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Field Analysis and Interdisciplinary Science: Scientific Capital Exchange in Behavior Genetics

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Abstract This paper uses Pierre Bourdieu's field theory to develop tools for analyzing interdisciplinary scientific fields. Interdisciplinary fields are scientific spaces where no single form of scientific capital has a monopoly and therefore multiple forms of scientific capital constitute the structures and stakes of scientific competition. Scientists compete to accumulate and define forms of scientific capital and also to set the rates of exchange between them. The paper illustrates this framework by applying it to the interdisciplinary field of behavior genetics. Most behavior geneticists envision their participation in the field as a means to compete for scientific capital in other fields. However, the scientific capital of behavior genetics has different values for scientists attempting to deploy it in different neighboring fields. These values depend on situations in each field and the ways behavior genetics mediates relationships among them. The pattern of relationships of exchange helps explain the social hierarchy and several features of knowledge production within behavior genetics.

Keywords Bourdieu · Field theory · Scientific capital · Interdisciplinary science · Behavior genetics · Knowledge production

Introduction

This paper will use Pierre Bourdieu's (1975, 1991, 2004) field theory to develop tools for analyzing the practices of scientists in interdisciplinary scientific spaces. Specifically, I focus on the concept of "scientific capital," the form of symbolic capital specific to the sciences and, according to Bourdieu's framework, the main

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object of competition among scientists. Bourdieu strongly emphasizes that scientific disciplines each have their own forms of scientific capital. I extend this framework to interdisciplinary sciences by suggesting that these be conceived as spaces where scientists pursue and exchange multiple forms of scientific capital with none enjoying a monopoly over others.

One reason science studies scholars have received Bourdieu's theory of science coolly is that it seems to turn on the notion of scientific autonomy. Helene Mialet puts the point sharply:

> Thus, though we agree with Bourdieu that science is 'in danger' and is 'dangerous', this is not because it is losing its autonomy, but because some (Bourdieu, for example) believe it to be autonomous, want it to be so, and continue to perpetuate this idea. The new sociology of science, on the other hand, strives to show the multiplicity of sciences' attachments. (2003, p. 619)

Thus, Bourdieu's framework might seem a quixotic starting point for analyzing interdisciplinary science. Whatever interdisciplinary science is, and the literature is hardly at a consensus (Jacobs and Frickel 2009), "autonomous science" is not on the list.

Given the polemical and uncharitable way Bourdieu has attacked the science studies literature (2004), and the rationalist and normative aims that animate his writings on science (1975, 2004), it is not surprising that science studies scholars have been mostly unwilling to read past the polemic to find the analytic core that could be useful to them. However, the current paper aims to join an emerging literature that takes a more pragmatic stance toward Bourdieu's work on science (e.g., Kim 2009, Albert, Laberge, and Hodges 2009). While engaging Bourdieu's epistemic and political claims are important tasks, one need not take this on to use his analytic toolkit. Against Mialet and others, I aim to show that Bourdieu's theory provides powerful resources for showing "the multiplicity of sciences' attachments."

A central irony of the literature on interdisciplinary science are the particularly limited ways of considering the tensions and dynamics produced by the fact that interdisciplinary fields mix scientists from different backgrounds. This issue has been addressed obliquely in four main ways: 1) the low status of interdisciplinary fields relative to disciplinary fields and the resulting costs to scientists for participating (Jacobs and Frickel 2009, esp. p.51); 2) the necessity of interdisciplines to maintain porous boundaries in order to enable scientists to maintain ties within and beyond science (Frickel 2004); 3) differences in disciplinary background as barriers to cognitive integration and mutual evaluation (Klein 1996, Hackett and Rhoten 2009, Lamont 2009, Lamont, Mallard, and Guetzkow 2006); and 4) conflicts that emerge in mixed-discipline research teams (Albert et al. 2008, Albert, Laberge, and Hodges 2009). The first two strands of research acknowledge that interdisciplinary fields are situated in relationships with other fields, but they do not really trace how these relationships affect practices within the interdiscipline. The last two strands focus on practices within interdisciplines but they have very static accounts of the effects of external contexts, describing them as supplying set cognitive frameworks and judgments.

As I will show, Bourdieu's analytic framework brings several crucial elements to the topic of interdisciplinary science. First, conflicts and struggles for power are essential properties of all fields rather than, as much of the interdisciplinary science literature claims or implies, problems to be overcome so "integration" can take place. Second, field analysis demands accounting for the collective properties of an interdisciplinary field in terms of the heterogeneity (professional, intellectual, practical, etc.) of the membership rather than assuming "community" with the presumption of homogeneity, commonality, and a telos of integration. Third, field analysis insists that we characterize this heterogeneity in terms of the mix of members' "strategies," the way these connect resource interdependencies among fields, and the processes of conversion and exchange that make resources useful in multiple spaces.

To exemplify this approach to interdisciplinary science, I draw on data from a larger project on controversy and knowledge production in behavior genetics—the field I introduce below. This project relies on a combination of fieldwork at professional meetings, scientific publications (research papers, reviews, letters, textbooks), archival sources, secondary sources, and, most importantly, interviews with thirty-six behavior geneticists, critics, and commentators on the field. Interviewees are identified in terms of the four main "positions" that comprise the field (described in detail below): ABG=animal behavior geneticist, MG=molecular geneticist, PG=psychiatric geneticist, PBG=psychological behavior geneticist.

The next section briefly lays out some key concepts in Bourdieu's field theory and adapts them for interdisciplinary science. Then I turn to the case study of behavior genetics, first explaining how the field's interdisciplinary membership and their struggle for scientific capital are organized and then showing how different types of behavior geneticists have different possibilities for using their scientific capital in other fields. Then I consider some of the implications for the field's distribution of power and knowledge production. I conclude by considering how Bourdieu's field theory might further the analysis of interdisciplinary science in science studies.

