

From a sociology of professions to a sociology of expertise

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Abstract

This paper argues that the time has come to replace the sociology of the professions with the more comprehensive and timely project of a sociology of expertise. Scope: “expertise” indicates a field of research at once wider than the sociology of professions, yet more limited and precisely circumscribed than the sociology of knowledge. Mode of analysis: while the sociology of professions typically reduced expertise to the interests and world-views of the experts, the sociology of expertise maintains an analytical distinction between experts and expertise as two irreducible modes of analysis, treating expertise neither as an attribution, nor as a set of skills possessed by an individual or a group, but as a network connecting together actors, instruments, statements and institutional arrangements. Knowledge: instead of debating whether expertise consists of abstract rules or practical/tacit knowledge, the sociology of expertise investigates the specific chains of abstraction characteristic of different forms of expertise, and the practical links of which these are made. Power: while the sociology of the professions understood power as consisting of control over the supply of experts and over demand for their services – typically taking the form of effective monopoly over esoteric knowledge – the sociology of expertise analyzes power as a complex interplay between monopoly and generosity. Topology: networks of expertise traverse the boundaries posited by the sociology of the professions between jurisdictions, fields or “ecologies”. The topology they require is of intermediate and relatively indeterminate spaces of overlap, interfaces, “trading zones”, etc. We illustrate these distinctions using examples taken from past research in military intelligence expertise and ongoing research on the emerging expertise of political risk analysis.

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The sociology of the professions has reached its end. There are conceptual as well as empirical/historical reasons for this demise. Conceptually, the sociology of the professions privileged the analysis of organizational and institutional *form* - credentialing, licensing, the formation of an association, a university department, etc – over the *content* of what the experts actually do. It operated, therefore, with a restrictive binary concept – profession/not profession – and an essentially teleological narrative of “professionalization”.¹ Historically, the heyday of professionalization has been already almost a century ago, and was followed by increased skepticism and resistance towards established professions from more minor professions, from social movements and lay expertise. (Epstein 1995; Wynne 1996)

We suggest, therefore, replacing and subsuming the sociology of the professions within a broader, timelier and conceptually more flexible sociology of expertise. Table 1 summarizes the differences to be discussed below:

¹ This critique was formulated by Abbott (1988). Despite its title - *The System of Professions* - a close reading of Abbott’s book reveals that it ends the sociology of professions and opens onto sociology of expertise. It should be seen as the definitive masterpiece of the sociology of professions, but also its logical conclusion. The book, in fact, is neither about a system nor about professions, and the subtitle – *An Essay on the Division of Expert Labor* – is a lot more accurate in describing what it does. I will refer to some of the key concepts employed by Abbott in the course of the paper, because they sum up the accumulated wisdom of the sociology of professions, but also remain constitutionally impaired by its limitations.

Table 1: Dimensions of the contrast between the sociology of expertise and the sociology of professions

	Sociology of professions	Sociology of expertise
Scope	Limited to professions and would-be professions	Inclusive of all who can make viable claims to expertise
Mode of analysis	Expertise reducible to the experts' interests and world-views	Experts and expertise distinguished as two different modes of analysis
What is privileged?	Organizational and institutional <u>form</u> : Credentialing, licensing, association, etc.	What experts actually do. How they put together a statement or performance that withstands trials.
Abstraction	The most distinctive characteristic of professions is their possession of esoteric, abstract, de-contextualized knowledge.	Abstraction is just shorthand for a chain of practical transcriptions. Different forms of expertise abstract differently, because their chains are different.
Account of power	Monopoly	Generosity
Where is expertise?	Fields, jurisdictions	Spaces between fields
What is expertise?	Attribution, a formal quality reducible to actors' interests	A network of connecting together actors, instruments, statements and institutional arrangements.

Scope

It is instructive to begin with a little bit of etymology. Expertise is a third-order transformation of the Latin root for *expertus*, “to try”. First the verb was transformed into an adjective, so “expert” was something like “tried”, “experienced”. The second transformation turned the adjective into a person noun, i.e. individuals who were consistently capable of expert performances were “experts”. Thus transformed, “expert” was yet hardly distinguishable from “experienced”. Finally came the third transformation when the person noun was transmuted into a substance noun – expertise – which is something that the experts possess, perhaps by virtue of their

experience, perhaps by virtue of something else. (Williams 1976) The Oxford dictionary concurs, defining expertise as “an authority by reason of special skill, training or knowledge” as well as “A person who has gained skill...from experience.” In short, the third transformation introduced a degree of freedom between the substance noun and the original meaning of *expertus*. Expertise could be due to experience, but also due to specialized education or what have you.

