DiMaggio & Garip, 2011). More recently, participating in a symposium about an outstanding paper on rational action and institutions provided an excuse to review the role of saloon keepers in politics during the Gilded Age and undertake a mini-project on Southern Senators' connections to slavery and subsequent political careers in the 1840s: departures from any reasonable research trajectory, but thoroughly enjoyable nonetheless (DiMaggio, 2017).

### 1.4 Crescive

So far, I have avoided the language of choice in describing the way projects and I find one another. The *crescive* mechanism, which accounts for 6.3 percent of the publications, represents the exception. There are problems that nag at one for a long time, compelling one's attention until one finally does something about them. Such problems may present themselves as theoretical or methodological. But I prefer to call them "theorodological" because behind every really knotty theoretical problem is a methodological challenge (because the solution often requires a new way of looking at data) and beneath any good methodological problem are theoretical choices.<sup>2</sup>

Toward the beginning of my career, most of these problems presented themselves as theoretical. In recent years, they have been largely methodological. My most cited paper, "The Iron Cage Revisited" with Woody Powell, was something like this: I had been studying small community-based arts organizations that sought to combat class biases in access to the arts; Woody had been studying small publishers who tried to find alternatives to commercialism; and we were both interested in collectivist organizations in other fields that sought alternatives to bureaucracy. Our collaboration was an effort to understand why mission-driven organizations had so much trouble remaining distinctive, often ending up resembling the very bureaucratic organizations to which they had hoped to be alternatives (DiMaggio & Powell, 1983). Second example: In describing the experiential mechanism, I noted the influence, first, of my engagement with popular music and, second, of my experience in the Swarthmore schools. In graduate school I pursued these interests separately, which led me to feel as if I was writing under two personas: An organization theorist who studied artistic innovation, and a Bourdieuan student of cultural reproduction. An invitation from Howard Becker to prepare a paper for an ASA thematic session enabled me to fuse these two

1. The distinction is not always clear-cut, in that at times I have responded to invitations with papers that I had already written or wanted to write. I classify as "invited" only those papers that I would have been unlikely to have written were it not for the invitation. Surprisingly (to me at least), the percentage has been stable over the course of my career.
2. Princeton University's Center for the Study of Social Organization sponsored a monthly "Theorodology Workshop" for several years and presented "Theorodology Awards" to Andrew Abbott, Mike Hannan, Arthur Stinchcombe, and Harrison White for lifetime contributions at the intersection of methods and theory.

intellectual halves, by thinking seriously about the way in which systems of cultural classification interact with social organization and the ways in which formal organizations affect cultural hierarchies (DiMaggio, 1987). Finally, from early in graduate school I noticed that theories of social reproduction were based upon constructs like the “correspondence principle” (Bowles & Gintis, 1976) or *habitus* (Bourdieu, 1977), that made strong assumptions about social cognition without reference to research in social or cognitive psychology.<sup>3</sup> At the time, psychologists, in recovery from behaviorism, provided little guidance; but the problem of finding a cognitive grounding for the sociology of culture continued to fester until psychologists finally provided suitable materials (DiMaggio, 1997).

More recently, a search for methods appropriate to theoretical intuitions has motivated much of my research. Given the magnificent diversity of the social world, and people’s facility for assembling beliefs and sentiments into surprising packages, I have long felt that studies of beliefs and attitudes too often assume that one model fits all. If we believe, as I do, that people organize the same ideas in very different ways and that, further (and more controversially), such differences in domain construals can rarely be reduced to groups defined by one or even two social identities, one needs an inductive method to detect population heterogeneity in attitude and opinion data. In this case, my major contribution was assembling a strong research team and scheduling repeated meetings until Amir Goldberg (2011) came up with a brilliant solution, Relational Class Analysis (RCA), which we and others have used to good effect to examine heterogeneity in several domains (DiMaggio & Goldberg, 2017; DiMaggio, Sotoudeh, Goldberg, & Shepherd, 2018). Similarly, inspired by Bakhtin’s work on multivocality, I long searched for ways to identify multiple voices, frames, or themes in texts. The work on RCA led to meeting a computer science colleague, David Blei, who had created a text-analysis program, LDA topic models, which did just that. The conversation led to a joint paper on the utility of topic models for cultural analysis (DiMaggio, Nag, & Blei, 2013) and ongoing work in that vein.

