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The Frenzy of Fields

An interview with Neil Fligstein on field theory and social skill

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DV: *One of the main problems with the way in which the products of American social science are received by European sociologists (and vice versa) is that the latter tend to have only a very superficial awareness of the state of the 'strategic action field' (i.e. contemporary US sociology) within which an intellectual project originates and develops. Could you perhaps sketch out the most important developments within American sociology, which led you to formulate the theory in its current form? You often mention the 'New Institutionalism' as both a positive and negative reference point for your work (see Fligstein 2001a, 2009), could you elaborate on that?*

NF: My view is that the discovery of the idea of 'strategic action field' (or any of its main variants: *field, domain, sector, organizational field, game*) happened not just within organizational sociology in the US but also across both political science and economics. The core idea is that there exist mesolevel social orders within which much of social life is organized. Such orders depend on actors (be they individual or collective) recognizing they are in some form of interaction with other actors where something is at stake. This means knowing who at least some of those actors are and orienting action to those actors (mostly strategically). Implicit in such an understanding is that there is common knowledge of the rules of the game, usually some form of hierarchy within a field whereby some actors are more powerful and others less powerful, and for many fields of consequence,

governments often help in the constitution of such fields. The imagery this creates is of a society consisting of millions of fields which can be densely interconnected. This idea is in contrast to common notions of something like 'macro' and 'micro' which have dominated our discussions of how to explain social action. This idea exists in 'new' institutional theory in economics, for example, in the theorizing of people like Doug North (1990) and Avner Greif (2006). But it is also a core tenet of game theory where players engage in strategic action under sets of rules with differing resources. It exists in political science in its game theory guise. But, it is also in work characterized as historical institutionalism. Scholars in this tradition are interested in how institutions evolve, how sets of institutions become interdependent and complementary and how social change is limited by existing institutions. Here I am thinking about the work of people like Peter Hall (see Hall and Taylor 1996), Paul Pierson (see Pierson 2004), and Kathy Thelen (see Thelen 2004). In sociology, 'new' institutionalism is associated with organizational sociology. The genesis of this idea was a reaction to organizational theories of the 1960s which emphasized that managers could read their environments, figure out what their main problems were, and alter their organizations to adapt to new circumstances. 'New' institutionalism saw the problem of figuring out what was going on 'out there' as difficult and therefore managers would have a hard time knowing what their problems were. They would also have a hard time formulating solutions and, finally, a hard time changing the direction of their organizations. This meant that the cognitive aspect of fields became important and the cultural stories people tell about who they are and what is going on central to the formation and ongoing stability of fields. It also suggested that such fields were social constructions and consisted of players with quite different understandings and motives. This led to the idea that fields were places that depended for their stability on actors following the lead of other actors who they perceived as successful (mimetic isomorphism), coercive isomorphism (following rules or laws laid down by government or important players), and normative isomorphism (doing what is thought to be 'normal' in a particular situation, often propagated by groups of professionals whose job was to advise managers). The exemplars in this were Meyer and Rowan (1977) and DiMaggio and Powell (1983) in sociology and March and Olsen (1965) in political science. The main negative reference point for me, is that they lack a conception of *strategic action* in the sociological version of new institutionalism. By this I mean, that the players are what Giddens (1979) has called 'cultural dopes'. They have no interests, no power, and their actions merely follow downloaded cultural scripts. Thus, the sociological version tends to underestimate power, conflict and the degree to which there exists hierarchy within fields. This has come to a fruition in the current literature in management studies (particularly in Europe) which focuses on the idea of 'organizational logics'. This idea is that the social structuring of a field represents a 'logic'. Researchers spend their time trying to understand how a particular logic emerged and how it helped create a field. Once in place, such 'logics' characterize the strategic behavior of participants and cause participants

to act in line with that 'logic'. There are recent attempts in this literature to allow multiple 'logics' to exist simultaneously in a field and even come into conflict. But, this very important change in language still does not solve the problem of how we recognize what a 'logic' is, who its agents are, and how conflict in a field is over who agrees and disagrees with the 'logic'. In sum, without notions of power, position, and what is at stake, the 'logics' approach tends to lack much agency and seems Hegelian in the sense that 'logics' operate a bit like the spirit of history, i.e. outside of us and behind our backs.

DV: *In the preface to your work, you indicate that this book has been quite long in the making, taking over two decades to complete. Could you briefly explain how two scholars from quite different backgrounds – one in economic sociology and the other in the sociology of social movements – came to converge on a field-theoretical approach? And secondly, what are in your view the most important developments that your conceptualization of SAFs underwent during these twenty years?*

NF: My own thinking about fields began in the early 1980s and came from my work on the emergence of the large corporation in the US in the nineteenth century and its transformation during the twentieth century (see Fligstein 1985). It is in the empirical study of corporations that I began to realize that these firms were in a field, one characterized by their watching one another and a strong role for government in constructing the rules of competition and cooperation. The real moment of discovery for Doug McAdam and I came in the mid 1980s when we were assistant-professors at the University of Arizona. At the time Doug had finished a book on the US Civil Rights movement (see McAdam 1988). He was working on *Freedom Summer*, a study of an important watershed event in that movement, one that actually precipitated social change and had a direct impact on other social movements of the 1960s in the US including the Free Speech movement, the Anti-Vietnam War movement, the women's movement, and the Gay-Lesbian social movements. What happened is that we both realized that the Civil Rights movement and the emergence of large corporations shared some basic sociological similarities. We came to understand that the creation of new social space was always about *social movements*. That is to say, that in order for social space to become ordered, actors had to figure out what the space was going to be, what was at stake, who was going to dominate, and how were political coalitions going to be built to construct new rules of the game and define identities. This was our first breakthrough and one that uniquely characterizes our approach to fields. We also realized that the processes by which fields emerged, then remained stable, and occasionally were transformed reflected quite different analytic problems, problems that could not be resolved simply with one theoretical understanding. Doug and I conceived of our broader theory of fields in this context and wrote multiple versions of a programmatic paper and a set of draft chapters that eventually turned into *A Theory of Fields* in the late 1980s to capture our insights. Our conceptualization got deeper as we continued to work on empirical projects and we had influence on those around us and they