Field Theory and Interdisciplinary Science

The scientific field, according to Bourdieu is,

> A field of forces whose structure is defined by the continuous distribution of the specific capital possessed, at the given moment, by various agents or institutions operative in the field. It is also a field of struggles or a space of competition where agents or institutions who work at valorizing their own capital—by means or strategies of accumulation imposed by the competition and appropriate for determining the preservation or transformation of the structure—confront one another. (1991, pp. 6–7)

The scientific field is thus comprised of a set of forces that shape struggles among scientists and struggles that reproduce or transform those forces. "Scientific capital" is the meeting point of the field of forces and struggles—it is the object of competition whose accumulation and distribution are crucial forces. Bourdieu defines scientific capital "inseparably as technical capacity and social power…to speak and act legitimately…in scientific matters" (1975, p. 19). Put differently, scientific capital is the combination of scientific expertise, social skills, and symbolic and material resources necessary for a scientist to achieve the recognition of other scientists. Scientific capital is intentionally a hybrid concept mixing material and symbolic goods, know-how and reputation because all of these are linked in competitions among scientists.

Bourdieu generally writes about science as if it is comprised of a set of disciplinary fields, each of which has its own form of scientific capital. He scarcely considers the situation where scientists bearing different forms of scientific capital interact,[1] let alone the situation of interdisciplines. Building on Bourdieu's framework, I suggest that interdisciplinary sciences be understood as spaces where no single form of scientific capital enjoys a monopoly, and therefore multiple forms of scientific capital constitute the structures and stakes of scientists' competition. Within interdisciplines, scientists simultaneously struggle over the distribution and definition of multiple types of field-specific scientific capital *and* the "exchange rates" among them (as well as their exchange with non-scientific forms such as economic or media capital). Thus, I suggest that interdisciplinary fields be analyzed as spaces where scientists are simultaneously pursuing capital from multiple scientific fields where the rules of the "game" are different and partly incompatible.[2]

All disciplinary fields have some members whose participation is oriented toward earning scientific capital in other fields. Thus the difference is one of degree between disciplinary fields where one scientific game has a near monopoly and interdisciplinary fields where multiple games are in more equal balance.[3] Further,

[1] Though see Bourdieu (2004, pp. 62–71).

[2] The "game" metaphor in Bourdieusian analysis emphasizes that actors are embedded in locally structured, relational competitions for advantage rather than the ludic dimensions of sociability as in Mead or Goffman.

[3] The value of the Bourdieusian approach is at least threefold over others, like actor-network theory (Latour 1987), the mangle approach (Pickering 1993), or even social worlds theory (Fujimura and Clarke 1992), that reject the notion of field autonomy and emphasize contingency and hybridity in all scientific practices. First, for Bourdieu scientific autonomy is a virtue, but also a variable to be assessed empirically. The fact that scientific truths are not *simply* determined by economic or political criteria means that the scientific field has a degree of relative autonomy from the economic and political fields. Similarly, as I show below, the fact that the definition of "good science" in behavior genetics has *partial* independence from standards in psychology or genetics means that it has a degree of autonomy. Relative autonomy is thus something that must be mapped and analyzed empirically, not just rejected as *merely* the ideology of scientists. Second, the network, mangle, and interactionist approaches that locate action in associations, the tuning of practice, or the problem of interactive coordination have limited resources for tracing action at a distance. The field approach is deeply concerned with how perturbations in one part of the field impact disconnected actions elsewhere (see Martin 2003). Thus, as we will see, many behavior geneticists' chances for deploying their scientific capital are affected by the practices and controversies with others with whom they have no direct associations, practices, or interactions. A third, related point is that field analysis draws attention to the crucial ways that practices are affected simultaneously by different forms of recognition. Actor network and mangle theories explicitly discount this while interactionist theory typically considers it only in immediate contexts while neglecting recognition further afield.

another variable among interdisciplinary fields is the degree to which pre-existing forms of capital are simply mixing as opposed to the emergence of a form of capital and struggle for recognition distinctive to the interdiscipline. In the latter case especially, a scientist's "dual citizenship" can pose serious practical dilemmas about how to balance and combine her investments in the interdisciplinary field with those in the disciplinary field in which she is trained and employed. These dilemmas are exacerbated when exchange rates between forms of capital are such that they can discredit each other.

Behavior Genetics: An Inside Out Field

Behavior genetics is the field, often in the news, that produces claims about the genetic influences on a huge range of behaviors including intelligence, personality, aggression, criminality, mental illnesses, alcohol and drug use, even social attitudes and cultural practices (Hamer and Copeland 1998, Plomin et al. 2001). These are largely human behaviors, but some behavior geneticists study animals, often looking for behaviors analogous to those in humans. The field has been controversial for much of its five decade history most notably because certain members have sought to use genetic arguments to explain racial differences in IQ or criminality (e.g., Jensen 1969, Herrnstein and Murray 1994, Rushton 1994, Whitney 1995). However, these and other reasons for its controversial reputation are not the focus of this paper (see Panofsky forthcoming).

Behavior genetics is an interdisciplinary field that draws scientists from many disciplinary backgrounds—psychology, psychiatry, genetics, statistics, neuroscience, zoology, etc. Behavior genetics is an interdiscipline without a disciplinary structure in contrast to, for example, neuroscience which draws scientists from many backgrounds but also has departments in hundreds of universities and thus produces and employs students under its own "label." It is useful to think of behavior genetics as having an archipelagic structure, a cluster of separate islands or positions notionally tied up in a whole. Members think of themselves as "behavior geneticists," not just psychologists who apply genetic techniques or molecular geneticists who sometimes study behavioral traits, though the broader identity does not erase the particular ones. As I will discuss below, however, what it means to assume the label of "behavior genetics" varies for individuals from different scientific backgrounds and thus it is one of contestation as well as commonality.

In the behavior genetics "archipelago" there are many scientific backgrounds but four are crucial to the story I will tell here—psychological behavior genetics, psychiatric genetics, molecular genetics, and animal behavior genetics. The imagined commonalities uniting these positions are bolstered by a high degree of integration at a practical, scientific level. This is a crucial point because despite the conflicts and ambivalence I will describe below, behavior geneticists from different positions collaborate frequently, cite each other's work, and use overlapping methodologies to study similar topics. However, different positions have different training, aims, and emphases in their scientific activities. Thus as in geographical archipelagos, the inter-"island" commerce and exchange are accompanied by island-specific identities, lifestyles, traditions, and affairs and, different relationships to “mainlands” outside the archipelago.

