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Spaces Between Fields

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Spaces between Fields
Gil Eyal

The problem

This set of reflections is meant to critically engage with Bourdieu’s concept of “field” by posing the question: “are there spaces between fields? And if there are, what is their significance?”

Let me explain what I mean by this: one of the crucial contributions of the concept of “field” is that it requires us to stop thinking in terms of entities, proper names, concrete individuals, things, and begin grasping all of these as bundles of relations. The prophet, the priest or the magician, for example, in Bourdieu’s (1987) masterful conversion of Weber’s Sociology of Religion, are neither concrete individuals nor ideal types, but certain intersections of the relations of dependence on and independence from the consumers of salvation, definite regions within the religious field. In my own work (Eyal 2003), I have followed Bourdieu in arguing that “left” and “right” in post-communist politics (indeed, in any politics) should not be grasped as fixed ideological platforms, but as constantly changing ideological “packages” assembled with respect to the crisscrossing pressures operating in the political field. This is what Bourdieu means by denoting his approach as “relational.” Yet, while Bourdieu is quite rigorous in applying this relational approach to political programs, works of art, styles of life, or scientific theories, which are all deconstructed into the bundles of relations that constitute them, he does not do the same thing with the distinction between fields themselves. Bourdieu constantly talks about things like “the economic field,” “the artistic field,” “the political field,” “the bureaucratic field,” “the academic field,” “the legal field,” “the literary field,” etc. It is as if the relational approach stopped short before these entities – which are nothing but a fancy way of saying “the economy”, “the political sphere”, “the state”, “academia”, “science”, “art”, i.e. proper names – and left them in their place as distinct “spheres” whose contents are clearly bounded and well distinguished from one another.

That this is a problem becomes clear when we consider, for example, the practices of gift-giving, charity and domestic money analyzed by Viviana Zelizer (1997), indeed even by Bourdieu himself (2000; see also Callon 1998). All of these practices, wherein money is involved, have a double meaning, or a “twofold truth” as Bourdieu puts it: they can be seen as “economic” (i.e. interested, egoistic, calculable) and/or they can be seen as “moral” or “social” (i.e. altruistic, disinterested, non-quantifiable). It all depends, as Callon says, on the framing, particularly the temporal frame within which they are viewed. It follows, therefore, that there is no easy, clear cut, self-evident criteria by which the various fields could be distinguished in terms of the content of the activity that takes place within them. Nothing is “economic” or “scientific” or “artistic” by itself. These are also shorthand for bundles of relations.

What would happen if we submit these entities as well to the relational method? If instead of treating them as given spheres, we would try to deconstruct them into bundles of relations? My argument is that we would have to begin to think in terms of spaces
between fields. The existence of these fields as separate spheres would have to be analyzed as a function of the struggles taking place in the spaces between them, and of the mechanisms set up in the spaces between them.

The empirical problem

This problem has emerged for me in the context of my own work on the history of expertise in Arab affairs in Israel, specifically in my analysis of the relations between academic Middle Eastern studies and military intelligence. While in an earlier paper (Eyal 2002) I conceptualized these relations as taking place within a “field of orientalist expertise,” as I reworked these materials into a book manuscript I found myself in need of additional concepts and drawing on Latour in describing a “network of intelligence expertise.” (Eyal 2006)

The problem I was seeking to understand is as follows: there is a branch of Israeli Middle Eastern studies, in fact the dominant mainstream of this discipline ever since the late 1960’s, typically practiced in academic research institutes, which is closely tied to military intelligence. Many of the scholars have served in Military intelligence in the past, and continue to provide services for it in the present, either as part of their reserve duty or as contract work. Moreover, critics have also claimed that in terms of the selection of problems, the style of writing, the methodology and mode of analysis, there isn’t really much difference between the two. (Sivan 1979a; Sivan 1979b; Baer 1979; Porath 1984; Toleidano 1989)

What happens, therefore, to the distinction between the academic field and the bureaucratic field (or however else one might choose to denote the location of military intelligence)? There are, of course, easy ways of dealing with this issue. One could deny, as many of these scholars do, that the contract work they do for military intelligence has anything to do with their “real” academic work. In short, they claim to belong squarely within the academic field. Or you could imply, as many of their critics do, that these academics have “betrayed” their calling and crossed over into officialdom. I find these answers, however, to be facile. All they seek to do is to save the distinction between academia and officialdom, to protect their appearance as distinct “spheres” whose contents are clearly bounded and well distinguished from one another. In short, they are forms of “boundary-work” between science and its environment, and as such they may interest me as part of the object of study, but not as reliable ways to conceptualize it. I begun to be interested in other ways of approaching this form of expertise, without reducing it to this or that side of the opposition between the “academy” and “the military.” What would happen if we leave it where it is, in the space between them? What would happen if, like the gift, we permit it to hold onto its “double meaning” or “twofold truth”? As will be seen below, Bourdieu’s concepts did not hinder me from doing so, but they were not particularly helpful either, and I needed to twist them in a particular way to get at what I wanted. For this reason, I am now interested in exploring this idea of spaces between fields.
**Objections**

But before we explore this argument, I need to deal with the various lines of defense that Bourdieu’s theory can erect against this line of questioning:

**Relations between fields**: the first line of defense might be to search in Bourdieu’s theory for a set of relations that could do the job of accounting for the existence of fields as separate spheres. One plausible candidate are the relations between fields themselves. Terms like “economic field” or “political field,” the argument would go, do not denote an essence (“politics”) or an entity (“the economy”), but a structure of relations itself determined by the total set of relations between fields, as well as the struggles in the field of power over the value of various forms of capital. This argument is clearly illustrated by Bourdieu’s visual device of depicting fields as nesting one within another, and all of them together within the field of power. I am not convinced by this answer. In Bourdieu’s theory, the relations between fields typically determine the degree of autonomy of a particular field (how far they are, within the field of power, from the pole of dominant economic capital). Autonomy is a concept that refers to the orientations of actors, and says very little about how to characterize the modality of their activities – are they “economic” or “moral”, “scientific” or “artistic”. It is a general, abstract characteristic that can pertain to very many different spheres, without regard to the specific character of activities taking place therein. So the first conclusion I would like to derive is that the necessity for a concept of spaces between fields has to do with specifying the modality of activities, rather than the orientations of actors.