on us. Doug and I talked to a large number of scholars about their projects and convinced them to employ field-like concepts. We also convinced many people of the value of seeing the emergence of new social space as social movements and bringing the social movement idea to places like markets. We both influenced many graduate students directly and indirectly. Folks using the ideas pushed us to develop them more. We tried several times to finish the book and when we finally did it, I think it was because we were finally ready to do it. There were several deep theoretical problems which were unresolved in the first version. First and most important was the realization that cooperation and hierarchy were alternative principles by which fields were organized. Second, we had a brief version of the existential basis for social skill that needed to be worked out. Finally, fleshing out our ideas about how fields interacted as groups of fields was important as well.

DV: *For most European readers the concept of 'field' is spontaneously associated with the work of Pierre Bourdieu (see Bourdieu 1993; Bourdieu 1996, also Bourdieu & Wacquant 1992: 94 ff.). Could you indicate to what extent your own perspective is indebted to Bourdieu's approach and on which points your own conceptualization of 'strategic action fields' parts ways with his work on 'fields of symbolic production'?*

NF: Whenever I am in Europe, this is the first question I am often asked. Let me answer it in a roundabout fashion. If you buy the idea that some notion of 'meso-level social order' exists in political science and economics, then it is quite a stretch to credit Bourdieu with being the principal author of the theory of fields. Game theory, for example, has been around since the late 1940s and Kurt Lewin's work on fields which Bourdieu acknowledges as inspiration, since the 1950s (see Bourdieu and Wacquant 1992: 97 ff.). Organizational theorists have been interested in something like field since the early 1970s. Bourdieu, in a passage discussing game theory, in *Invitation to a Reflexive Sociology*, clearly claims to have a different understanding of 'field' and 'game' than game theory even though he admits that game is central to his notion of field. His discussion of that idea means he was certainly aware of it when he was formulating his field theory. So, one might ask, how is Bourdieu indebted to John Nash? My main acquaintance with Bourdieu's work came relatively late. I did not really read him until the early 1990s after I arrived at Berkeley. *Invitation to a Reflexive Sociology* (Bourdieu and Wacquant 1992) was probably the first work I read (circa 1993). I later looked at *Distinctions* (Bourdieu 1984) and some of his earlier work on education. Over time, I have read more of the works as they have become more readily available in English. I teach *Outline of a Theory of Practice* (Bourdieu 1977), which I like very much. My work has been enriched by the encounter with Bourdieu. I admired his systematicity, the elegance of his framework, and the way that it seemed to provide satisfying answers to agent/structure problems. The trinity of 'habitus/field/capital' seemed to sum up a lot of sociological thinking in a sparse, yet rich fashion. It helps us understand how and why social structures, once in place tend towards reproduction. I like to think that had Bourdieu (who I met several times and

had the good fortune to interact with on a stay at his centre in Paris [*Centre de Sociologie Européenne*, DV]) lived to read *A Theory of Fields*; he would have liked it and seen it as both clarification and extension of his perspective. From my perspective, Bourdieu's discovery of the theory of fields was a kind of simultaneous discovery to those in organizational sociology, economics and political science. For me, the theory of fields holds out the possibility of presenting us with a kind of unitary social science that can only be imagined but probably never realized. This is because at the end of the day, the language and analytic frameworks of the disciplines and the problems they are applying the theory to, make it impossible for one group of scholars to really accept the premises of other groups. So, for example, if you have ever met a game theorist, you know they think the rest of us are just mushy-headed thinkers and don't get it. While from our perspective, their unrealistic view of social situations might be illuminating, but horribly incomplete and too simplistic. But your question was about how our work differs from Bourdieu. There are a whole set of issues that Bourdieu barely touched upon and that are central to *A Theory of Fields*. Let me just create a laundry list for you:

1. We were interested in how individual and collective actors got collective action. Much of Bourdieu's empirical work on fields concerns individuals who were important either to the founding of a field (like his work on literary and artistic fields) or who encounter fields (like his work in *The State Nobility* or the people that inhabit various places in the social structure in *Distinctions*). He never really theorizes collective actors and how people get collective action. We see fields as meso-level social orders not just as individuals encountering some other individuals or a pre-existing set of social understandings, but as involving players some of whom are individuals and some of whom are groups. For us, meso-level social order resides at every level of interaction. So, for example, one of the places where our field theory is having an impact is on the study of international organizations and the construction of social fields to engage in forms of collective action. Scholars interested in topics like climate change, human rights, the creation of rules for international trade, all face studying organizational arenas that are the social construction of the organizational players. In the book, we call this the 'Russian doll'-nature of fields. Fields overcome the issue of being macro and micro by suggesting that fields exist at all levels from the system of nation states to individuals vying for position in a job market.

2. This immediately raises a whole set of questions for which Bourdieu's theory does not entirely offer us satisfactory answers. The most important question is what is a field? For us, fields are political by nature and reflect the outcome of political processes. We see two possible social principles at work: hierarchy and cooperation or coalition.