Behavior genetics is an “inside out field” (Panofsky n.d.). According to Bourdieu (1975, esp. p.23), what differentiates scientific fields, ideal typically, from all other kinds of social fields is the fact that scientists cannot acquire recognition as scientists except by engaging other scientists who are their direct competitors. The implication of these dynamics is to produce increasingly inwardly directed, reflexive, and competitive scientific fields. An inside-out field, by contrast, is one where scientists’ struggle for mutual recognition within the field is relatively less important than their struggle for recognition in other scientific fields. Every field has some members who “moonlight” in other fields, but in an inside-out field this becomes a general principle of action. Thus, dynamics of reflexivity and mutual competition become deemphasized and a range of practices become more outwardly directed.

A particular struggle over the definition of behavior genetics’ scientific capital characterizes the field as inside out. Among my interviewees two basic orientations toward mutual competition and recognition prevailed. The “heteronomous orientation,” the disposition of the majority of behavior geneticists, especially those that research humans, views participation in behavior genetics importantly as a means to compete in other scientific fields. The “autonomous orientation,” held by an embattled minority mostly of animal behavior geneticists, sees participation in behavior genetics as an end in itself. They wish there was more collective investment in mutual engagement and building the field’s institutions.

The central dynamics of this struggle can be seen in the following quotes which illustrate two basic orientations toward mutual competition and recognition in behavior genetics. The heteronomous orientation can be seen in this quote from a leading psychological behavior geneticist:

> It’s always been my goal to be a developmental psychologist who does genetics. It’s probably my message in terms of giving away the field. I don’t want it to be a specialty—well it can be a specialty field for people who have the methodological skills, but I want to give it away in the sense that I try to tell people, “You don’t have to be a geneticist to do this stuff.” … I think that’s an important issue—[seeing behavior genetics as a] tool not a school—and giving the field away because it makes the biggest impact. (#28 PBG)

Despite being a dominant actor who identifies with behavior genetics and receives ample recognition from his peers, this speaker voices ambivalence about commitment to the field. More importantly, the point of participating in the field, as he sees it, is not primarily to gain the recognition of others in the specialty but to gain tools and skills that can be “given away” as a way to gain recognition in other fields. These views were echoed in various ways in the majority of my interviews, and they suggest that heteronomously oriented researchers take the recognition of behavior geneticists basically for granted while the real competitive excitement is achieving recognition in other fields.

Those with the autonomous orientation were more likely to justify their research in terms of curiosity or inherent interest in how genes and behavior are connected rather than targeting the issues and actors of other fields. They wish behavior geneticists would collectively tackle intellectual controversies and take each other's recognition more seriously. A sampling of these views can be seen in this quote from a mouse brain and behavior researcher.

> I don't want IBANGS [International Behavioural and Neural Genetics Society] to become the mouse meetings. There are psychiatric genetics meetings, [the] BGA [Behavior Genetics Association] and it's 90% human quantitative genetics. I would love to have them come to our meetings too. I would love to have all those meetings together. At one point I tried that. I proposed a joint meeting of the ISPG, the International [Society of] Psychiatric Genetics, BGA, and IBANGS. And everybody said "it's a nice idea," but then nothing happens, nobody gets back to you. Because they're not really interested. (#7 ABG)

The speaker describes the failure of efforts to bring behavior geneticists' meetings together to foster greater communication and interaction. The quote illustrates two things characteristic of the minority expressing autonomous attitudes. First, they are concerned to cultivate greater interaction, exchange, and also deliberation about crucial scientific issues among their colleagues. Second, they feel frustrated in these efforts—a frustration that my interviews with heteronomously oriented individuals mirrored in their ambivalence about deepening their commitments to behavior genetics' institutions.

These are a small sample of the attitudes expressed by my interviewees that suggest the competition for "field specific capital"—in particular, mutual recognition of behavior geneticists—has become secondary to several separate competitions for scientific capital in other fields. Most behavior geneticists compete for resources, recognition, and rewards in behavior genetics in order to better compete for scientific capital in their home disciplines of psychology, psychiatry, genetics, and neuroscience. The field is "inside out" in my terms. But behavior genetics is suspended in a set of unequal relationships to these fields. In what follows I consider how these relationships affect the competitions among behavior geneticists who are dependent on their abilities to convert or exchange the scientific capital from multiple fields.

Exchanges of Scientific Capital

Behavior genetics' archipelagic organization means that most participants actively maintain "dual citizenship" as behavior geneticists and members of another disciplinary field. Thus scientists hope to take the scientific capital they "earn" as behavior geneticists and "spend" or "exchange" it in their home disciplines (the mainlands with which their islands in the archipelago are associated) for the local forms of scientific capital. However, these fields each have different relationships with behavior genetics, and thus the meanings of associating with it—and the terms upon which its capital will be exchangeable—vary from field to field. In this section I track these meanings and their effects on the chances for exchanging scientific capital for each of the four major groups of scientists working in behavior genetics—psychiatric geneticists, psychological behavior geneticists, molecular geneticists, and animal behavior geneticists. Throughout, I focus on scientific capital as symbolic recognition, consider how behavior geneticists describe it being proffered or withheld under different conditions, and relate this to different capacities that symbolic resources offer.[4]

Psychiatric Genetics

Members whose home field is psychiatry have found the value of their scientific capital rise and fall as conditions there have changed. Prior to psychiatry's "biological revolution" genetics was viewed as an oddity. Genetics' role in that revolution secured it a place in psychiatry, but more recently researchers have struggled to distinguish genetics in psychiatry. The following quote from a leading British psychiatric geneticist describing his decision to enter the field in the 1970s illustrates psychiatry's ambivalence toward genetics:

> There were people in important positions in various spots in British science and British psychiatry who saw it as unimportant. And, in fact, I mean there were people within the Medical Research Council who said, "You've got no future in psychiatric genetics. You know they've done twin studies; they've shown there is a genetic contribution to schizophrenia; that's it. You know, where else can they take it?" … I mean to say that if you wanted to do that kind of thing it was met with a very mixed response. On the one hand, there was a then professor of psychiatry…who was quite encouraging. He seemed to have not much grasp of what I actually wanted to do; he thought it was generally a good idea. And, then there were other people, there was a well known social psychiatrist….who said to me, "How [archaic], an extraordinary thing to want to do…," in a very discouraging way not in a "how fascinating" way. (#24 PG)

This speaker encountered both encouragement and resistance, yet even the encouragement seemed more tolerant than optimistic.