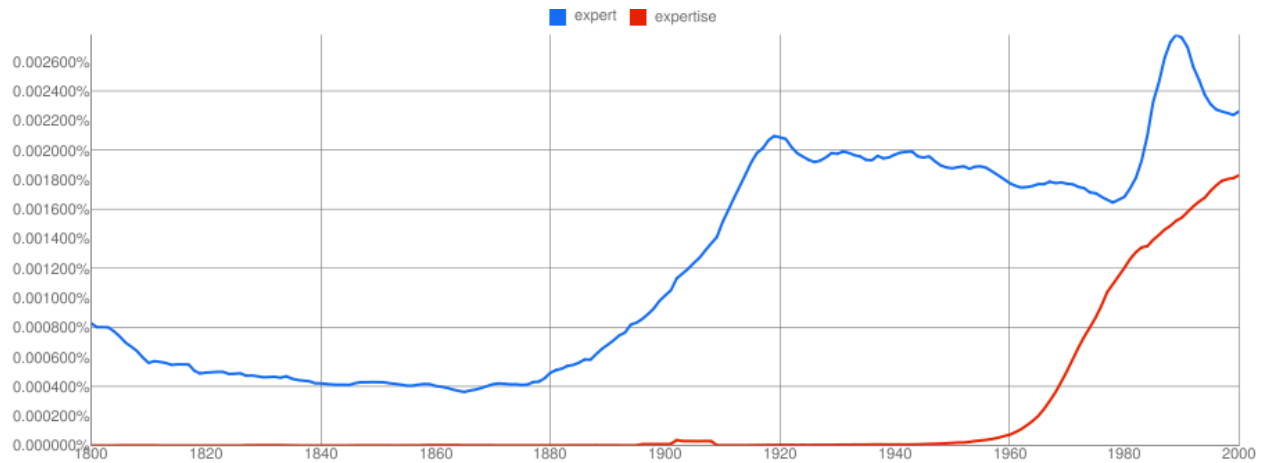
Williams (1976) traced this transformation to the rise of the division of labor and specialization in the 19th century. Even if he is right, the substance noun has lain fallow for a long time. As you can see in Figure 1 below, Google Labs’ *Culturomics* records a very steep rise (500%) in the use of the person noun, “expert”, between 1880-1920, corresponding, if we are generous, to Williams’ chronology, but the substance noun, “expertise”, only takes-off by 1960 with a similarly steep curve that does not seem to have ended yet.² We were told by Robert Crease, editor of a book on the philosophy of expertise currently being translated to Mandarin Chinese, that Mandarin Chinese has no word yet for “expertise”, though there are equivalents for “expert” and for “skill”.

This historical and cultural specificity of expertise makes us think that the key to the third transformation is simple: as long as it is very clear who the experts are, and how to recognize them, you do not need a word for “expertise”, let alone a mode of analysis for it. Once the number of contenders for expert status has increased and the basis for their claims have become more heterogeneous; once the struggles between these would-be experts intensify; expertise becomes problematized and the word is bandied about, because the question is how to determine whose claim is legitimate. This is what Collins and Evans (2007, 2) call “the problem of extension”, i.e. how far could be extended the right to participate in public debate about technical matters? It signals that the scope of the phenomenon has changed, and that a new approach is needed.