**Excursus on the scientific field**: there is a brilliant economy to Bourdieu’s (1975, 23) famous formulation that the specificity of the scientific field consists in that "the producers tend to have no possible clients other than their own competitors (and the greater the autonomy of the field, the more this is so)." But the tightness of the formulation comes at a heavy price. What Bourdieu is saying, after all, is that the more autonomous a field is, the more scientific, without needing to say anything about the type of activity taking place therein. Clearly Bourdieu intends this formulation to enable distinguishing “real sciences” – typically abstract and highly detached disciplines like theoretical physics or mathematics – from disciplines that are at a lower degree of scientificity or altogether “false sciences”, because the producers are heteronomously oriented to recognition by political, economic and other actors. Whatever one may think of the wisdom of this endeavor, could this criterion really serve to identify the specificity of the scientific enterprise as distinct, let’s say, from art or the intellectual field? Here we are entering very dangerous terrain for Bourdieu. He could argue that while artists and intellectuals may pretend that they need no clients but other artists and intellectuals, they in fact are dependent on the recognition granted by larger groups of clients such as the reading public or art critics (see Bourdieu 1969). But the same could be said for psychologists or economists, no? Are they, therefore, lesser scientists, false scientists, artists or intellectuals? I say this is dangerous terrain for Bourdieu, because what becomes evident here is that the “specificity of science” is nothing but an idealization of the science, or even worse, an “ideal type”. The irony is, of course, that Bourdieu developed the concept of “field” precisely in order to overcome the limitations of Weberian “ideal type” methodology (Bourdieu 1987), but when it comes to differentiating the fields themselves from one another, he seems to recapitulate Weber’s essentialism.
Relations within the field: If the relations between fields cannot do the job, how about the relations within fields? Specifically, the struggles that take place in any field over the boundaries of the field itself, the criteria of membership in it, the costs of entry to it, in short the struggles over “boundary work”. While the relations between fields set up the general parameters within which such struggles take place, it is up to “boundary work” to produce the distinction between fields, for example the distinction between the academy and the state. I am not persuaded by this answer either. I can register my misgivings with the following rhetorical question: “where is boundary work itself located? Inside the field or outside it?” When scientists write op-ed pieces in order to expose charlatans, is this scientific or journalistic activity? Obviously, any answer to this question would be boundary work itself. Another question: does the boundary separate or connect? To return to the example of gift-giving and charity: what makes something a gift, and not an investment or payment? The fact that I report it as such on my tax form, i.e. it is excluded from the economic by being included in it (on the tax form), and vice-versa, it is included in the economic by being excluded from it, as when my accountant calculates that I am better off contributing to charity now to avoid taxes later. So my second conclusion is that the necessity for a concept of spaces between fields has to do with the idea of “boundary work.” One cannot analyze “boundary work”, I would argue, without being armed with a set of concepts that make sense of what goes on inside the volume of the boundary. I propose that we cease to think of the boundary in Euclidean terms, as a fine line with no width to it, and begin to grasp it as a real social entity with its own volume, so to speak. As such, the boundary does not simply separate what’s inside and outside the field, e.g. what is economic and what is not, but is also a zone of essential connections and transactions between them. On the one hand, the volume of the boundary is where struggles take place to apportion actors and practices this way and that; on the other hand, it is also where networks provide for a seamless connection between fields. This also means that we could think of actors within the volume of the boundary as at once excluded from the field, and yet included in it by virtue of this very exclusion, which apportions them a particular network role.

Misplaced concreteness: At this point, Bourdieu and his followers may retreat to their strongest and most fortified line of defense. All of this is a big misunderstanding, they would say. We are, in fact, in no disagreement. What you call a space between fields is nothing but another field. The point is that field is not a thing, but a concept, a heuristic hypothesis, in a sense a fiction. There is no need to reify it and talk about spheres as if they actually existed. It is simply the point that in analyzing the actions of this or that politician, or the words of this or that intellectual, or the work of this or that scientist, one has to consider them as taking place in the context of a total set of relations; one has to take account of all the actors, proximate or distant, whose actions impinge on the politician/intellectual/scientist and shape their likely conduct. Hence, there is also no need to define boundaries for the field – the lines of force reach to infinity; they are only weaker at a longer distance. And fields therefore could easily overlap with one another. So, if you are interested in marginal actors, who exist on the frontiers of fields where the lines of force become weaker, you can simply shift the focus and analyze the sphere of their activity as a different field – the field of generalized cultural production, the field of scientific popularization, the field of charity organizations, etc. This is a good answer, as
far as it goes, but far from full proof. First, it is a bit disingenuous, because while we are given freedom to analyze fields wherever we see fit, it is still the case that Bourdieu almost always limits himself to the canonical distinctions between economy, politics, culture, etc., and any additional fields analyzed are characterized negatively as “less” autonomous, “less” important. Second, we may agree that “field” is just a concept, and hence there is no need to reify it, but the crucial question then becomes, what kind of a concept is it? What is it designed to do? If it is just a concept, we may decide not to use it if we do not think it can shed light on the problem at hand, if we do not think it may provide us with fruitful hypotheses. This is related to a problem I commonly face trying to teach Bourdieu’s theory and methodology, and guiding dissertation research. It is invariably the case that students come to me worrying whether their case study, the activity they are analyzing, really qualifies as a field. Saying to them that it is a heuristic concept helps somewhat, but is empty talk without making clear what is the problem the concept is geared to address, and what are the alternatives, what other concepts may be used (heuristically) to address other problems.

Bourdieu and Latour compared on the matter of overcoming the false polarity between internalist and externalist explanations

At this point, it would be useful to clarify what kind of concept “field” is, and what it is designed to do. The main point is that it is not equipped to deal with the issues raised here, which are better addressed by the Latourian concept of “network”. The wager then is whether it is possible to integrate the two approaches in some meaningful way, and this is exactly what I propose to do with the idea of “spaces between fields”.