He and others I interviewed explained that their research began to be taken more seriously as advocates of biological approaches started to dominate the field psychiatry in the 1970s. Behavioral genetic discoveries played a role in this transition as a stream of quantitative genetic studies using twins, adoptees, and families demonstrated that a sizeable portion of the population variance in most mental disorders could be explained by genetic variance (Rutter and Plomin 1997). Interviewees described mutual reinforcement of the scientific capital they brought in with the forms emerging in psychiatry. Thus, for example, their genetic studies were aided immensely by the standardized diagnoses promulgated by the Diagnostic and Statistical Manual. In turn, genetic studies provided evidence that mental illnesses

[4] I do not attempt to quantify symbolic recognition (as a citation analysis might) or closely investigate how it relates to material resources.

were organic disorders which helped justify their new definitions as more than conventional.

But by the mid-1980s, quantitative genetic studies became less valuable. They had helped biologically-oriented researchers became hegemonic in the field, but their distinctive contribution dimmed in a field that was seeking to make biology pay off in terms of treatment and where massive attention was being focused on drug development (Guze 1989, Shorter 1997). Psychiatric geneticists found themselves somewhat disregarded. The psychiatric geneticist, Kenneth Kendler, described his and others' eagerness to revalorize their scientific capital in the late-1970s and early-1980s:

> [There was] rapid, and nearly relentless, success of human genetics in mapping the classic Mendelian human genetic disorders.
> Few in the psychiatric genetics community could avoid feeling envious as these major disorders were mapped one by one. There was a great desire to get into the line with the hope that schizophrenia, manic-depressive illness, panic disorder, or alcoholism would be next. (1994, p. 100)

Psychiatric geneticists invested heavily in molecular genetic studies of mental disorders to demonstrate linkages with chromosome regions or associations with particular genes or markers. Researchers advocating this approach have been embarrassed by a series of failed replications of boldly trumpeted discoveries of genes associated with bipolar disorder (Baron 1997, Holden 1994, Risch and Botstein 1996). Psychiatric genetic research has continued on a large scale, but its frustrations have held to the same pattern: rather than identifying a small number of genes that account for a large portion of a mental disease's manifestation, research has identified a large number of genes that, through complex interactions and dependencies, generally explain only a small portion of disease variance (Turkheimer 2006).

Psychiatric geneticists have been chastened by these developments and their inability to deliver after setting high expectations that genetics would (re-)revolutionize psychiatry (Rutter 2002, Holden 2009). As Arribas-Ayllon, Bartlett, and Featherstone (2010) have shown, psychiatric geneticists have tuned to a discourse of "complexity"—especially in articles intended to communicate to other psychiatrists rather than each other—to moderate expectations and to signal their (new) responsibility as sober scientists. I would suggest that they find themselves in this position partly because of intense competition in field of psychiatry encouraged them to distinguish themselves by setting outsized expectations. Having mobilized scientific capital "on credit," as it were, and failing to reap profits in proportion to their promises, now they engaged in image maintenance to regain the confidence and recognition of psychiatric researchers.[5]

Thus, psychiatric geneticists have seen their scientific capital rise with the dominance of biological psychiatry, falter under increased competition, spike and then crash with a wave of speculative symbolic investment (backed by substantial

[5] This narrative parallels what psychiatric geneticists have sometimes said about their field, albeit in more optimistic terms (Propping 2005, Rutter 2002).

material resources), and now facing an era of "complexity" with overall lower but hopefully more stable levels of scientific profit. Overall, then, psychiatric genetics has been a somewhat ambivalent way to gain scientific capital in the crowded and clinically-oriented psychiatric field.

Psychological Behavior Genetics

Compared to the other fields it intersects, researchers in psychology have been able to convert their behavior genetics capital for the most value. This, somewhat ironically, is largely because it has remained controversial for diverse psychological audiences, and thus it has been a steady resource for gaining recognition and resources in scientific competition.

The nature vs. nurture controversy has long raged in psychology. Behavior geneticists have probably been the most vocal and successful force driving the rise of the nature position through the 1970s and 1980s. Especially in the 1970s, behavior geneticists often viewed themselves as radicals who would revolutionize psychology by reducing it to biological science. One interviewee explained the attitude when he was in graduate school during this period: "scientifically there was this kind of macho reductionistic attitude about everything. That old fashioned soft psychology and the new hard sciences of brains and genes were just taking over. And that in twenty years psychology wouldn't exist anymore" (#35 PBG). Unlike in psychiatry, bids to monopolize the definition of psychology's scientific capital through biologization failed. However, this meant that psychological behavior geneticists did not have the same problems as psychiatric geneticists distinguishing their approach.

One of the reasons they were able to do this was that they sought to spread behavior genetics widely into the field. Psychology is a very broad field ranging all the way from laboratory research on brain cell chemistry to clinical psychologists who counsel people but do no research. The gambit of many behavior geneticists in the 1970s and 1980s was that genetic approaches need not be cloistered in the laboratory-based subfields of psychology, but could be brought closer to the clinically-oriented mainstream. Thus they attacked almost every topic that they could: intelligence, personality, mental illness, child development, political attitudes, divorce, religiosity, television watching, etc. (Hamer and Copeland 1998, Plomin et al. 2001).