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http://ngrams.googlelabs.com/graph?content=expert%2C+expertise&year_start=1800&year_end=2000&corpus=0&smoothing=3 (last accessed 5/25/2011)

Figure 1: Frequency of appearance of “Expertise” and “Expert” in Google Books from 1800 to 2000



The history of expert testimony in American law is especially instructive in this regard. Judges have wrestled with how to decide whether expert testimony is credible and pertinent at least since the late 18th century. In the early 20th century, the *Frye* ruling codified a particular approach to this problem which asked whether the witness or the method enjoyed “general acceptance in the particular field” considered. (Edmond and Mercer 2004; Golan 2004) Despite the vagueness of this formula, it raised no problems for several decades precisely because the job of vetting the experts was essentially delegated to the various professional associations (Golan 2004, 257). The *Frye* consensus began to fray, however, in the 1970s with an explosion of mass tort litigation organized around a new object of expert knowledge – risk. The assessment of risk is always an interdisciplinary matter and requires the mobilization of different experts and forms of expertise. Risk, moreover, was often produced by science or technology itself, and therefore almost universally pitted one group of experts against another. In the early 1990s, the *Frye* standard was finally replaced by the *Daubert* ruling, which among other things empowers judges to look beyond “general acceptability” and to determine for themselves whether the methods and knowledge claimed by expert witnesses stand up to scientific standards of falsifiability, etc. (Jassanof 1995) In short, from relying on the professions to vet the *experts*, the law has burdened judges with the fairly impracticable task of evaluating *expertise*.

Hence our point about scope. The sociology of professions reflects an earlier status-quo, perhaps of “organized modernity” (Wagner 1994), when meaningful participation not only in courtroom performances of expertise, but also in the policy arena, the public sphere, etc., was restricted to those experts dully recognized as members of legitimate professions. The sociology of expertise,

by contrast, permits to take in view the much larger space opened up as this status-quo crumbled. “Expertise” refers to a phenomenon that is more widespread than the professions, yet more precise and circumscribed than knowledge *per se*. It involves a distinction between lay and expert, i.e. not everybody can be an expert, and not any form of knowledge qualifies as expertise, but at the same time, the term expertise does not prejudge where such boundaries lie. It even permits the oxymoron of “lay expertise” (Epstein 1995; 1996), which is impossible to construct with “profession”. We consider this, therefore, an advantage over the sociology of the professions, which always involved a difficulty about whether a group was “really” a profession or not. It permits to analyze forms of expertise that are put together as collaborations among differently trained individuals, including also clients, patients and other lay individuals, and that compete with similarly hybrid forms of expertise.

Take the case of political risk analysis as an example, and by the same token also of the multiple forms of hybrid expertise that populate the fuzzy in-between space of security and risk management. We will explain below why we think it is fruitful to consider both security and risk within the same frame of analysis, without collapsing one into the other. The origins of political risk assessment were in intelligence, especially Cold War collaborations between American intelligence agencies, American academics and think tanks. One of the first pioneers was Stefan H. Robock at Columbia School of Business, a navy intelligence officer turned economist.³ Yet, to become political risk analysis it had to undergo a detour through the academic disciplines of political science and economics, and undergo successive attempts to transform it into a marketable business tool that could be exported outside of academia. These attempts were strongly supported by think tanks such as the Conference Board, which funded one of the first studies of how corporations deal with political risk (Kobrin et al 1980), and they eventually led to the formation of several commercial ventures or consultancies, some of which are by now 30 years old. Some of these ventures then turned back and supplied services to state agencies such as the CIA, the Intelligence and Research Bureau of the Department of State, and the Army of Corps of Engineers. So political risk analysis, even as it is packaged and sold as a standalone product by a commercial venture, in fact requires inputs from several distinct institutional milieus: state intelligence agencies, academia, think tanks and commercial firms. Even though a professional association of political risk analysts was formed early, it seems to be dormant, while the personnel involved in political risk analysis hail from diverse backgrounds and disciplines – some have intelligence background, many have degrees in economics, political science, business, statistics – and they circulate rather promiscuously between institutional milieus. To submit political risk analysis to an analysis that focuses on professionalization and asks who has jurisdiction over it, we suspect, would yield very little insight. On the contrary, the more political risk analysis becomes a reality, especially as a viable commercial product, the more it becomes a hybrid of different institutional and professional inputs.

³ “AIB - Fellow Bio: Stefan H. Robock,” <http://aib.msu.edu/fellow.asp?FellowID=48>.