3. We note that for Bourdieu, fields are always only about power and hierarchy. We think that this is because establishing social space by fiat or with the intention of the largest and most important players is the easiest way to create social space. But, it is also possible for fields to be more cooperative or coalitional in nature. How can that be? We argue that if at the founding of fields, there exist collective and individual players with roughly the same power, then they form the field by

agreeing to a political coalition usually put into place under a compromise and often by creating a collective identity.

4. This raises another question: how do actors attain collective social action? At the core of our theory (and this differs from Bourdieu's conception of 'habitus') is that humans have what we call 'social skill' which is the ability to get others to cooperate with you. The theory of social skill is necessary to understand how collective actors can emerge in the first place, and how the representatives of collective actors can cooperate or compete with each other. For us, at the core of social skill is the concept of empathetic understanding and with it the key role for the creation of identities. In the book, perhaps the wildest chapter is one on social skill. Here, we argue that at the core of social action, whether spoken or not are existential questions, questions like 'who am I?'; 'what is the meaning of life?'; 'how do I show I belong'? For us, the construction of fields involved creating collective identities for actors and producing meaning.

5. The whole idea of field begs the question about how do fields come into existence, how do they remain stable, and under what conditions are they transformed? Here, our theory using social movements, not as a metaphor but as an actual description of how new social fields, came into existence. This pushes a frontier that Bourdieu hints at in places as well as Swidler in her influential 1986 essay on culture (see Swidler 1986), and much earlier Max Weber. Weber, who is the source of this for both Bourdieu and Swidler, saw new social order coming out of the crisis of the old order. An old order had to be collapsing before it might be swept away. A new social order required a charismatic leader who would offer his followers an identity that gave them meaning and provided their allegiance to the leader. Social movement theory and our theory of fields situates this kind of analysis in a more general emergence of social space.

6. We also realized that the problem of figuring out what is going on in a field is not straightforward. Players in a field are limited in their thinking in a number of ways. First, they are shaped by their experiences and their positions. This means that their version of what is going on and their interpretation of other players will be very much from their perspective. This means there is a lot less consensus in fields than we think, conflict is more normal, and fields frequently less settled than we believe. Second, it also means that many fields have ways in which information and ideas about what is going on, what should be going on, are disseminated. We argue that such internal governance units (in the case of markets, usually things like trade associations or standards-setting organizations) are often thinly veiled ways in which incumbents inform challengers about their actions. But such units can also represent the field to other fields and in particular to governments.

7. Finally, is the biggest frontier issue in field theory; the nature of the relationships between fields. It is my opinion that Bourdieu's study, *The State Nobility* is one of his greatest works precisely because it studies the relationships between concrete social fields and delves into their interdependency. Here, in the book, we try and clarify more clearly what the nature of relationships can be between fields, how ensembles of fields form, and how the actions of fields end up

providing most of conditions that produce the transformation of fields (the so-called 'external shock theory' which so many scholars use but find so unsatisfying because it seems like an ad hoc explanation of change). Our argument is to try and embed fields in other fields and in understanding their links, be able to construct arguments about when nearby fields produce destabilizing effects and when such fields actually provide support for the status quo.

DV: *Interestingly, you chose to start your book – which provides tools to quite fundamentally re-think traditional macro-sociological questions (stability, change, development, crisis, interdependence, etc.) – by outlining a microsociological foundation. Could you briefly sketch out the core of this foundation and, perhaps more importantly, indicate why you felt compelled to provide such a foundation in the first place?*

NF: For many years both Doug and I were troubled with a lack of interest in sociology in trying to construct a distinctly *sociological* theory of action. The two main contenders are one we borrow from economics, i.e. the 'rational' or 'bounded rational' actor and the one we get from Durkheim, people who obey norms because they are fearful that others will sanction them or alternatively that we seek out approval. In the 1980s, we think that sociologists tried to solve this problem in a somewhat new way by invoking 'culture'. Here, I have in mind work by Sewell (1992), but also Giddens (1984). The basic idea is that social change comes from the conflict between different cultural understandings. Instead of endowing actors with some real agency, cultural sociology substitutes shared meanings, cultural frame and cultural conflict for structures in order to get new forms of structure. In essence, instead of relying on over-structural accounts of action, we began to rely on overly cultural accounts. Again, both of us were coming at this from empirical observation. What we saw were actors who were strategic, pragmatic and persistent. We were interested in strategic action. We also saw such actors behave quite irrationally from the perspective of rational choice theory. No one who wants to create something new can do it alone. Yet rational choice theory has us believe that we can get others to cooperate by offering them selective incentives in a situation where it is probably clear that we will benefit much more than them. Who will want to follow you if all you get is a few pieces of gold when the leader gets most of the benefit? This led us to begin thinking about a sociological theory of collective action. It is in thinking about such a theory that one needs to ask questions like, how do individuals and groups mobilize? This led us to wanting a sociological micro-foundation that would help give us insight into field-level processes.