In the early years, behavior geneticists gained recognition through combat with the psychological mainstream. One interviewee described the attitude as, "They're not going to listen unless you hit them with a two-by-four…be in their face" (#28 PBG). Echoing others, he explained that more recently behavior geneticists have realized that their "greatest impact" comes through "giving the field away" which is to partner with "real experts… asking driven questions interesting to people in the field" (#28 PBG). Whereas behavior geneticists long had the reputation for arguing that the environment (e.g., parenting or schools) does not much matter in producing differences among children (Rowe 1994, Harris 1999, Scarr 1992), today they often claim that their methods can help environmental psychologists. As several explained to me, starting with genes can put psychology on causal bedrock, and controlling genetic heterogeneity is the best way to measure real environmental effects (see also Plomin et al. 2003). They have used genetics to appeal to psychologists' desire for their work to be "harder" and "more scientific." This is mostly based on survey data collected from registries of twins and adoptees they control—a research platform that they can offer access to which is compatible with the tools of most non-laboratory psychological researchers. And, indeed, behavior geneticists have successfully partnered with many psychologists, some of whom formerly criticized their work sharply.[6]

At the same time, behavior genetics has remained controversial among many psychologists and continues to be criticized (e.g. Moore 2001, Richardson and Norgate 2006). At one level these conflicts put behavior genetics' scientific capital at risk, but they also produce opportunities to generate more—each critique can stimulate new publications and grant applications. Molecular genetics methods have also been appealing to psychological behavior geneticists, offering the possibility, as many interviewees told me, to address the common criticism that their quantitative genetic methods have said nothing about which genes might affect behavior or the mechanisms involved. The results of molecular research have been no more successful for psychologists than for psychiatrists (Turkheimer 2006), but they have perhaps been less disappointing and discrediting. I believe this is because psychologists, having more and more diverse opportunities for continuing to build recognition did not play the same high stakes gambit that the psychiatrists did by attracting attention by promising revolutionary results.

This brief account does not do justice to the long and contentious history of behavior genetics in psychology. But the upshot is that behavior genetics has provided psychologists with a variety of means for accumulating scientific capital. Behavior geneticists have been able to exchange methods and recognition with some psychologists and do combat with others. All of this has been driven by their distinctiveness in psychology. This contrasts with the psychiatric geneticists who more rapidly and completely gained acceptance but then had difficulty maintaining recognition for making distinctive contributions in a field that was highly biologized and intervention-oriented.

Molecular Genetics

Compared to the success behavior geneticists have had in pursuing scientific capital in psychology and psychiatry, they have remained relatively more marginal to biologically oriented fields like human genetics and genetically-oriented neuroscience. Geneticists have often used the idea of behavior genetics to drum up public support (and spending) on their expensive research. However, they have been much less willing to participate scientifically in the field, and have tended to engage the less contentious mental illness phenotypes when they do. They perceive behavior genetics as dangerous for its inclination to controversy and methodological

[6] For example, Kagan (1969) and Wachs (1983) shifted their critical positions to become allies of behavior geneticists (e.g., Kagan 2003 and Wachs and Plomin 1991).

shortcomings. However, for some geneticists these dangers are attractive as behavior genetics enables them to gain attention and notoriety that would be otherwise unavailable.

In their efforts to drum up public enthusiasm and financial support, geneticists have repeatedly invoked behavior genetics (Nelkin and Lindee 2000). In his immodestly titled "A Vision of the Grail," the eminent geneticist Walter Gilbert, justified the Human Genome Project on the grounds that it would answer the question, "What makes us human?" (1992, p. 86). To most people what makes us human is not proteins, but behavior. Daniel Koshland (1987, 1989, 1990), the former editor of *Science*, wrote a series of editorials championing the Human Genome Project, arguing that genetic findings would allow society to address problems caused by mental illness and deviant behavior.

However, geneticists' participation in behavior genetics has not matched their enthusiastic invocations for public relations. An oft echoed lament by behavior geneticists is the "parent discipline of genetics hasn't paid any attention to it" (McClearn 1993, p.48). Human geneticists have largely stuck to non-behavioral phenotypes. Hirschhorn et al. (2002) did a comprehensive survey of genetic association studies and found that only about 12% (20 of 166) of associations reported in multiple publications were on behavioral traits, and all of those were psychiatric disorders not the "normal range" behaviors of interest to psychologists.

This wariness about behavioral genetic research is partly rooted in the field's legacy of politicized controversy and geneticists' longstanding efforts to valorize their scientific capital by dissociating it from socially stigmatized eugenics (Kevles 1985). Behavior genetics has raised the specter of eugenics repeatedly by hosting scholars who have argued that genes explain part of the fact that compared to whites, blacks have lower IQ and achievement scores and higher rates of criminality and suggesting that public policies might be promoting dysgenic trends (Herrnstein and Murray 1994, Jensen 1969, Lynn and Vanhanen 2002, Rushton 1994, Whitney 1995). Some geneticists have chosen to criticize this research actively (e.g., Devlin et al. 1997). Geneticist Jonathan Beckwith (2001) has argued that other geneticists have preferred not to engage these debates in part because they're wary of becoming associated with stigmatized topics.

Geneticists have also been wary of behavior genetics because they perceive it as methodologically suspect. One interviewee expressed a sentiment expressed commonly in interviews and the literature:

> I think there are some that see behavior as kind of poor cousin in human genetics. The phenotype is definitely messy relative to things like hypertension or diabetes. And the physiological models are just not worked out, so it's harder to do the genetics, so I think a lot of human geneticists are reluctant to get involved or align themselves with behavior genetics on that account… Others, though, find it fascinating. If you think about it, the burden to society, behavioral disorders have an extraordinary burden to society, autism, schizophrenia, depression, alcoholism. They all have genetic components associated with them, and arguably it's important to understand what those

> are, but it's not as easy I don't think with behavior as it might be with other traits. (#23 PBG)

Thus, difficulties in defining behavioral traits are often seen as off-putting to geneticists even though the "burden to society" makes the effort tantalizing. It is not surprising that the psychological behavior geneticist just quoted would identify the problem with the behavior, but a molecular geneticist I interviewed targeted the *work* of behavior geneticists as a reason to resist too close an association:

> A big problem here is trying to map out a separate domain of behavior genetics, because there is no such separate domain. I mean there're a few people who are geneticists and behaviorists and who don't do other things, but they don't really make breakthroughs, you know. I mean, if you think that it's a breakthrough to characterize the heritability of a behavior, then fine. But if the issue is what is the origin of that behavior, then elucidating that is the achievement of different methodologies, including studies on functional gene variation and measurement of phenotypes that are in the pathway of that gene and its relationship to behavior. (#13 MG)

The speaker expresses a view of some molecular geneticists that behavior genetics' scientific capital is not well regarded. For those few geneticists who don't avoid it altogether, they often pursue a strategy like that described here: limiting the dependence on behavior genetics and supplementing it with methodologies of genetics and physiology.