Mode of analysis

If one advantage of the sociology of expertise is that it permits to enlarge the scope of investigation beyond the narrow one prescribed by the sociology of professions, another is that it encourages a distinction between experts and expertise as two different modes of analysis. For reasons that reach back to American sociology's response to Mannheim's sociology of knowledge, the sociology of the professions has been primarily a sociology of "men of knowledge" (Znaniecki 1940), i.e. of experts, and has treated expertise as largely reducible to their interests, role sets and modes of organization. The sociology of expertise, on the other hand, begins from the recognition, as Nikolas Rose (1992, 356) puts it, that "the social consequences of psychology [read expertise] are not the same as the social consequences of psychologists [read experts]."

How was expertise reducible to the experts in the sociology of professions? It was treated chiefly as an *attribution*, a quality that the experts possessed by virtue of recognition granted by significant others. (Collins and Evans 2007, 2) Hence the focus of the sociology of professions on organizational *form*: credentialing, licensing, the formation of professional associations and lobbying outfits are all calculated to secure the recognition of significant others and enforce it with a legal mandate if necessary. Insightful as this approach is, it provides no tools with which to approach "the problem of extension," no way of deciding between competing expertise claims (ibid), and most importantly it has very little to say about what experts *actually do*. Sociology of expertise, by comparison, must pay attention to what experts actually do, how they render problems visible and actionable; how they put together a statement or performance that withstands trials and critical scrutiny. (Latour 1987; Boltanski and Thevenot 2006) This means that it has to distinguish two complementary modes of analysis, one proper to experts, the other to expertise.

What should be the mode of analysis proper to expertise? The obvious alternative to attribution is to treat expertise as a real and substantive skill possessed by an individual, typically by virtue of being socialized into a group of similar experts (Collins and Evans 2002; 2007) and consisting primarily of embodied and intuitive mastery rather than rule-following (Dreyfus and Dreyfus 2005). This is certainly close to the everyday colloquial meaning of the word, but for this reason also seems to us to throw the (relational) baby with the (attributional) bathwater. In everyday language, expertise is a term which immediately implies hierarchy and the attribution of worth. Models of expertise as a skill possessed by an individual or group inevitably participate in this assignment of worth – witness Dreyfus' careful distinction of true expertise from mere proficiency or competence. One may, like Collins and Evans (2007), embrace this normative role

of adjudicating competing claims to expertise. And in a world of vaccine scares and global warming controversies there is certainly a need for someone to play this role. But we have strong doubts that they can persist in playing this role very long before they themselves become embroiled in a controversy about their own expertise and credentials. What they are seeking to adjudicate are, in Latour's (1987) terms, "trials of attribution", tests of strength. Where is to be found a neutral language and mode of analysis that would adjudicate this and not become infected over time with the very indeterminacy and essentially conflictual nature of what it seeks to adjudicate? It has been tried before, and never successfully. Just imagine the sociologist of expertise pulled into the court as an expert witness or even a special adviser to the judge (as is periodically suggested by advocates of "science courts" in the US – and periodically rejected...) on how to decide whose expertise to trust. If not torn to shreds by this or that side's lawyers, he or she would no doubt invite scathing criticism from the experts themselves. So we are skeptical about Collins and Evans' project. In what follows, we gradually outline an alternative approach to expertise, which seeks to avoid the pitfalls of both attributionalist and substantivist approaches.

Abstraction

There is another reason why we are skeptical about the normative role claimed by Collins and Evans for their "studies of expertise and experience". The fact remains that however neutral they wish to appear, their model of expertise inevitably is biased in a particular way, marked by a second context, a second provocation to which the sociology of expertise arises in response (the first was the increased contestation of science and the professions by social movements, lay experts, marginal forms of expertise, etc.), namely the challenge posed by "expert systems" and "artificial intelligence."

Despite their differences, Collins and Dreyfus both understand expertise as primarily contextual, practical know-how, a matter of "tacit knowledge," embodied or at least embedded in a particular "form of life." Expertise, they say, is exactly the opposite of the abstraction required in order to codify explicit and a-contextual rules that a machine could execute. So their very definition of expertise seems calculated to exclude expert systems, rigged from the start in how it assigns worth to competing performances. There is good sociological reason for that since their emerging knowledge science is in competition with those branches of artificial intelligence and computer science who study complex forms of human expertise in order to replace them with algorithms. Yet a robust sociology of expertise must be symmetrical. It cannot exclude out of hand the actual achievements and considerable successes of expert systems but must, on the contrary, explain it just as it explains human experts.