DV: *It is at this point in your argumentation that we encounter the notion of 'social skill', a concept that seems strongly indebted to symbolic interactionism and Mead's views of social action in particular. Could you briefly indicate what social skill is and, especially, why it plays such a pivotal role in the overall functioning of SAFs?*

NF: The idea we constructed was social skill, which was based on Mead's work. In order to understand social skill, I encourage readers to pick up Mead's discussion of 'society' in *Mind, Self, and Society* (see Mead 1934: 227 ff.). I am the first to admit that Mead does have a somewhat optimistic, American middleclass version of what could happen in the future for humanity. But if you ignore that, you will see that he offers us a tremendously powerful theory of the role of empathetic understanding in creating collective social life. He also really understands the existential character of identity in determining who we think is like us and therefore our friend, and who we think is not like us and therefore our enemy. In essence, he proposes that social life requires us to cooperate with each other. This means that each of us has to learn how to appeal to others to allow us to be part of a group and to contribute to that group, and engage in cooperative actions to protect and reproduce that group. His contribution to field theory (or SAF theory) is to propose a theory of leadership that focuses on the ability of a leader to get others to cooperate. It is here that we see that leaders have to offer followers a sense of themselves that they find meaningful. It is also the case that such a sense helps create identities and makes people feel a member of a group. While leaders may materially benefit from their ability to do this, it is not their core goal. They need to be pragmatic, not narrowly driven by self-interest, but instead by creating a group identity that will motivate people to act. One of my favorite books that takes this theme up is Chris Ansell's work on the emergence of the French Socialist Party (see Ansell 2001) where he shows how the eventual leader of that party was able to overcome deep identity rifts between various parts of a nascent working class.

DV: *Since it plays such a central role in your account of how fields function, I was wondering if you could unpack this notion of 'social skill' a bit further? I am particularly interested in what one could call 'the determinants of social skill'. At times in the book (Fligstein and McAdam 2012: 48) and elsewhere (Fligstein 2001a) it seems like social skill is something that is quite equitably distributed across social space, where skilled actors can be found in all types of settings and walks of life. At other points you quite explicitly insist on the fact that skilled action is 'not a disposition or a quality of an individual' (Fligstein and McAdam 2012: 181), but rather depends both on the state of the SAF and one's position in it. This made me curious as to how precisely you view the conditions under which social skill emerges and especially its relationship to things like status-differentials. For instance, can social skill be construed as a type of 'capital', which can be acquired, transmitted and even monopolized by certain groups? Put differently, is social skill itself socially determined and hence amenable to sociological analysis or is it precisely that which introduces an element of (individual) contingency in the way in which fields operate?*

NF: This is a tough question and one that would require a long answer. First, I believe that we all have social skill to some degree. It is part of what makes us human. Durkheim was right: we have never seen a person raised outside of society or exist for very long without society. We are social creatures by nature.

By this I mean if we cannot get others to help us and if we do not respond to the needs of others, we will perish. Second, I do believe that social skill varies within populations of humans. I do believe it can be taught. I also believe that there will always exist socially skilled players on both sides of any SAF-interaction. I think it may be useful to think of social skill as a capital or a resource. One problem is that it seems to me to be of a different sort than say, social or cultural capital (which may be the outcome of social skill processes). If you 'google' social skill, what comes up are a set of proscriptive websites that offer ways to teach children social skill (a Meadian but perhaps also Foucauldian project!) But I would be happy to see someone develop this point. Now some have countered the argument about how social skill might matter, by saying that doesn't this mean that the social skill of actors on both sides of any interaction end up cancelling each other out? Put another way, in a situation where we have a set of collective actors vying for some ends and there are individuals on both sides with good social skills, isn't it likely that the winning hand will come from who has the most resources? Hence, structure rules as a determinant of what happens in particular situations? I certainly believe that this may be the case most of the time, particularly in settled fields. (I note that even in settled fields, social skill matters. Actors have to keep their groups together no matter what, and this is not entirely a given.) But, this view of fields ignores the possibility of the exact opposite situation. When resources are evenly distributed, doesn't that mean that the only possible difference in a struggle comes down to social skills? The problem, in reality is that our theories of resources and skill are not well enough developed that we can easily tell if resources are really equal or alternatively if resources are decidedly lopsided. There is much work to be done here. In extreme cases, say where one group has a near monopoly, resources certainly dominate, and in fluid situations at the beginning of fields where the goals in the field, the means to attain them, and what exactly a resource is, are undefined, skill will matter more. But most of the world lies somewhere in between and parsing out what is resources and what is social skill is really hard to do. Another objection to the social skills argument that is common is that when we see something novel, we attribute it to the skilled action of actors. This is a kind of selection of the dependent variable for people who want to raise this objection. But the same exact problem exists for those who want to have a more structurally determinist account. They have to assume that if something happens it is because the opportunity to make it happen is there and anyone can do it. Doug McAdam has recently written a book (McAdam and Boudet 2012) where he shows that in 20 situations where one could reasonably expect that collective social action could be engaged to take advantage of a political or structural opportunity, it only happens two or three times. This suggests that having a structural opportunity to make something happen is maybe a necessary condition, but certainly not a sufficient one. It requires actors to recognize such opportunities and be able to mobilize others to take advantage of them. McAdam's book suggests using social skill to get collective action is harder than we think. It is not automatic to get people to cooperate and get them to take time out of their lives to do something

new and novel. It also suggests there are lots of structural opportunities that go unrecognized all of the time, something that should make social change oriented sociologists both happy, but frustrated! Another place to see this problem is in the emergence of new social space. We have argued that skill might have its largest effects when social space is disorganized. After all, at that point, no one is sure what is at stake, what a resource is, and what are the ways to get others to cooperate. This means that the possibilities to engage in new forms of collective action appear and skilled actors being pragmatic can work to construct new political coalitions deemed impossible under other circumstances. So, in many ways, the best cases we have for the dominance of social skill are in understanding the founding of social space. Finally, my point about what social strategies will be deployed in a particular situation implies that social skilled actors are strategic. That is, they will use their repertoire of strategic action tools depending on their empathetic understanding of a particular situation. So, for example, if you are in a stable situation and you are the decision maker, you don't need to do much to reproduce that situation.