However, the risks of associating with behavior genetics have an appeal of their own, and some geneticists have pursued research there precisely because it enables them to use scientific capital to pursue a symbolic capital of notoriety. For example, in explaining his motive to do the "gay gene" study, Dean Hamer explained that he was bored with the sober cancer genetics research he had done for many years, and he wanted to be able to study what "makes people tick" (Hamer and Copeland 1994, p. 19). Hamer got immense attention for this research (Hamer et al. 1993), even if some of it was negative. He has parlayed it into a career as a public intellectual, making media appearances and writing popular books (Hamer and Copeland 1994, 1998), turning lately to the genetic origins of religiosity, or the "God gene" (Hamer 2004). Though Hamer's story is not typical, it exemplifies the kinds of attention and notoriety that others have pursued in much milder doses. It also demonstrates the kinds of associations that behavior genetics can carry for molecular geneticists and thus why it is a risky but potentially rewarding strategy to pursue.

Animal Behavior Genetics

Unlike the positions considered above that are each identified with a particular disciplinary field, animal behavior geneticists come from diverse disciplinary backgrounds—biological sciences, neuroscience, genetics, psychology, zoology, ethology, and biomedical and agricultural sciences. More crucially, animal behavior geneticists have the greatest difficulty converting the scientific capital they earn into recognition from other fields.

At least some animal researchers perceive costs to participating in behavior genetics. Describing his decision earlier in his career to submit a series of articles to *Behavior Genetics*, one animal researcher said, “We thought it was a good journal of course… [But] we were not really looking to go to the top journal. We were looking for a journal where people would find the stuff that we were doing, and that it would get read” (#7 ABG). Another researcher explained that he essentially disassociated from the field, and reframed his research in other terms in order to be taken seriously:

> As soon as [research on intelligence and cognition] got polarized, the whole issue became a non-issue to science. And other people working in other fields went on about their business and advanced our genetic understanding of how the brain works and there you go. I work on genes that affect memory. I don’t call it genes of intelligence or IQ deliberately. I’ve sort of depolarized the issue by talking more biologically about the memory process. (#34 ABG)

Animal researchers who do remain in the field often frame their research in ways that connect it to human studies.[7]

Animal behavior geneticists frequently express frustration that researchers whose work they consider to fit a substantive definition of “behavior genetics” refuse to recognize themselves in that label. As one fruit fly researcher said to me, “people are doing behavior genetics research that don’t even know it….there’s a lot of behavior genetics going on, and there has been a lot that ain’t called behavior genetics” (#11 ABG). A mouse researcher explained this non-identification as an active avoidance:

> All these people doing knockout studies, and transgenic animals and things like that. They’re looking at genes and the effect of genes on behavior. Well, how else should I define behavior genetics? But they don’t see themselves as behavior geneticists. They hardly if ever send their work to a journal that is in behavior genetics… They never came to the BGA [meeting], although I tried to get them there. (#7 ABG)

What is crucial here for the story of scientific capital is that these non-identifying behavior geneticists represent a problem for those who do identify with the field. Their indifference toward behavior genetics or their active refusal to participate—declining invitations and such—represent a withholding of the recognition necessary to form allegiances and exchange and convert scientific capital easily from field to field. In these and other quotes from animal behavior geneticists, we can see that they face a dilemma through their association with the field. Claiming membership and framing research in its terms offers a intellectual community with shared interests, but it seems to lower chances for other forms of recognition.

[7] For example, Kaplan (2000, pp. 60–3) suggests that some animal behavior geneticists have interpreted their research in ways that support determinist ideas in human research though alternative interpretations are available.

The reasons for animal behavior geneticists' recognition problems seem to concern controversy and methodology. The speaker above suggested that "polarization" during the IQ debate led potentially interested scientists to shun behavior genetics. This was echoed by another who complained that the field's ongoing association with researchers claiming a genetic basis to racial differences in intelligence are "spreading disrepute over the subject" (#11 ABG). She was chagrined when her research on mating in fruit flies was cited as supporting the claim that racial mixing leads to reduced fertility. Recognition problems are linked to differences between "experimental systems" (Rheinberger 1997). Most of the research characteristic of the three positions discussed above is correlational in nature—it uses genetic information to partition population variance into genetic and environmental components or it seeks to identify genes or genetic markers that have statistically significant relationships with a particular trait. In contrast, the power of using animal models is the ability to experimentally manipulate their genetic makeup (for example, through breeding, gene knockout or insertions, or genetic chimeras), to track their development and behavior in controlled conditions, and ultimately to study their behavior via physiology and neuroanatomy (Balaban 2001a). As an animal behavior geneticist put it, those who could but don't identify with the field "are neuroscientists…. They are interested in brain mechanisms… They want to know, How is this working? What's going on?" (#7 ABG). Though mechanism-oriented research is not incompatible with animal behavior genetics, the field's association with correlational research has made these connections difficult for scientists to appreciate (Balaban 2001b). Animal researchers' difficulties parallel those of the molecular geneticists but with much less potential to leverage the potential to draw attention or be provocative.