Significantly, the “attributionalists” of the sociology of professions (and one can include Bourdieu, as well, in this camp) have no problem doing the latter because they opted for the opposite solution, namely abstraction. It would suffice to consider Abbott (1988) here. What characterizes professions, he says, is the abstract quality of their knowledge. While crafts compete by controlling technique, professions compete by controlling the abstractions that generate practical techniques. “Only a knowledge system governed by abstractions can redefine its problems and tasks, defend them from interlopers and seize new problems” (8-9). Precisely for this reason he is able to view the rise of expert systems quite differently from Dreyfus, as embodying a serious challenge to the professions dealing with information. So a parallel exclusion: while the substantivists exclude expert systems, the attributionalists exclude the craft-like occupations. Of course, Abbott is smart enough to note, halfway through the book, that in fact the relation between abstraction and the control a profession is able to exercise over its jurisdiction is an inverted U-curve. Too little or too much abstraction spells out weakness and invites attack. Very insightful, but it ultimately means that the concept of abstraction is empty. It can do anything you want it to do. How do we know the optimal level of abstraction unless by hindsight? As a recommendation for research this is *carte blanche* to find whatever you wish to find.

It would seem that if we learn something from this double and mirrored impasse, it is that expertise consists neither in practical mastery *per se* nor in the abstractions that codify it *per se*. The opposition gets us nowhere. To insist on the role of practical, tacit knowledge was a very important move in the era when sociology of science still celebrated scientific thought as unique and completely detached from everyday thought; by the same token, Abbott’s distinction between professions and crafts is very insightful. It seems that two things need to be done in order to connect these insights into a more fruitful synthesis. First, we need to unpack abstraction. Abbott’s formulation is too magical. Abstraction seems like a theorem that “begets” a practice. Instead, we should think about abstraction as an unbroken chain of transcriptions, each of which consists of altogether practical devices and forms of reasoning. (Latour 1999) Abstraction, therefore, would refer to the full set of actors, tools, machines and transcription devices necessary to convey a statement up or down this chain. From this point of view, (optimal) abstraction is just a shorthand for a network of concrete and practical arrangements that has been successfully (though provisionally) black-boxed. It is telling that while Collins and Evans insist on the non-relational meaning of expertise when dealing with human actors, when it comes to computers Collins (1990) actually has a very insightful and relational approach. Seeking a middle position in the debate about artificial intelligence, he argues that computers can be competent “artificial experts” provided that humans continuously digitize their input and repair their output, two non-trivial tasks that involve various other characters (programmers, debuggers, data entry workers, end-users, measurement devices) and all sorts of practical, tacit rules-of-thumb. So where is expertise? Who is the author of expert judgments? Is it the

computer, the programmer or the end-user? Clearly, as the term “system” implies, expertise is a property of the network composed by all three. Why deny, therefore, to human actors what one affirms about non-human actors? They too would be helpless without their tools and assistants.

The second point is that we need to recognize that there are different types of abstraction. For convenience sake, we will use Latour’s (2010) contrast between scientific and legal abstraction, but clearly “science” is too imprecise, and different forms of expertise – some scientific, some not – all may abstract differently, precisely because their chains of transcriptions are organized differently. To make a legal statement, one needs to abstract from the case at hand, but in a way that is very different from scientific abstraction. Scientific abstraction, what Latour (1999) called “circulating reference”, is always traceable back, the chain of transcriptions being fully two-way road, while legal abstraction intentionally cuts off the possibility of return as a condition of making a legal statement. (Latour 2010, 235, 264-265) Legal abstraction is a one-way road because law has to render a decision, while laboratory science doesn’t. The two-way nature of scientific abstraction corresponds to the temporality of the laboratory, where time could be rolled back, and whatever emerges as fact is “open forward” so to speak. Legal temporality is different. Its abstractions are “flat”, so to speak, connecting legal statements with other legal statements, and systematically erasing the details from which these emerged, because it is oriented to the supreme value of legal stability. To quote Latour: “so that nothing is lost and nothing is created, everything that inexorably passes by – time, humans, places, goods, decisions – remains attached by a continuous thread, so that legal stability serves as a net for all potential applicants, and humans may live in the house of law...” (277) It follows that “regulatory science” is somewhere between these two forms of abstraction, since like law it must render a decision (about “acceptable risk”), yet it must keep its chain of transcriptions as reversible as possible (by means of double blind controlled studies), hence its hybrid nature and many challenges.