DV: *Let's return to the concept of a 'strategic action field'. The notion of a 'field' is just one among many of the concepts that have been used to characterize the type of meso-level social structures your book addresses. According to you, what advantage does talking about 'fields' have over other metaphors such as 'systems' (Luhmann 1995), 'sectors' (Meyer and Scott 1983), 'policy domains' (Laumann & Knoke 1987) or 'ecologies' (Abbott 2005)?*

NF: As I noted earlier, I think that many of these approaches have similar roots. I really do believe that fields are real and they do provide us with a general sociology that allows us to really begin anew the project of sociology as a science. Sectors, policy domains and ecologies are all subsumable under a general theory of fields from my perspective. Indeed, the work on sectors and policy domains were inspiration to both Doug and myself. But having said that, I think authors tend to prefer their terms and they tend to resist having their terms subsumed into other people's term. I find Luhmann's to be the most foreign to a field approach as I have outlined it here. It is also the case that the differences in these points of view can have real empirical implications. I do believe that observation is theory laden and if so, then different theories will cause us to pay attention to different things. If the study of fields moves forward, I would imagine a Habermasian dialogue involving undistorted communication about these differences (hey, we can try and imagine a genuinely intellectual exchange, can't we?). This way, these differences will have to be discussed and resolved mostly by understanding the 'scope conditions' of the different ways of thinking about fields.

DV: *While recourse to meso-level concepts is often associated with the desire to stress the autonomy of local social orders – a perspective that informs such seemingly disparate projects as Luhmann's theory of 'social systems', Bourdieu's work on 'fields' or Abbott's work on 'ecologies' – your book explicitly calls for a focus*

on interdependence which re-embeds fields within their broader environment. What led you to this shift from a 'field-centric' perspective to one that addresses the myriad relationships that tie SAFs together?

NF: I want to first disagree with your characterization of Bourdieu above who I believe is sensitive to the fact that dependence of fields is a variable not a constant.³ He also is open to the fact the fields are interdependent which is his central argument in *The State Nobility*. Empirical work really demonstrated this to both of us. While fields are independent, they can also be interdependent or even dependent. Indeed, one of the things we argue is that fields within themselves do not just have varying degrees of hierarchy and cooperation, but these principles occur across fields. This is a very eerie insight implying that the underlying structural principle of fields is very general. Hierarchy and cooperation are the two ends of a political spectrum that reflects various balances of power. In *A Theory of Fields* we have created a classification of the types of arrangements that can exist. But I do not think we or anyone else have a good grasp on the dynamics and the measurement of these relationships. We know only too well from empirical work that what happens in one field can disrupt what is happening in another. Indeed, most of our research on change begins with this kind of story. But, we know far less on how interdependence might produce stability and prevent change. We do need to study that as well. In our book, we have a retelling of the Civil Rights movement story. One of the key ideas in the chapter is that the legal oppression of African-Americans was not just a set of laws in the political field of Southern states, but a whole set of fields which provided support for that particular legal field. The unraveling of those laws would not have been possible without the crumbling of many of the fields that supported that system. This included the southern system of agriculture, the nature of the Democratic Party in the US, and the international conflict represented in the Cold War.

DV: *While your book makes a strong case for a focus on the interdependence of SAFs and provides an extensive conceptual vocabulary to analyze their interrelationships, I was struck by your quite parsimonious treatment of the structure of SAFs themselves. From what I gather, SAFs are marked by a (more or less clear) set of boundaries, characterized by 'incumbent-challenger' relationships, equipped with 'internal governance units' (IGUs) and defined by a more or less shared conception of the rules and positions that structure the field. Is this a result of your focus on the external relationships, rather than internal dynamics of SAFs or is it a quite conscious decision to keep the concept as flexible and hence widely applicable as possible?*

NF: I think the purpose is to keep the concept flexible and widely applicable. But, in practice, I actually think that the answer to this question (if I understand it correctly) is that it somewhat depends on what the analyst is trying to explain. Sometimes, we want to explain some set of internal changes in an SAF or perhaps its emergence in the first place. For such an analysis, it makes little sense to have

to construct an elaborate argument about where the SAF is situated, how it came to be that way, and its set of dependencies. Indeed, often an internal account of an SAF begins with an external shock, i.e. some disruption caused by its relationship to other fields. If the analyst wants to follow the story of the SAF, we think it is fine to just keep its 'situatedness' as a set of external constants. On the other hand, sometimes we do want to look at the ensemble of fields making up some sector. I think that Hall and Soskice's *Varieties of Capitalism* (2001) book is built on just that kind of field thinking. Liberal and organized capitalism are really a set of fields that they argue are mutually interdependent. This has been a very powerful theory, one that has spawned an impressive research program. I think this is because it does require the analysis of a multiple set of fields, their dynamics over time, and the possible causes of their stability and change. Also, in the book, the chapter with Doug McAdam's reconstruction of the Civil Rights movement and my rendition of the financial crisis illustrate the need to situate these events in multiple fields of interaction that are mutually supporting, and ultimately, the cause of transformation.

DV: In many ways your theory of SAFs provides a much more flexible and dynamic account of the way in which fields emerge, coalesce and disintegrate than previous uses of the concept. Your examples of SAFs range from the field of higher education, the structure of an individual firm, competition within the automobile industry or the US mortgage market to the emergence of a Cold War international order and the genesis of the French and German states. At one point you even speak of 'society consisting of millions of strategic actions fields' (Fligstein and McAdam 2012: 101) which arguably makes SAFs into one of the most widespread building blocks of contemporary societies. However, do you see certain minimal requirements before one can approach a research-object in field-analytical terms? Could a sports team, local community or individual family qualify as an SAF? Or are these questions your theory does not aim to address a priori and leave it to empirical analysis to sort out?