Bourdieu (1985) has already provided us with an answer as to what kind of concept “field” is, and what it is designed to do: it is meant to overcome, he said, the false polarity between internalist and externalist explanations of artistic, intellectual, scientific or political practices. I think this is true, but only with respect to a limited rendering of this problem: the concept of field is meant to overcome the false polarity between interpreting actors’ actions as motivated by material self-interest (externalist explanation), as against construing their actions to be disinterested and guided only by a search for the “truth,” “beauty,” “authenticity”, etc. (internalist explanation). What does it mean to “overcome” this polarity? Nothing grandiose or mysterious. For a long time now, everybody knew that this is a false polarity. Certainly Mannheim already did. “Overcoming” it, however, means to invent a device that permits you to interpret the motivations for action without at any point occupying the Machiavellian position of the master strategist, the prince, who has full knowledge of his interests. Neither is it permissible to explain action as “disinterested” - disinterest, explains Bourdieu (1975, 32), is nothing but orientation to “a system of specific...interests which implies relative indifference to the ordinary objects of interest - money, honors, etc.” – nor is it permissible to explain it as a rational calculation of interests. Bourdieu’s solution, therefore, is to speak of actors as investing in the particular illusio of the field into which they enter, and thus, even as they pursue their interests they are never masters of their own strategies. That this is what the concept of field is designed to do, can be seen, for example, by the quote from Bachelard that Bourdieu (1975, 19) chose as motto for his
article about the scientific field: "The training of the scientific mind is not only a reform of ordinary knowledge, but also a conversion of interests." The concept of field is all about interests and motivations, and correspondingly, it says very little about the problem at hand, namely the modality of activities. Additionally, this is also why it resorts to the image of a thin and strict boundary, since investment in the *illusio* of the field means that each actor orients his or her action to only a limited set of recognized and recognizing actors.

Put differently, even if we did shift the lens and analyzed the space between fields as another field, we would gain very little. We would be guided towards conceiving of this field as a “lesser” one because the circle of recognizing others would be too heterogeneous and unstable – “lesser,” namely less autonomous, *ergo:* less of a field. Similarly, we would be led to conceive of the actors in it as relatively weak and incapable of controlling their clientele, instead of attending to their actual powers of invention and recombination.

Latour’s concepts of network and hybridity, by comparison, deal precisely with the issues of modality and boundary neglected by Bourdieu. Yet, they have their own blind side. If you want to understand what this paper is about, you have to know two things about me: I like French social theory, and I like peace. So this is my modest attempt to negotiate peace between Bourdieu and Latour, between field theory and actor-network theory: let’s give fields to Bourdieu, and the spaces between them to Latour.

Just like Bourdieu, Latour (1988, 218) says that his concepts are meant to overcome the false polarity between internalist and externalist explanations. That’s good, because it means that these two enemies share another enemy (in fact, two other enemies) and the noble goal of defeating it. But on closer inspection, it is clear that by this polarity Latour means something different than Bourdieu: not the opposition between self-interest and disinterest, but the opposition between objective fact (internalist) and social construction (externalist). What does it mean to overcome this polarity? To invent a device that permits to explain scientific practices without at any point occupying the post-hoc position of full knowledge or Truth: neither is it permissible, he says, to explain scientific practices as “discoveries”, i.e. that they are somehow verified by our post-hoc knowledge of what is the truth about reality or nature; nor is it permissible to explain them as “social constructions”, thereby assigning “society” with somehow greater reality and agency, and trivializing scientists’ actual, practical grappling with recalcitrant materiality. The concern of this approach, therefore, is with materiality, with specifying the modality and content of practices, precisely because it is no longer permissible to conceive of neither nature nor of society as “really real”. Latour’s (1987, 86-90, 121-132) solution, therefore, is to speak of the scientist neither as discovering, nor as constructing, but as “recruiting”, “mobilizing” and striking alliances with the objects of their study. This means perforce that the boundaries between nature and society, reality and discourse, science and politics, are thick and fuzzy, and that they are crisscrossed by the networks that scientists weave in order to recruit allies, or as Latour puts it, the boundary is “internal to the network”.
At the same time, what Latour (1987, 118-119) calls the “secondary mechanism” or “purification” is responsible for producing the appearance – what Timothy Mitchell (1991) calls the “effect” - of a strict and impassable boundary between two entities: the work of all the allies, from the microbes to the manufacturers of instruments to the public authorities that implement hygienist policies, is attributed to the ingenuity of Pasteur who “discovered” the microbes; the actions orchestrated by a strange network, composed of Halliburton, of a vice-President with one foot in each game, of private intelligence and interrogation contractors, of local turncoats with their own Pentagon-funded organizations, of a professional army and a national guard with their own private contractors, are attributed and appear as “state action.”

This can explain why, aside from the vagaries of the French intellectual field, these two thinkers and their camps were so dismissive of each other. Indeed, I would argue that each is strongest precisely where the other is weakest, and each privileges precisely what the other discounts. Their omissions are, in a sense, symmetrical, and for this very reason, while their theories lead in different directions, they could also be complementary.

What Latour probably finds the most problematic in Bourdieu’s theoretical edifice is the strong insistence on the “epistemological break” between science and commonsense knowledge (as well as “false science”). This insistence is encapsulated in Bourdieu’s claim that concepts such as “space” and “field” represent the “objective” moment of scientific inquiry with which it is possible to construct a representation of the total context of action. This claim is the complete inverse of Latour’s sociology of science, and of his careful avoidance of privileging any point of view as the “truth” or as “objective”. As Latour (1987, 78-79) says, what is deemed objective and what is deemed subjective are relative to “trials of strength” in specific settings. These are not qualities affixed once and for all to actors or theories, because actors strive to fortify themselves so to become “objective” and to deconstruct others so they become “subjective.” And his view of the epistemological break is no less scathing: "'Science' - in quotation marks - does not exist. It is the name that has been pasted onto certain sections of certain networks, associations that are so sparse and fragile that they would have escaped attention altogether if everything has not been attributed to them." (Latour 1988, 216) It is impossible, therefore, not to conclude that from Latour’s point of view, Bourdieu’s treatment of his own scientific practice is hopelessly internalist.