To return to the case of political risk analysis: it probably represents yet another mode of abstraction different from science, regulatory science and law. Its chain of transcriptions is neither fully reversible nor fully flat. It aims to arrive not at a scientific fact “open forward”, nor at a decision, but at a “rating”. To illustrate how it works, we use the case of The PRS (Political Risk Services) Group originally founded in 1979 by Frost & Sullivan (itself a business consulting firm that trains executives) as World Political Risk Forecasting (WPRF). The core product is the Coplin-O’Leary Country Risk Rating System™ which is an analytic method “developed by Professors William D. Coplin and Michael K. O’Leary during 20 years of research at the Maxwell School of Citizenship & Public Affairs, Syracuse University.” The WPRF system is essentially a survey administered to country specialists, who are asked to assign probabilities of risk for investors in two stages, first identifying the three most likely future regime scenarios for each country over two time periods (18 months and five years) and

assigning a probability to each scenario over each time period. In the second stage, the specialists are then asked to identify likely changes in the level of political turmoil for each regime scenario and 11 types of government intervention that affect the business climate. A numerical consolidated score is then calculated for all regimes, pooling together all scenarios and all experts' ratings. The score is then converted to a letter grade (on a scale from A+ to D-), no doubt in order to resemble credit scoring agencies.⁴ The chain of transcriptions clearly is not fully reversible. If you ask other experts, or if you ask the same experts another time, you are likely to get different answers. In fact, the summing up of the reports into a score and then a letter grade is meant indeed to cut off the possibility or need to return to the specific "local" knowledge of the expert (itself already the product of prodigious abstraction and de-contextualization). Each expert and expert assessment singly are "subjective," but their aggregation is taken to produce an "objective" ranking. Moreover, risk ratings bear a similarity to the "flat" network of legal statements referring to one another, because the ratings are meant to be used by the actors as a basis for their decisions and to the extent that all actors are using similar ratings, they have a performative, self-fulfilling quality. It is no longer as important whether the country in question is "really" risky or not, because all relevant actors are treating it as such, it is consequently investment starved, and must resort to other avenues – corruption, providing costly political risk insurance for every deal – that render it even riskier in the eyes of most investors.

Power

This approach to abstraction is at once "substantive", in the sense that one is analyzing what the experts actually do rather than attributions, but it is also "relational", because expertise resides neither in the experts themselves, nor in impersonal systems of rules, but in practical chains of transcription that involve many steps and many participants. What are the implications of this approach to the analysis of the power and influence of experts and expertise, and how does it differ in this regard from the sociology of professions? The best place to start an answer is the concept of "jurisdiction" employed by Abbott (1988), a concept that neatly summarizes a great deal of the received wisdom of the sociology of professions over at least three decades. Each profession or group of experts, says Abbott, seeks to gain jurisdiction, i.e. control over actual work processes. Unlike craft-type groups, professions seek this control by means of abstraction. Medicine is usually used as the quintessential example, since in matters pertaining to health medical doctors control their own work processes and of those subordinate to them, and while this control is protected by legal mandate, its ultimate justification and bulwark are the

⁴ http://www.prsgroup.com/PRS_Methodology.aspx (last accessed 5/30/2011). We should note that the Coplin-O'Leary model has been published by the Syracuse Research Corporation, which is itself a collaboration between a government agency and a university institute. See also Brewer (1985, p170).