NF: Doug and I have discussed the 'scope conditions' of field theory. I certainly think a sports team can be a field unto itself and its embeddedness in the larger league (i.e. another field) is certainly a place where one can see interfield dynamics at work. Communities probably contain sets of fields that bear relationship to each other. So, one may be able to separate out a religious field, a political field, and different kinds of economic fields. Of course, these fields can bear relations to each other. It is certainly possible to see families as fields, particularly extended families. I think this is one of the places where field theory might make less sense. But the family and kin group are one of the building blocks of social action so it is not improbable to try to use field theory to inform our understanding of families. Maybe it could challenge the economic theory of families put out there by [Gary] Becker and his ilk (see Becker 1993). I think it is harder to use field analysis where actors or collective groups are harder to find. So, for example, applying field theory to gender relations or race relations in general might be hard to do. These are institutions in the sense that such practices occur in many settings. But, it might be possible to see how various

fields where such relations exist act to create reinforcing systems of fields (as we did in the book in the case of the Civil Rights Movement).

DV: Your theory attributes considerable importance to the role of the state, whose structure cannot only be conceptualized as a series of overlapping and hierarchically nested SAFs, but which is also crucial in facilitating or obstructing the emergence of other SAFs. What is it about the state that makes it so crucial to the functioning of non-state SAFs?

NF: One of the things we have pondered is the existence of types of fields. The idea of the types of fields originates with Weber who saw society as divided into distinct orders each of which had something different at stake. Before modernity fields were more of mixture of these types of ends. One of the interesting innovations in modernity was the separation of fields along lines like political, legal, economic, religious and social. Another, of course, was the self-conscious use of the creation of fields by actors. The separation of the political from the economic is one of the key features of capitalism (as Marx noted). The state is seriously important to the proliferation of fields in many ways. Without the state, many of the actors who constitute fields would not exist (like organizations). The claim of the state to make the rules for everyone in a territory is very important to being able to construct a society composed of millions of fields. Indeed, the modern state and the modern field society are both reinforcing and generally have co-constituted each other over time. This is because public order and general rules and laws about field formation are necessary for actors to build and proliferate fields, particularly in the economy but also in politics and the social world. Actors in fields appeal to the state to help them in their field construction projects and to deal with their crises. As we indicated, the state is a set of fields as well, but ones with the specific claim to organize other social spaces. The use of that claim makes other fields more stable.

DV: Your strong focus on the role of the state reminded me of similar work being done along these lines and especially of Loïc Wacquant's analyses of the state as a 'bureaucratic field' (which in turn aims to develop Bourdieu's work on this topic). Are there affinities between his approach and your own views on the state as a type of 'meta-SAF'?

NF: I think Bourdieu's paper on the bureaucratic field (which appeared in *Sociological Theory* in the mid-1990s) develops a view close to ours (Bourdieu 1994). One of the things that paper shows is some affinity to Althusser's view of the state and the role of ideological state apparatuses in supporting the state and the current system of power.⁴ Our view of this is to see again the interdependencies of these fields and their co-creation. In my work on economic sociology, for example, I have argued that modern markets and the modern state are co-constituted. Many of the capacities that states have evolved in the past 100 years including their welfare functions are in response to crises emanating from markets. So, we can look at the history of individual states as a set of political and

economic crises to which the building of new state capacity (i.e. fields) creates the possibility of order. One could read the history of the crises of a society through its expansion and extension of state capacity. Indeed, one could do an archaeological deconstruction of a given state by its adding such capacities over time layering old state fields with new ones. This is the main reason that in the varieties of capitalism literature, scholars have discovered that the Hall and Soskice idea of two types of capitalism (liberal and organized) do not fit empirical national models. Each national model has had its own trajectory and as a result has constructed quite different capacities to act and organize. Once in place, these have reinforced the fields of corporations and markets and particular social arrangements.

DV: *Your own conceptualization of SAFs also shows striking similarities with Andrew Abbott's work on 'ecologies' (Abbott 1988) and, more recently, 'linked ecologies' (Abbott 2005). Are there affinities between his perspective and your own and wherein, according to you, lies the main difference between a field and an ecology?*

NF: At one level, I think we are all saying more or less the same thing and to the degree that I understand what he is saying, I think it is compatible with these other ideas. But Abbott has developed a language to describe things that is not connected to these wider debates. So, I would be interested in hearing what he would say about how his ideas relate to those in the varieties of the 'new' institutionalism and field theory more generally.