If they ever cared to pay attention to Latour, Bourdieu’s camp probably should be the most riled up about his treatment of interests, encapsulated in the concept of “translation.” Translation is a general term meant to cover the process whereby the interests of both humans and non-humans are interpreted, modified and adjusted to one another so as to create alliances. It thus contains the important insight that oftentimes it is not interests that explain alliances, but the other way around, the fact of alliance is what explains how the interests of the various parties are construed and adjusted. Latour is insistent that the principle of symmetry requires that we treat alliances with humans and non-humans as essentially the same and use the same language to describe them. From Bourdieu’s point of view, however, this “symmetry” is bought at the price of an even more problematic asymmetry, that between who Latour calls the “fact builder” (almost
always a scientist or an engineer) and those he or she aim to snare in their net as allies. The crucial section in Science in Action (Latour 1987, 108-121), I would note, where Latour expounds the idea of translation, is written completely from the point of view of the “fact-builder” and the language used to describe his actions is Machiavellian (Latour indeed pays homage to Machiavelli, and seems proud to do so): the fact-builder’s problem is, to put it simply, how to recruit allies by convincing them to align their interests with his. Once they are recruited, their actions are still unpredictable, and the problem is how to control their actions and channel them towards the desired goal. Thus, the “fact-builder” is the rational strategic actor par excellence. This is the complete inverse of Bourdieu’s sociology of interest formation, and of his careful avoidance of privileging any actor as the master strategist. If translation is the case, from Bourdieu’s point of view the issue would be precisely how to take account – in some sort of parallel, not serial, processing - of all the translations that all the different members of a network simultaneously operate on one another. First, no account is given of the processes by which the fact builder’s own interests were formed or translated. These interests are taken to flow directly from the need to build and secure the said fact. No meaning or interpretation intervene between this (imputed) need and action; nothing like habitus, socialization, training, serves to connect the interpretation of interests with social position. The second point is that no account is given of the processes of translation that the other parties to the alliance operate on the strategic actor. This is what I mean by asymmetry. Rationality is on the side of the Machiavellian actor, who translates (aligns, adjusts, interprets, shapes, distorts, misrepresents) the interests of all the allies, who themselves are either led, misled or resist. Of course, one could then shift the focus and describe the situation from the point of view of one of the allies (just as Bourdieu could shift the focus and describe a new field in between two old ones), but this is precisely the point: the Machiavellian language of “translation” is not geared to take into account all at once, symmetrically, all the members of a network as partially rational, partially determined actors. I would argue that this is precisely what the language of “field” (and

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I would argue that the Machiavellian language of mobilizing allies and controlling their behavior is the price one has to pay if one wants to include in the account the non-humans as actants, and to provide a completely symmetrical account. “Translation”, when it comes to the non-humans, does not have to include any account of meaning and understanding. So this dimension gets reduced and truncated in the Latourian treatment of humans as well. Latour would probably reply that the very idea of “meaning” as something that precedes and determines the act of translation is metaphysical. I agree. As Lakoff (2003, 59) and Johnson have shown, it involves a “container” or “conduit” metaphor, according to which words “contain” and “convey” meaning. This metaphor is very useful for our everyday dealings, but must be rejected as a serious account of language and meaning. Meaning is not inherent in words, but is created in and through use. One could even say that meaning is what appears whenever one “translates” from one metaphor to the other. It exists in the space between words. But all of these considerations do not change the point that when it comes to humans, unlike the non-humans, an important dimension of translation is discursive and involves understanding. It involves linguistic persuasion aiming to get the other party to meaningfully understand their interests as aligned with the fact-builder. The very term “discourse” conveys the point that while words do not contain meaning in and of themselves, and could be interpreted and pushed in different ways, linguistic usage is anything but random. It is structured by systematic practices, rituals and habits, which are themselves the product of concerted pedagogic action. From very early childhood and all the way through the rituals required in order to enunciate rarefied forms of discourse, pedagogic action works to stabilize and reproduce the meaning of linguistic formulations, first and foremost through embodiment and habituation, i.e. it produces the very “metaphors we live by,” those that model a less physical form of experience upon a more physical one. (Bourdieu and Passeron 1977; Foucault 1986). It would be
habitus) is adept at doing. It is impossible, therefore, not to conclude that from Bourdieu’s point of view, Latour’s treatment of the question of interests is hopelessly externalist.

It is clear now what feats of juggling (of keeping one ball in the air at all times) it would take in order to negotiate peace between these two theories: in order to account for the formation and mutual adjustment of actors’ interests, one would need to keep the concept of field. In order to account for the formation and mutual differentiation of practices, however, one would also need to keep Latour’s concept of a network connecting humans and non-humans and straddling the boundaries between what are known as “science”, “society” “politics” and “nature.” If these two concepts are to sit comfortably together, several provisions must be made: the theory of fields must shed the all-knowing narrator’s voice that Bourdieu adopts, speaking from the point of view of scientific objectivity; actor-network theory, correspondingly, would need to part with the Machiavellian narrator’s voice that Latour adopts, speaking from the point of view of the prince. Field theory will also have to make room for some of the insights contained in the concept of “translation” (particularly the point that distance – that objective measure par excellence in Bourdieu’s theory – could be modified by interpretation); but at the same time, actor-network theory would have to grant more of a reality to the phenomenon of a bounded sphere – which it tends to treat as appearance or “effect” – and accept that to explain it one would need sturdier mechanisms than the flimsy secondary mechanism of attribution and purification. I do not know how to do all that, unless by means of a truce, a partition of jurisdictions: fields to Bourdieu, and the spaces between them to Latour.

Back to the problem of orientalist expertise

I first attempted to deal with the problem of the relations between Middle Eastern studies and military intelligence from within Bourdieu’s research program, but with some telling modifications. I opted for the third line of defense discussed earlier and analyzed the exchanges between these two as taking place within a “field of orientalist expertise.” To construct this field I had to ignore the boundary between the state and academia. In a sense, this was an intermediate field between the two.

Figure 1 presents a simplified sketch of this field, modeled after Bourdieu’s conversion of Weber’s Sociology of Religion, and serving as a prototype for any field of expertise. The principal opposition in this field, along the horizontal axis, has to do with the degree of independence/dependence of the experts from the consumers of their services. As Weber and Bourdieu argue, experts seek to control the demand for their services, and their major coup in this respect is when they are able to completely dictate to the consumers what completely asymmetrical, therefore, to assume that the fact-builder is somehow less determined by discourse, somehow is less subject to the embodiment of these practices, habits and rituals, somehow freer in the use of linguistic formulations than the presumed allies. Obviously in some cases this may be true to some extent, but these are simply cases at the furthest extreme pole of a continuum (in fact, the idea of a space between fields can make sense of these extreme cases, since it is a space of relative freedom, wherein marginality also means that actors are less determined). But such extreme cases are not the rule. This is why one needs concepts like “field” and “habitus.” With non-human allies, however, these considerations do not come into play at all.
their needs are. The secondary opposition, along the vertical axis, has to do with the 
degree of openness/closure of expert knowledge and expert recruitment, i.e. with the 
control over the supply of expertise. Weber and Bourdieu argue that experts seek to 
create artificial scarcity of their expertise, both by limiting access to the knowledge they 
produce, i.e. making it esoteric, as well as by limiting access into their ranks through 
exams, ordeals, credentialing systems, etc.