Animal behavior geneticists tend to be the most ardent backers of the "autonomous orientation" described earlier. Their desire that behavior geneticists be concerned with the field for its own sake, rather than for earning capital in other fields, is partly due to their relative lack of success at in these efforts for exchange. Further, their desire to strengthen ties and institutions within the field is linked to their efforts to broaden the definition of behavior genetics' scientific capital. This is behind the largely unsuccessful efforts quoted earlier to forge connections with neuroscientists and those in other fields. One interviewee expressed his desire to foster greater coordination among researchers working to relate genes to behavior:

> It would be nice if all of the people in the different areas who want to study these things can kind of get their act together about some common framework for posing meaningful questions…. But that would be my minimal hope, not that these kinds of distinctions would go away but that we could be a little better of finding ways of putting our energy into meaningful venues for getting answers to them. (#3 ABG)

For them, linking to other researchers, increasing participation and conversation between the field's positions, and creating "common frameworks" and "meaningful venues" is also about extending the boundaries of the field to encompass those who "ain't called" behavior geneticists and loosening the strong association between "behavior genetics" and correlational research on humans. To bring their potential interlocutors into the field would, of course, negate animal behavior geneticists' need to play the game of scientific capital exchange which they find so challenging.

One might wonder why animal behavior geneticists would persist in this identity if it serves them poorly in the competition for scientific capital elsewhere. Some, indeed, do abandon the field, but for those who remain Bourdieu's notions of illusio and habitus are key reasons. A concomitant of their already substantial investments is that they accept the illusio that doing "behavior genetics," competing to produce findings, receive recognition, and to re-define the field is "worth the candle" (Bourdieu 1998, p. 77) even if it is irrational from the point of view of *maximizing* scientific capital.[8] Further, habitus is durable and it would be no small matter to pick up a new scientific identity, reimagine one's work along new lines, and compete for the recognition of new audiences.

Consequences of Capital Exchange

These differences in the capital exchange chances for behavior geneticists in other fields help interpret patterns of social organization and knowledge production within the field. Within behavior genetics psychological researchers top the hierarchy of positions and psychiatric, genetic, and animal researchers are subordinate. One version of the field's internal hierarchy can be seen looking at the Behavior Genetics Association. About half the elected presidents have been psychological behavior geneticists (18 of 39), a quarter animal behavior geneticists (11), with the rest being two psychiatric geneticists, three geneticists, and five "hybrids" who fit both the psychological and another category. Thus the psychological position dominates, but animal researchers have carved out a prominent secondary spot. Rather than compete for prominence in the BGA, psychiatrists formed a separate society—the International Society of Psychiatric Genetics in 1992—and geneticists have mostly stayed with the American Society of Human Genetics. However, in collective representations of the field, especially over the last two decades, human research from psychological, psychiatric, and molecular genetic perspectives has dominated, and the representation of animal research has usually been of a token nature. This can be seen clearly in behavior geneticists' recent major edited collections and textbooks.[9] It was also reflected frequently in my interviews. When asked to "map" the key directions or participants in behavior genetics, human researchers tended to overlook animal research or mention it as an afterthought. Animal researchers often complained bitterly about being implicitly written out of the field. Thus, within behavior genetics the typical hierarchy of the sciences where "harder" natural sciences dominate "softer" social or behavioral sciences has been mostly reversed.

This hierarchy maps onto the different positions' capacity to convert scientific capital. Psychological behavior geneticists have been able to parlay the field's

[8] Also the differences in chances for receiving scientific capital are relative and the stakes are not life and death—animal behavior geneticists can have respectable careers despite these problems.

[9] See, for example, Carey (2003), Hamer and Copeland (1998), Hay (1985), Plomin et al. (2001), Plomin et al. (2003), and Rutter (2006). In Fuller and Thompson (1960) the animal/human relationship is reversed.

capital in their disciplinary home most successfully, followed by psychiatric geneticists, then molecular geneticists, while animal behavior geneticists have gained very little in other fields. This internal hierarchy essentially inverts what is the stereotypical hierarchy of science in which "harder," more biological perspectives dominate the relatively "softer," more behaviorally-oriented perspectives.

Ordinarily, we might expect those who benefit the most from membership in the field to be its most committed and passionate members. But in behavior genetics, animal researchers are often most committed to the field with increasing degrees of ambivalence being expressed by the psychologist, psychiatrist, and molecular geneticist positions. Animal researchers are most likely to exhibit attitudes consistent with the "autonomous orientation" described above while those from the human research positions are generally "heteronomously oriented." For those three groups, participation in behavior genetics serves their projects in other fields, so their commitments are split, and a strong adherence to behavior genetics can be a liability elsewhere. But animal behavior geneticists are relatively unable to use capital earned in behavior genetics to compete in other fields, so for those who haven't abandoned it, commitment to the field runs deep. Thus, they are in the ironic position of being committed to a field whose structure keeps them dominated—even though part of this commitment is to projects that would valorize their capital over the long run.

The exchange of behavior genetics' scientific capital with other fields also has consequences for the shape of the capital itself. One is that behavior genetics' capital is highly dependent on the intellectual resources of other fields especially for the definitions of behavioral phenotypes. Human behavioral geneticists use the survey instruments and diagnostic criteria of psychology and psychiatry to measure behavioral traits. Skeptics have long criticized this practice, arguing that traits like "intelligence," "extroversion," or "depression" are highly culturally dependent and causally very different from the molecular context of proteins through which genes affect development and physiology (Balaban 2001b, Moore 2001). But this was not always the case. The field's first textbook (Fuller and Thompson 1960) and review article (Fuller 1960) highlighted some research on sensation, perception, and reflexes—topics that would later largely be forgotten. Incidentally, one implication of animal researchers' desire for recognition in the field is the tendency, according to some critics, to imagine their research as more directly analogous to humans than it is—for example, describing tail twitching or defense postures during mouse social encounters as revealing something meaningful about aggression in humans (Balaban, Alper, and Kasamon 1996).