DV: *To illustrate the analytical potential of your theory, you apply your framework to what is arguably one of the most ground-shaking social phenomena of the past decades, namely the recent financial crisis. Apart from providing a much needed sociological account of how the crisis unfolded, your own version of events tends to deviate from more mainstream explanatory schemes. In fact, whereas the latter often locate the origins of the crisis in the period of intense deregulation of the financial sector, initiated by the Reagan and Bush administrations and continued under Clinton (with the repeal of the Glass-Steagall Act as the pinnacle), you situate its roots much earlier, dating back as far as the Johnson administration. Could you elaborate on that?*⁵

NF: Sure. For me, the real interesting field change that brought us to the crisis was the shift in the US from the field where mortgages were sold to individuals by local savings and loan banks who held onto the mortgages to the field that later emerged where the purpose of mortgages was to pack them into securities and sell those securities to a wider investment community. So at the core of the whole crisis was the creation of the market for mortgage securities which had come to replace the market for mortgages. Without field theory, one cannot understand what was there in 2007 nor how it worked and how it got there. Before 1985, most mortgages in the US were originated by savings and loan banks. These banks were quite local. They drew their funds from local communities and they

lent to people in those communities. Their activities were heavily regulated, a product of the Great Depression. The interest they could pay on savings accounts was fixed by government regulation (say it was 3 per cent), the amount a mortgagee could be charged for the loan was about 6 percent. The bank lived off of the difference. This led to a standing joke about these bankers where they were described as living in a world which was focused on 3–6–3. They borrowed at 3 per cent, they lent at 6 per cent, and they were on the golf course by 3 p.m. These banks constituted a field of mortgage financing. This system was wildly successful from the Depression into the 1960s. Home ownership rates increased in the US from about 25 per cent in 1930 to about 64 per cent by the mid-1960s. During the 1960s, the Johnson administration, the last socially activist government in the US, was worried about what was going to happen when the baby boomers began to form families and try to buy houses. They were convinced that there was not enough financing in the savings and loans system to fund those houses. So, they wanted to create a program where more loans for homes would be available. Just like today, the government was worried about government deficits. So, they did not want to become the lender for all of those homes themselves. So, the economists at the FHA came up with a great idea. They would package a set of mortgages into a bond and sell the rights to the cash flow generated when the monthly payments people made on their houses were paid. The government would stand behind the bonds and give the full faith and credit of the government to the bonds. To do this, Congress created Fannie Mae, Freddie Mac, and Ginnie Mae as ‘government-sponsored enterprises’ (GSE). The first mortgage backed security was issued in 1970. There are a lot of things that had to happen before the mortgage market dominated by the savings and loans fell apart and was replaced by the mortgage securitization market. There are two keys to the story. First, is recognizing the government involvement at all critical junctures. Second, is the eventual building of the market for mortgage securities and the role of the GSE in producing that market. For me, what the deregulation argument misses is the fact that the government never really left mortgage markets to their own devices. The government’s fingerprints were all over the rules and regulations that created the markets and when the market for mortgage securities emerged, the government was there to promote it and offer its replacement of the failed savings and loan model. Indeed, the GSE took over when the savings and loans banks fell apart in the late 1980s and early 1990s and virtually invited commercial and investment banks to join them in making the market for mortgage securities.

DV: *Tying back to the main thrust of your book, how did the concept of SAF help you to disentangle the various causal strands that produced the crisis and how did it help you organize the results of your empirical work?*

NF: A theory of fields is necessary to a sociology of markets. It makes no sense to not consider markets as SAFs. This was Harrison White’s original insight (i.e. that market players watch each other, see White 1981) and it has unfortunately gotten lost in subsequent focus on networks, market devices and performativity. The creation of the

social structure of a particular market depends on many things, but if we myopically ignore that markets are centered on firms and their interaction, we will miss the big picture. Don't get me wrong: I love lots of the work going on elaborating market devices (Aspers, Becker, Karpik) and performativity (Callon, MacKenzie, Knorr Cetina). I just wish more of these scholars would widen their lens to consider how firms and government play a role in making the possibility for these things to exist. Donald MacKenzie is one of those who has wisely included these broader discussions in his work as a result, when you read his studies, you are satisfied that he has presented a coherent vision that situates the market product in the context of the firms and the regulators. I have written an entire book on the topic of markets as fields (*The Architecture of Markets*, Fligstein 2001b). For me, you have to start by asking 'what is the field, who are the players, and what is at stake?' This is what led me to realize that the market for mortgage securities was different than the market for mortgages. The players were different (savings and loans vs. large national conglomerate banks), the product different (holding individual mortgages vs. selling securities), and what was at stake (making money off of a long-term relationship vs. making money by moving mortgages through a pipeline whereby fees were generated and money was made off of bonds). It caused me to ask what caused the first, what crisis undermined it, and why did we end up with the market for mortgage securities? The fact that I discovered the government's hands all over the market is obvious once you start to read the history that has been written about the housing industry. Indeed, only if you ignore that history is it possible to see neoliberalism and deregulation as the key causal forces. By focusing on deregulation, scholars have missed how mortgages got turned into mortgage backed securities that entirely changed the game and created entirely new fields. It also puts sociologists in the odd position of agreeing with economists that the key feature of markets is keeping the state out (which they interpret as good and sociologists generally interpret as bad). We should never rule out state participation in any market without careful empirical and historical study. Seeing neoliberalism here misses the social construction of these markets and the key role government played in that process.

DV: *One of the perhaps most intractable problems facing a field-theoretical perspective (not to mention social science in general) is the question of the boundaries of fields or the question of which institutions, actors or groups are considered to be inside and outside of the field? Could you briefly indicate how you tackle this problem in your book, both conceptually and empirically?*

NF: Here, I stand with Bourdieu. It is an empirical question as to whom is in the field and where the boundaries stand. I think the key analytic problem is that individuals and groups participate in lots of fields simultaneously. In the 1950s sociology, we called this idea a 'role set' (see Merton 1957). Separating out players and fields get messy, because players have their hands in multiple fields. Since empirical reality is often opaque, to do research, we have to simplify things and here is where we deploy the concepts of the theory of fields to try and do this.