I had, however, to modify Bourdieu’s framework for my needs. Unlike Weber, Bourdieu 
did not seem to appreciate the extent to which the accumulation of resources and the 
attainment of domination within fields of expertise was a precarious balancing act. The 
thrust of his model was towards defining power as a situation where experts fully control 
both the supply of and the demand for their expertise. How else to interpret Bourdieu’s 
definition of the specificity of the scientific field? As you may remember, he defined it as 
the situation when "the producers tend to have no possible clients other than their own 
competitors (and the greater the autonomy of the field, the more this is so)." Put 
differently, so great is the independence of the experts from their clients, that they have 
no clients! This definition was designed to maximize this element of control over 
demand, or in other words, autonomy, and Bourdieu is also explicit that it is the result of 
a high degree of control over supply and a formidable accumulation of scientific capital, 
i.e. power, autonomy and full control over supply and demand are all equated in his 
scheme. But this is ridiculous! If they had no clients whatsoever; if nobody was interested 
in what they did, what kind of power would they have? And how on earth would they 
have been able to control supply so well? (Unless because nobody wants to enter such a 
disconnected field.) On this point, see Latour’s (1987, 145-175) devastating proof that if 
science is to have an “inside,” as he puts it, it is only by virtue of having an “outside,” 
and the more and “harder” inside it has, it is first and foremost due to the extensive work 
performed outside.

My choice, therefore, was to conceptualize strength or domination or capital in a field of 
expertise as predicated on the capacity to balance the contradictory imperatives of 
independence vs. dependence and closure vs. openness (which, adopting Nikolas Rose’s 
[1992] usage, I called “generosity”). I thought that, in fact, this way of thinking about the 
problem was much truer to Weber’s analysis. The main point is that the opposition 
between independence from or dependence on the laity is truly a dilemma or a double 
constraint: too much dependence on the consumers exposes the group of producers to 
competition, critical appraisal and devaluation; but too much independence may lead to 
isolation and loss of clientele (the “ivory tower” phenomenon). The same holds for the 
opposition between generosity and closure. Weber (1963), after all, notes that the closing 
of the scriptures, and the monopolization of their interpretation at the hands of a small 
and select group, leads to a peculiar form of tension within the priestly vocation, between 
those entrusted with maintaining the purity of the original revelation and those who 
engage in the everyday pastoral work among the laity. The latter, first of all, are more 
dependent on the needs of the laity. Not every household dispute, every confession of sin, 
every delicate business question, could be answered by relating it to the doctrine of
“grace.” The scriptures, the revelation, must be made to work and shown to be relevant to the life questions of the laity. In Nikolas Rose’s (1992) language, it must become possible to “graft” it onto other forms of knowledge, discourses and technologies of the self. If they were to stick closely to the scriptures and remain distant from the needs of the laity, the pastors would become irrelevant and their place would be taken by magicians, soothsayers, oracles, diviners, witchdoctors, and the like. Moreover, they cannot afford to remain esoteric. They must make the laity understand at least portions of the revelation, appreciate its relevance to their life problems, and apply it to themselves. Put it more generally: there is an inverse relationship between how esoteric and monopolistic expert knowledge is, and the capacity of the expert group to present itself as useful and relevant for the laity.

Hence the position of the research institutes – where Middle Eastern studies and military intelligence intersect and fuse – at the center of Figure 1. In my research (Eyal 2002; 2006), I have followed the actions of two generations of doctoral students and young lecturers at the Institute of Oriental Studies at the Hebrew University. I have shown that their professors exercised such strict control over the supply of their expertise as to greatly limit the mobility of these two groups. Moreover, they also cultivated strict independence from the needs and demands of their possible clients (the political and military leadership), and thereby earned themselves and their students relative isolation and irrelevance. Nonetheless, as their students rebelled against this definition of orientalist expertise and moved to take up positions, first in paramilitary intelligence and the Jewish Agency, and then later, after 1948, at military intelligence and the Foreign Office, they discovered that what they gained in relevance, influence and proximity to their clients, they lost in independence and the capacity to control the attribution and dissemination of their discourse. Put simply, while the leadership now listened to them, it did not have to acknowledge them (unless when predictions and assessments failed, then the blame could be apportioned to them). They were subordinate officers, taking orders and submitting reports. Whatever insights were contained in such reports, the leadership could pass as its own. I argued that the invention of midway research institutes provided a way out of this quandary, a means of balancing a measure of independence with proximity to clients’ needs, and of generosity with the capacity to control the attribution and dissemination of expert discourse. Top political and military decision-makers are routinely invited to these institutes, to participate in conferences and seminars, and some serve on their board of directors. Military intelligence officers and foreign office experts routinely spend a year as guest researchers at these institutes, and some move to full-time positions there after they finish their service. The result is that the professors who control the research institutes, who at the same time also lead academic departments of Middle Eastern studies, are capable to enjoy both proximity to the men of power and prestige as independent commentators. The research institutes have become the site where a great deal of resources – financial, academic, administrative, social connections – are accumulated and converted into political influence, academic power and even high-level political appointments. In short, their leaders have managed to become the most dominant group within the field of orientalist expertise by balancing monopoly with generosity, independence with dependence.
I was not completely satisfied, however, with this way of conceptualizing my results. The main benefit of using the concept of field, reworked in this way, is that I no longer needed to attribute selfish motives *per se* to my actors, nor did I need to present them as grand strategists who have designed the research institute from scratch to “solve” their problems. I was able to show how their actions were guided by a philological *habitus* acquired in academic settings, then carried over to military intelligence wherein it generated homologous practices and institutions, thus permitting the formation of halfway institutions between the military and the academy. At no point did I need to add the device of rational calculation or disinterestedness to explain the motivations of my actors, who were guided by the acquired desire to recreate the social hierarchy of the philological *modus operandi* (composed of apprentice, pedant and speculative interpreter) and to occupy its apex – the position of the speculative interpreter, converted within military intelligence into the position of a researcher who assesses the *intentions* of the other side.