Behavior geneticists have not exchanged scientific capital equally within psychology, but rather have been associated with particular positions in that field. Thus there has been a huge amount of research on the genetics of intelligence measured in terms of IQ scores or its subcomponents (Bouchard 1996). But there has been almost none on other conceptions of intelligence such as Gardner's (1983) multiple intelligences or Sternberg's (1985) triarchic theory. Areas like social psychology and social interaction have been neglected almost completely. Behavior geneticists have been criticized also for focusing on "global index variables over analytic, proximal ones…[and] for not measuring the *processes* by which children [et al] come to be different from one another" (Scarr 1981, pp. 528–9). These gaps are partly explained by methodological problems—the difficulty of doing more than giving standardized surveys to samples of twins. But there is no reason to think such problems are insurmountable, and the science might look very different if behavior geneticists were distributed evenly into psychology rather than into positions that were *in conflict with* non-IQ versions of intelligence, developmentalists, and social process-oriented psychologists.

Vigorous exchange has also meant that behavior geneticists have kept their scientific capital in a portable and general form. Elsewhere, I have argued that knowledge in behavior genetics is usually pursued extensively rather than intensively (Panofsky n.d.). One general model for working in genetics is to estimate the heritability of a trait, then try to identify genetic markers, clone the responsible gene, and then work out the gene's biological function. Behavior geneticists have typically focused on the population level, applied their techniques to new populations and new traits, and found heritability estimates for a growing list of traits without deeply exploring the meanings or causes of the genetic correlations identified (Turkheimer 2000). For many researchers, then, the technical essence of behavior genetics is a set of statistical tools that can be applied to data sets that have related individuals in them (especially twins). The methods can be moved from trait to trait, data set to data set and, as I quoted the speaker above, "You don't have to be a geneticist to do this stuff."

Finally, one of the most enduring critiques of behavior genetics is the tendency of its practitioners to interpret heritability (the portion of trait variance in a population that is accounted for by genetic variance) as having causal weight (Lewontin 1974, Kaplan 2000). The accusation is that this reflects an ideology of genetic determinism that has long been a professional inclination of geneticists (Lewontin, Rose, and Kamin 1984, Hubbard and Wald 1993). But there is a case to be made that the "deterministic" tendencies of behavior geneticists have more to do with the intellectual dispositions of social and behavioral scientists. Part of this is their inclination to interpret sophisticated analyses of variance (the essence of correlation-based statistical methods) in causal or quasi-causal terms. Biologists, in contrast, usually consider physical mechanisms articulated in terms of physiology or development to be causal. The other part is the tendency of behavioral scientists to view many behavioral traits—especially, personality and intelligence—as deeply habitual, dispositional, and resistant to change (Armstrong 2009). Thus, the greater dependency of behavior genetics' scientific capital on psychology than genetics may help explain why behavior geneticists acknowledge that genetic determinism is false yet still talk about their work in ways that continue to attract the accusation of determinism (Kaplan 2000).

Conclusion

This paper developed a Bourdieusian framework for analyzing interdisciplinary science and applied it to the field of behavior genetics. Behavior genetics is an interdisciplinary field with psychologists, psychiatrists, molecular geneticists, and various animal researchers. They compete to earn and define scientific capital, but most seek to exchange scientific capital earned within behavior genetics for that of their home fields. Behavior genetics' scientific capital has different meanings and offers different possibilities in each of these fields, so scientists from each position have different chances for successful exchange. Crucially, though, behavior geneticists are tethered together and affect each other's chances since each position has a different effect on the field's scientific capital and thus its meaning in the other home fields. Different positions' chances for capital exchange help explain their orientations toward and power within the field—in particular, the dominance of heteronomous human researchers over the less powerful, autonomously oriented animal researchers. Finally, I argued that these relationships help interpret certain aspects of behavior genetics' scientific knowledge, that is, the "form" of its scientific capital.

I would like to conclude by discussing some implications for the project of bringing Bourdieu's theory further into science studies. This paper should put to rest the assumption that field theory only applies to the kind of well-bounded, highly integrated, autonomous, ideal typical fields that science studies scholars are skeptical even exist. I showed that scientific autonomy is not an ontological assumption of Bourdieu's method but a stake of conflict in the field to be evaluated empirically and that the concept of scientific capital can be used to track scientists' multiple attachments.[10] It might be noted that this model is compatible with models of interdisciplinary science emphasizing cross-boundary coordination, translation, associations, or the tuning of practices (Fujimura and Clark 1992, Latour 1987, Pickering 1993). However, the Bourdieusian field model would demand these be linked to recognition practices suspended in relationships beyond the immediate context of interaction.

The literature on knowledge production in interdisciplinary science has tended to take the core problem to be barriers to intellectual integration and exchange; usually emphasizing differences in cognitive style or epistemic culture. In contrast, the Bourdieusian field theory approach places the competition for recognition at the center. Cognitive integration, then, is a function of the competition for recognition, that is, the relative independence and coherence of the interdiscipline's scientific capital as a stake of struggle for scientists in the interdiscipline. Exogenous disciplinary cognitive differences certainly matter, but the struggles for scientific capital, and the power imbalances that result, can produce differences that may or may not map onto disciplines.[11]

Further, the literature often takes the status of interdisciplinary entities for granted, even while acknowledging relative incoherencies like porous boundaries (Frickel 2004). But field analysis, by requiring a reconstruction of the specificity of scientific capital and the conditions of competition, enables analysts to think

[10] In this case attachments within science, not to economic or political contexts.

[11] For example, in behavior genetics, psychologists and psychiatrists often perceive themselves as more cognitively different from each other than their scientific practices suggest, and animal behavior geneticists from many different disciplinary/cognitive backgrounds share similar views and experiences.

rigorously about what ways an interdiscipline is and is not “fielded.” Behavior genetics currently has a field-specific form of capital that has a differential though overall weak pull on competitors. Though its centripetal and centrifugal forces have been in balance for about 50 years, today the field seems more likely to fly apart (becoming a set of positions in other fields) than to coalesce into a robust collective competition for mutual recognition. Field theory enables a set of tools for a very specific reconstruction of different forms of interdisciplinarity and how they affect scientists’ practices. Pursuing field theory in this way will help researchers think more globally, critically, and comparatively about interdisciplinary fields.

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