DV: *One of the aspects of the book I found particularly compelling is your resolutely 'ecumenical' stance when it comes to empirical applications of the theory. You argue, convincingly, that the theory does not favor any particular methodology (i.e. qualitative vs. quantitative) or research technique (i.e. network-analysis vs. correspondence analysis) and can cater to the needs of those who wield both 'positivist' and 'realist' conceptions of scientific practice. However, this does make me curious as to your own views. Do you view SAFs mainly as heuristic constructs aimed at informing research, or do you subscribe – like Martin (2003) and Bourdieu (2000: 131 ff.) – to a more 'physicalist' conception of SAFs as 'force-fields' that constrain and compel actors in quite determinate ways?*

NF: I appreciate you were convinced by our argument in the Methods chapter. I think these kinds of disputes are what causes us to build walls between groups of scholars instead of appreciating the differences and trying to see if there are deeper connections to what we are doing. I have encountered people who believe that just because Bourdieu proposed correspondence analysis as the only way to model fields, correspondence analysis must be the only way to model fields (see Bourdieu and Wacquant 1992: 96). We are still not ready for a coherent social physics whereby the model is reality. Instead, there are families of statistical models that do similar things and choosing between them as ways to portray fields is hard to do. To say that there is only one statistical model to get at relations between actors is well, difficult to believe. I am glad you accept our argument. My view is that sometimes fields can act in a force field way, particularly ones that are powerful and well established. An individual or a group entering an existing social field will be hard pressed to deny the power of the already existing order. But, I still see fields as resolutely about actors, positions, conflict, different interpretations, and always moving from being more or less organized. So, I accept some of that view, but I think that there is always lots going on in every field.

DV: *In your book you outline several of the factors that prevent contemporary sociology from developing a more unified interpretative framework, but also indicate how field theory might provide a way of bridging disciplinary and subdisciplinary divisions. Could you elaborate on that?*

NF: I have elaborated earlier my views on this. Field theory is a very general discovery across the social sciences. As such, it has the possibility of creating a very general sociology. But I am a realist. I think scholars themselves live in fields and those fields are defined by incumbent-challenger structures. So, for example, economics is at the top of social sciences and I see no reason why they will want to create such a theory that is not entirely under their control. Within sociology, we have built up similar kinds of camps. First, we have subdivided the field to lessen competition between groups of scholars. If I am in the 'theory' camp and I believe that theory is about writing about other theorists, then I can conveniently ignore people who call themselves theorists but are working on empirical

problems. Within subfields, we build up such structures as well. Scholars define who they are in terms of who they perceive as the opposition. This helps them create research programs, build careers and gain honors. The puzzle for me is given that our entire reward system is built on this kind of field structure, how can a general theory emerge, one that requires scholars from different parts of the field to see one another in a collective activity? The pessimist in me sees this as a daunting problem and thinks nothing will ever change. Indeed, the current structuring of American sociology is working to create more and more subfields and less and less ability to hold discussions across large numbers of scholars. The optimist realizes that in spite of this, field theory has emerged and gets used for a wide variety of empirical problems (either consciously or unconsciously as many people who actually have a field perspective either do not recognize it or are in denial about it). This suggests scholars do want to be part of something bigger and more important. So, according to the theory, if a set of socially skilled actors could offer an identity to enough people that they would find resonates with their very being by offering them the 'pie in the sky' of a unitary theory of meso-level social order that will unite us not divide us, we can create a social movement and transcend all of this. Hey, I can dream can't I?

DV: *You sure can! Prof. Fligstein, thank you very much for this interview.*

Notes

1: This paper was originally published in *Sociologos: Tijdschrift voor Sociologie*, 2014, vol. 35, Issue 1-2: 79-105. Reproduced with permission from *Sociologos*.

2: For Fligstein and McAdam, the internal dynamics of fields are inherently characterized by the relationships between 'incumbents', those who determine the rules of the game and control the structure of the field, and the 'challengers', those who occupy dominated positions and often wield alternative visions of how the field ought to function (see for instance 2012: 13 ff.).

3: Bourdieu repeatedly stresses that he developed the concept of a 'field' as a means of accentuating the relative autonomy of the social microcosms within which symbolic goods (books, paintings, scientific texts, etc.) develop and this against the so-called 'short-circuit fallacy'. The latter attempts to 'explain' the logic of symbolic goods by linking their production to the global structure of a society, be it in the form of an economic infrastructure or a cultural *Zeitgeist* (see Bourdieu and Wacquant 1992; Bourdieu 1993: 176 ff.; Bourdieu 2000: 17 ff.). Abbott (1988) also devised the concept of 'ecology' to stress the internal dynamics of inter-professional competition and refined this further in his concept of 'linked ecologies' (Abbott 2005) that aims to specify the interdependencies between ecologies. Finally, Luhmann's (1995) concept of 'self-reference' can also be understood as a means of accentuating the autonomous logic of social (sub)systems.

4: It should be mentioned that Bourdieu is rather explicit in his rejection of the oeuvre of Althusser – especially his conception of the state and 'ideological state

apparatuses' – which he brands as a typical example of 'pessimistic functionalism' (see Bourdieu & Wacquant 1992: 102).

5: The 'Glass-Steagall Act' is a series of legal provisions that are part of the more general 'Banking Act' of 1933. This law was part of a series of economic measures undertaken by Franklin D. Roosevelt in response to the Great Depression. One of its stipulations was that banks had to choose whether they wanted to act as commercial or investment banks. This effectively prohibited capital from private loaners to be used for purposes of financial speculation. Under pressure of financial lobbyists (supported by Citibank and Bank of America, amongst others) and with the support of the then Secretary of Finance, Robert Rubin, the Act was repealed by the Clinton administration in 1999.

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