The main disadvantage, however, of thus deploying the concept of field, even when modified by the idea of “generosity,” is that it requires us to maintain a somewhat artificial distinction between “experts” and “clients.” It is as if expertise was a thing, an entity (a set of “skills”), which could therefore be possessed by the experts and provided to the clients. But there were several aspects of my subject matter that defied such easy division. The most important had to do with the public and political functioning of the assessments written by the research branch of military intelligence, as well as the commentaries written by Middle East experts at the research institutes. Such assessments and commentaries, it became clear to me, could not be analyzed as simply the opinion of the experts about what is likely to take place in the Middle East. They must be analyzed as political speech acts that are central to the Israeli political system. (Peri 1983, 167) Intelligence assessments, for example, while they are officially submitted to the eyes of the Chief of Staff and the Government alone, have another, more important form of existence: their main points are almost immediately leaked to the press, either by politicians, or by high-ranking intelligence officers who have developed their own set of give-and-take ties to military correspondents. The position of the research branch on this issue was summarized by one of its former commanders: “The principal points of the intelligence assessment should be passed to the media, so it would present them to the public. The public must know the truth; it is an important moral we drew from the Yom Kipur fiasco.” (Amos Gilad, quoted in Schiff 2001). Such leaks permit intelligence officers to gain some power vis-à-vis the politicians. Even when political and military decision-makers are interested in the supply of specific assessments and recommendations to support their policy choices, and when these may be different from the assessments of the research branch, they are forced to avoid, as much as possible, overt conflict with the research branch. Supply and demand must coincide. The result is a process of negotiation, in which official policy and intelligence assessment are slowly adjusted to one another, facilitated by the social proximity between the commanders of military intelligence and the political hierarchy. In essence, such leaks turn the

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2 There were, of course, notorious cases in which an open controversy erupted between the research branch and certain decision-makers, but the status of these events as “scandals” shows that they are the exception that proves the rule. The most famous such scandal was the disagreement between the commander of
intelligence officers into de facto political players. Much the same could be said of the Mid-East specialists working at the research institutes. Sometimes they participate directly in the formulation of intelligence assessments. Other times, they act as commentators in the media, reacting to the assessment, validating it, repeating it, unfurling its various implications and thus contributing to its overall market value.

It follows, therefore, that intelligence assessments are not merely predictions about what may happen, but policy tools, messages directed at politicians, the Israeli public, the enemy or the international community, and calculated to elicit certain performances. The intelligence assessment is at one and the same time the lens through which the enemy’s actions are viewed as indicating certain intentions; a message to the other side that their schemes have been exposed (as well as a message to the Israeli public or the international community that this is the state of affairs, i.e. propaganda, or in nicer language, “public relations”); and the interpretation guiding Israeli policy in reaction to these intentions. Raymond Cohen (1989) demonstrated how this mechanism worked in 1986, when tensions between Israel and Syria escalated to the brink of war: the research branch assessed that Syrian maneuvers indicated aggressive intentions. This assessment was leaked to the press, so as to warn the Syrians that the game was up. The Syrians, however, understood these leaks as a different kind of message, a threat that may indicate an Israeli intention to attack. As a result, they mobilized their forces. Israel responded in kind, and the resulting dynamic of escalation validated the original assessment. While Cohen sought to warn against the dangers of escalation inherent in such assessments, I think this is not necessarily the most important point. It is just as likely that the opposite scenario could have taken place, i.e. that the Syrians would have cancelled their maneuvers in order to avoid escalation. This was exactly the performance that the assessment sought to elicit. In this case, the assessment and the reaction to it would have been presented as a success, because in this way the adversary’s intentions were “foiled,” i.e. it would have been validated all the same. With respect to this mechanism, it is difficult to make a strong distinction between “experts” (intelligence officers) and “clients” (politicians). In fact, if we consider how this mechanism functions in the long range to produce a stable state of affairs, it doesn’t even make much sense to make a strong distinction between the subject of knowledge (the expert) and the object of knowledge (the enemy), since the latter’s performances are part of what produces the stability and validity of the former’s assessments.

It should be clear to the reader that these considerations emerged for me once I was no longer preoccupied with explaining the interests and motivations of the actors who established the research institutes – for which I needed the device of an intermediary field – and when my attention turned to questions of modality, of characterizing the specific nature of the form of expertise practiced at the research institutes. This form of expertise now appeared to me to be neither a property of an individual nor even of an organized group (a profession), neither a given body of knowledge nor an acquired skills set, but a network connecting together individuals found in different positions and with different military intelligence and the Defense Minister, Ariel Sharon, over the scenario the latter painted in order to justify extending the invasion of Lebanon in 1982 beyond previous and more limited plans. (Yaari and Schiff 1984, 24, 99-122)
skills, as well as connecting them with arguments, devices, resources, demonstrated effects and models. To say that expertise is a network means that we treat the distinctions between expert and client, or between subject and object, as provisional and internal to the network. There are no strong and impassable boundaries between those who are empowered to speak as experts; those who are empowered to listen to them and “make decisions”; those who produce knowledge but cannot speak, humans as well as non-human devices; and those about whom they speak - the network of expertise also involves a set of alliances or manipulations meant to secure from those about whom the experts speak (in this case, enemy leaders) a set of performances predicted by the experts. For the network of expertise to function, all of these must be connected with one another, and certain mechanisms must control the flows of speech, work, information and performances between them. Additionally, for the network to function it must also continually produce the appearance of a strong boundary between at least these 4 sets of actors: experts, clients, foot soldiers/technicians and the enemy. But this is a secondary mechanism, superimposed on the more basic facts of connection, alliance, recruitment and division of labor.

It follows, therefore, that this network could be described as straddling, at the very least, the boundaries of the state, the academic field and the media field, crossing them rather promiscuously, indeed, even reaching across state borders to enemy states. Moreover, the position of the research institute, which plays an important role within this network, is best represented, as in Figure 2 (loosely adapted from Medvetz 2007), as located in the space between these fields, in the volume of the boundary between them.