### 1.5 Do Some Mechanisms Lead to More Influential Papers than Others?

To address this question, I conducted an OLS regression analysis in which the dependent variable was logged citations (given dispersion in the raw count) and where independent variables included dichotomous mechanism indicators (stochastic omitted) and a vector of controls.<sup>4</sup> Raw citation totals are a poorer measure of influence than weighted centrality measures, but the latter were unavailable. And “influence” via citations may be a less important indicator for someone reflecting on or planning a scholarly career than, for instance, personal satisfaction with a research product or even enjoyment in its production. But without having much sense of the stability of subjective measures or the extent to which they may have been contaminated *post hoc* by knowledge of citations counts, I stick with the more conventional indicator.

The model predicted a respectable amount of variance in logged citations (33.04 percent) even without distinguishing journal quality (which would have added explanatory power) or exploring interaction effects. Because older papers had more years in which people could cite them, I assumed that age would be associated with number of citations, and it was significantly so. Books and chapters received slightly more citations than papers ( $p < .10$ ). Papers on economic sociology, organizations, stratification and the Internet were cited more than papers on culture, networks, social attitudes and nonprofit organizations.

A rational-agent-centered model would expect a scholar’s best work to be internally motivated, and to emerge from long-term engagements with tightly related research questions. From this perspective, we would expect invited papers to have less impact than papers for which the motivation is internal, and papers with a history, even a semi-stochastic one, and, *a fortiori*, those of crecive origin, to have more impact than those whose origins are purely stochastic. A pragmatist might also expect experientially-based papers to be

3. Basil Bernstein (1973), who grounded his claims in his research on sociolinguistics, was the notable exception.

4. I compiled citations from my *Google Scholar* personal page, using the regular *Google Scholar* listings to add data on several papers that are omitted from my personal listing, summing references to the same paper under slightly different names, and cross-checking across the two sources to try to minimize error. That said, it is clear that *Google Scholar* introduces considerable noise to the measure. Independent variables were years from publication, authorship (1-sole authored, else 0), dummy variables for books and chapters (journal articles omitted), whether the publication stemmed from an invitation rather than my personal agenda, and binary variables indicating whether the paper dealt with topics in culture, organizations, networks, economy, internet, attitudes, or nonprofits (with many papers falling into more than one category). Full results available on request. Analyses were conducted using Excel 2016’s regression utility (from the Data Analysis Add-In).

especially fecund, and someone vainer or with more faith in individual agency than me might predict that sole-authored papers would surpass multi-authored ones.

Table 1 – Predictors of Logged Citations to DiMaggio's Publications (Selected Variables Only)<sup>5</sup>

	Unstandardized <i>Coefficient</i>	<i>p</i>
Experiential Mechanism	.130	.734
Semi-Stochastic Mechanism	-.153	.372
Crescive Mechanism	.898	.011
Sole-authored	-.022	.775
Invited	-.551	.008
N	96	
Adj. R <sup>2</sup>	.330	

Pragmatists may not be happy with these results, nor will agency enthusiasts with a heroic view of the lone scholar. Neither experientially based nor sole-authored papers have been cited more than others. (And, for multi-authored papers, it didn't matter whether or not I was first author.) Pragmatists and believers in agency will take heart, however, in the success of papers that emerged crescively through repetitive encounters with obdurate problems. The poor performance of invited papers, especially compared to crescively generated ones, provides strong support for the importance of doing what one feels is important. At the same time, there is little support here for a rational model of path-dependent problem selection, as papers that flowed logically from prior research received no more citations than stochastically generated publications. Discipline is important, but the stochastic mechanism introduces quasi-random elements into one's intellectual life in ways that can be productive, entertaining, or both.

So what lessons can we take from this? Probably none, as citations are a rough estimate of influence and a poorer indicator still of satisfaction; and, moreover, the model just explains variance among one sociologist's publications; and it is not at all clear that the results would be robust to a different distribution of mechanisms. Gathering similar data from a few hundred other sociologists (especially with some hedonic measures) might identify modal patterns, each with its own distinct correlates of relative publication success. Until then, it is probably best to understand one's intellectual predilections, do work that addresses problems one finds challenging and important, and hope that enough other people agree to support one's research habit for six or seven decades.

5. Dependent variable from *Google Scholar*. Omitted mechanism is "stochastic." Excludes reviews, working papers, reports, and ephemera. Model controls for years since publication, and substantive topic.

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