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**Figure 2 about here**

**What are these spaces good for?**

Now that the idea of a space between fields has been at least indicated, I believe it is possible to give shape to this concept by asking about its utility. What analytical and conceptual tasks can it perform? How can it be put to use in the analysis of historical change? What advantages do we gain by thus supplementing the concept of “field”? I can think of at least two possible avenues:

**Boundary work:** we can return now full circle to where we started. I would argue that this notion of a space between fields, of a thick boundary zone, is crucial in order to provide a fully relational account of the differentiation between fields. Obviously, the key to such an account is the notion of “boundary work”. But the way this notion has been deployed up till now leaves much to be desired. Here is how Thomas Gieryn (1999, 4-5) defines it with respect to the field of science: “The discursive attribution of selected qualities to scientists, scientific methods and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science.” There are two problems with this definition. The first, and obvious, is the fact that boundary work is limited to rhetoric. The social mechanisms that limit the number of authoritative speakers, that assign their statements with differential values, that close off certain topics and devices from non-expert inspection, that characterize something as “calculable” or
“not calculable,” etc., these mechanisms are far more robust than mere rhetoric. Rhetoric alone would never have been able to produce the relational reality of science or the economy, or politics, etc. The second problem has already been noted: where does boundary work itself fall? What is the status of the practices, symbolic materials and persons who serve as boundary signs? While they mark the boundary between two sides, they themselves belong to neither side. They are hybrid. Put differently, the very act of drawing boundaries by the same token also transgresses them. All of this serves merely to point out that, cartographically, it does not make sense to depict boundary work as taking place within neither side, neither inside the field nor outside it in another; neither upstream nor downstream as Gieryn would have it; but in the space between fields, within the volume of a thick boundary. Fields secrete these thick boundary zones as an inevitable aspect of their functioning, as fuzzy zones of separation and connection.

Take, for example, the practices and mechanisms that constitute the “economy.” There is a famous claim of Michel Callon (1998) that “the economy is embedded in economics” or more precisely that economics and accounting format the economy. What does that mean? Something along the lines of the example I gave earlier, of the tax form. Callon points out the crucial role played in economic life by the identification of externalities, i.e. certain objects or activities are identified as factors or consequences of the economic process, but are nonetheless qualified as incalculable, outside the economy. This exclusion from the economic is, however, also a form of inclusion, because the identification of an externality is, of course, the first move in disentangling it from its entanglements in social contexts, a first move towards bringing it within calculation, i.e. re-entangling it within a network of market devices. Callon thus suggests a far more robust and interesting concept of boundary work. “Boundary work” is real work. It is not just the rhetorical castigation of something as non-scientific, non-economic, etc., i.e. drawing a “fine line”, but akin to the work of a border patrol: mapping the terrain, establishing connections to who lies beyond it, transacting with them. Moreover, Callon notes that this work of identification, disentanglement and re-entanglement is typically performed by economics. Hence the subtitle of his introduction: “the embeddedness of the economy in economics.” It is not typically the work of the elite of the profession, university-based, highly abstract mathematical modelers, but of practical, hands-on, rank- and-file economists in government and corporations, who continuously identify externalities and devise means of measuring, calculating and disentangling them. One gets the sense, therefore, that the constitution of the economy, of things economic, takes place in a boundary space between the economic field, the bureaucratic field and the academic field, by actors who have a foot in each of these, but by the same token are also somewhat marginal to each of these. What they do at once connects the economic field with the academic and bureaucratic ones – since to identify, measure, calculate and disentangle externalities, i.e. to qualify things as “economic”, to produce the specific modality of activities in the field, is a collaborative effort that requires the participation of scientists, politicians, administrators, etc. – and yet reproduces their separation since the very product of this collaborative cross-boundary effort is the qualification of things and activities a “economic” or “non-economic”
We can return now to the problem of the gift. We saw already how, on the page of the tax form, it straddles a space between the economic and the non-economic. To report something as a gift on our tax form is boundary work in the simple sense of excluding it from among things economic, but it is also boundary work in the more complex sense suggested above, since in order to be excluded the gift is included as a candidate economic fact, and is measured, probed, disentangled by the simple act of estimating and writing down its value. This sort of boundary work may serve as the basis for the IRS, for example, to intervene and determine that the gift is not a gift at all, and is taxable. We all know that corporations use gifts in order to brighten up their tax bill, hide money away, etc. In short, included in the tax form, the gift becomes a boundary object (Star and Griesemer 1989), at once connecting and separating the economic from the non-economic.

Let me return now to the case of Middle Eastern studies. Like the gift, the work carried in the research institutes functions at one and the same time to connect and separate Middle Eastern studies from military intelligence, the academy from the state. While at certain times the experts did not shy of admitting that “papers [written] in university research institutes…were mostly prepared in the framework of projects connected to the defense establishment or of a similar nature,” (Shamir 1979) on other occasions, when admission of such proximity became threatening, they averred that “we, as researchers, our role is to analyze, and not to educate or direct; to asses, without making recommendations that have a directive operative political application. As researchers our role is not to tell the government what to do.” (Itamar Rabinovich, quoted in Eyal 1974) In both cases, they attempted to carve for themselves, within the network, the position of those who deal with “basic research” or “didactic intelligence”, as they variously called it. The papers produced within the research institute, and more specifically the notion of “assess ment,” were thus boundary objects between the military and the academy, at once connecting them (because they used data produced by military intelligence and aimed at the same goal of assessing intentions) and separating them (because they were “basic” and “didactic”, not “operative”). The research institute thus lay in the liminal space between the bureaucratic and academic and media fields, connected them, and yet also producing the relational reality of separate spheres.

A space of opportunities: a different way to think about the space between fields is as interstitial in Michael Mann’s (1986, 15-16) sense, i.e. it is a space that is undetermined, where things can be done, combinations and conversions could be established, that are not possible to do within fields. In short, it is a space that has been opened up by some abrupt change, and that can generate even more changes. There is, in fact, one place in which Bourdieu (1996, 51-53) seems to make use of just such an idea. This is his discussion of the Salons of mid-19th century French high society ladies as “genuine articulations between the political and artistic fields.” In these “bastard institutions” it was possible for writers and politicians to rub shoulders. The politicians, typically second tier, could acquire powers that were not available to them in the political sphere; the writers could intercede and act as pressure group for material or symbolic rewards. And out of these exchanges and “shady deals” the structures and oppositions of
a nascent literary field begin to emerge. This example suggests several possible
dynamics:

1. **Field-making**: clearly, Bourdieu’s story is about how such a space, or articulation,
between fields became the site for the making of a new field, the literary field.
Arguably, Tom Medvetz (2007) has similarly shown how the space in-between
academia, politics, business and the media became the hothouse where the
contemporary American field of think-tanks was hatched. But focusing on successful
cases is dangerous, because we may think that the space between fields is not
interesting for its own sake, but only if it serves to bring into being a new field. A less
successful case has been studied by Lisa Stampnitzki (2008): terrorism expertise. In
the early 1970’s, and especially after the attack on the Israeli athletes in Munich,
various people become experts on terrorism. Some were academic political scientists
or psychologists, others were defense analysts at RAND, others were former military
intelligence officers, and others were journalists. One can describe, therefore, a space
of terrorism studies that lies adjacent, or in-between, the academic, journalistic and
state fields. This space is characterized by: a) permeability, entry into it from all the
other fields is relatively easy; b) under-regulation, rules about what one can
legitimately do are relaxed there. Academics can study an object that is essentially
defined by state activity – “terrorism” – and state officials can engage in research that
does not have clear policy implications; c) high stakes, the prizes to be had in this
field are relatively large – government money, inside connections and media fame –
and cannot be had normally in the other fields, precisely because this is d) a space of
articulation between the fields, where exchanges and alliances are contracted that
cannot be done within the fields; e) marginal actors – the actors that move into it are
typically marginal in their own fields, that’s why they opted out; f) stalemate – these
marginal actors engage in field building (for example, the academics produce dubious
“chronologies” of “terrorism events” and amass no less dubious bibliographies of
terrorism-related knowledge, they even write dubious retrospective histories of the
field’s emergence), but the struggle over the type of expertise dominant in this space
(which each brought with it) – military, social scientific, journalistic – is never
decided. Unlike the field of think tanks, no clear division of labor and hierarchy of
worth emerged.

2. **Raid**: stalemate is not the only reason why the idea of a space between fields, which
is a space of opportunity, can be insightful. Such a space may be significant and long-
lasting because it provides those in adjacent fields with the opportunity for a “raid”
strategy: incursion from another field, rapid amassing of profits, and rapid retreat into
one’s original field where these profits may be reconverted into currency that will
improve one’s formerly marginal position within it. A sort of “gold rush” image.
Let’s consider the development of human behavioral genetics, studied by Aaron
Panofsky (2006). This sub-discipline could be described as lying on the margins of
the scientific field. It is considered by many practitioners of animal behavioral
genetics to be only quasi-scientific, because the sort of controlled experiments
performed with generations upon generations of lab animals are obviously impossible
to perform with humans. On the other hand, this is a research site that generates a
great deal of media attention, and into which flows a great deal of funding (from
foundations and the government). For this reason, this space that lies between the natural and human sciences, as well as between academia, the media and the foundations field, is the site of constant incursions by actors who are relatively marginal within their own fields, or whose disciplines occupy a marginal or liminal position – failed animal behavioral geneticists, psychologists, professors of education, publicists and science writers. Each perceives this in-between space as the site where combinations disallowed in their own field, the sort of “grafting” Rose describes (which in this case may connect for example identical twin studies from psychology with DNA mapping and socio-economic stratification data), are permitted. Each perceives this in-between space as a site of opportunity, where resources could be accumulated and then possibly converted into an improved position within their own home field. For this reason, Latour’s Machiavellian language may indeed apply better to describe the actions of these actors, but only because of how their *habitus* was shaped by their specific trajectory within given fields. Marginality is the mother of invention and improvisation, of seeing value in heterodox combinations and in exploiting fuzzy frontiers. Moving into the relatively unstructured space between fields, these marginal and heterodox actors may indeed approximate the Machiavellian model.

3. **Fuzzification:** it is also possible, that the in-between status of the space between fields is valued for its own sake – not as a base for creating a new field or return to one’s home field – and its fuzzy nature is therefore actively cultivated and reproduced. There are great advantages in staying liminal. This is true, for example, with respect to what Justin Lee (2004) calls “hybrid wellness practices” – alternative medicine, spiritual guidance, techniques of body/self improvement. The sociology of professions always assumed that professionalization (i.e. qualifying for entry into an existing field of expertise), either successful or failed, is the only trajectory available for new forms of expertise. But a no less plausible trajectory, especially for those who were not marginal actors in scientific or professional fields but entered the in-between space from another direction, is to remain within it and exploit the fact that it is an under-regulated space. Instead of submitting to the close governmental and collegial regulation that comes with the status of professions, they may choose to suspend claims for scientificity or professionalism, and remain in the space that straddles the medical field, the field of personal services, etc. The idea of a space between fields allows to make sense, cartographically, of such choices. Such spaces of fuzzification and hybridity are typically formed in response to massive cultural and social changes, which deconstruct the existing system of distinctions and classifications. A case in point are autism therapies. (Eyal et al 2010) The massive deinstitutionalization of mental retardation, which took place in the early 1970s, opened up a space of opportunity as it destroyed the existing system of classification for childhood developmental disorders, and shook the self-confidence of the reigning medical and psychiatric professions. Because of the massive and sudden nature of the change, barriers to entry (in terms of credentials, funds, licensing) were rather low. At the same time, because deinstitutionalization meant that parents and schools were left to their own devices, in charge of kids who previously were institutionalized, there were vast opportunities for profits to be made in this space. Monetary profits, but more significantly symbolic profits in terms of “upgrading” the prestige and of practitioners
and therapists who were fairly marginal previously. Securing these profits, however, depended on striking a coalition with parents, which meant that techniques had to be kept simple and knowledge open. Low barriers to entry, high profits to be made, the necessity for keeping knowledge “generous” and open, all these meant that this was a “space between fields”, a space of opportunity, into which entered all sorts of entrepreneurs from adjacent fields – academic field, medical field, the market, special education and advocacy – each peddling a therapy that was low-tech, eclectic, not paying too much attention to psychiatric nosologies, and oriented towards an alliance with parents, in this way guaranteeing that the space would remain in-between, would not be colonized by adjacent, powerful professions, and as represented in Figure 3, remain open to new entrants from the marketplace, advocacy organizations, etc.

To conclude, I have offered the idea of “space between fields” as a strategic research site or a work space where it is possible to combine field analysis and Actor Network Theory in a fruitful way. Only time will tell if it will stick. Given, however, the current lack of productive interchange between field analysis, ANT and science and technology studies more generally, the sociology of the professions and even the sociology of intellectuals, my hope is that this idea would at the very least foster dialogue between these divergent and competing approaches by bringing them together into this work space delineated by the notion of “space between fields”.

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