abstraction produced by medical science. Abbott is well aware, however, that most groups of experts do not enjoy anything remotely similar to medical dominance (Friedson 1970), and that even medical doctors themselves have to cooperate with other groups of experts (nurses, technicians, administrators) to do almost anything. He suggests, therefore, 5 different “settlements” in which jurisdiction is shared: subordination, division of labor, intellectual jurisdiction, advisory jurisdiction and client-based division. These are useful empirical types, but theoretically speaking they are ad-hoc. Take “advisory jurisdiction” as the most glaringly obvious example. Jurisdiction means control of actual work, right? Yet, everybody knows that “advisory” is a qualifier that says you are *not* going to be given control. The qualifier, in effect, destroys the original meaning of “jurisdiction” and exposes a constitutional lacuna in the sociology of professions. Essentially, “jurisdiction” is a concept that reduces expertise to the experts. For the actual work to be performed, the collaboration – however hierarchically organized - of multiple experts is needed. If one is interested in explaining how a form of expertise is put together - how is it possible for expert performances and statements to be produced, reproduced, circulated and withstand trials - it is less useful to ask who has jurisdiction.

The problem can be summarized as follows: the sociology of the professions has equated power with the capacity of a profession to maintain a monopoly over its knowledge – whether abstract or practical, it must be esoteric, restricted to the circle of initiated. It analyzed professions as corporate groups organized to enforce and defend this monopoly. Friedson’s (1970) “dominance model,” which theorized medical power, is the canonical example. From this point of view, all phenomena of knowledge-sharing, cooperation, cooptation, lay expertise, co-production of objects, are deeply problematic. They are anomalies, yet they are in fact pervasive. We would like to suggest, therefore, that while power-as-monopoly may be a useful lens to analyze the interests of *experts*, when it comes to *expertise*, the opposite lens of power-as-generosity (Rose 1992) is more relevant.

By “generosity” Rose (1992) means that a form of expertise, as distinct from the experts, can become more powerful and influential by virtue of its capacity to craft and package its concepts, its discourse, its modes of seeing, doing and judging, so they could be grafted onto what others – whether authorities, experts or the lay – are doing. Generosity is thus the opposite of monopoly and it does not lend itself to be analyzed in terms of “jurisdiction.” The experts – like the psychologists Rose analyzes, or like political risk analysts – may gain no jurisdiction, but the form of expertise spreads *across* jurisdictions. To call this “advisory jurisdiction” is merely to sweep the conceptual problem under the empirical rug. An adviser can be influential, but has no jurisdiction. Often, advisers *are* influential – as Eyal (2006) found in his research on military intelligence – precisely by virtue of their capacity to erase themselves altogether so that not only

they have no jurisdiction, but their advice is no longer attributable to them. The advisee of his own accord now sees, does and judges precisely as the expert would.

Put differently, even if we think only about the power and influence of the *experts*, it is more often due to a judicious balance between monopoly and generosity. Fully monopolized, fully esoteric knowledge is likely to be irrelevant to the needs of clients, and is likely to be viewed with suspicion. Hence, for political risk analysis to be successful, the pioneers of the field had first to teach firms and managers to view their problems as one of “political risk”, to teach them that this language and mode of seeing were relevant to them. By the same token, the methodology of the Coplin-O’Leary Country Risk Rating System™ is trademarked but not patented or shrouded with industrial secrecy. The questions used in their questionnaires, the rating methods, the presentation methods, the way they screen their “country specialists,” have all been published. They even provide a worksheet! Yet, they do not reveal the identity of their experts. Monopoly and generosity are balanced. The network of experts seems to be the “proprietary” monopoly of The PRS Group, but this is indeed more like a trademark guaranteeing quality.

Finally, since it applies the lens of generosity to the experts it studies, the sociology of expertise itself should be judged by similar criteria. Here is a good empirical test for the distinction between sociology of professions and sociology of expertise: does the sociologist have something to say *to* the experts, about what they do, that can be useful to them, that can be grafted onto their own practices, and that will not be immediately perceived as an accusation? Can sociological expertise be generous as well? The sociologist of the professions will find it difficult to speak to experts. Nobody wants to hear that their actions are explained by ulterior motives, by the quest for monopoly, and they likely will turn the accusation back at the sociologist. Experts typically are bored by the things that interest the sociologist of the professions – organizations, licensing, credentials, etc. They consider these incidental to what they do and are insulted or bemused that we are not interested in the real stuff, in the *content* of what they do. The sociologist of expertise, on the other hand, has a better chance of engaging in dialogue with the experts, of all kinds. To the extent that dialogue encourages self-reflection, the sociology of expertise is also critical, though in a way that is continuous with the reflexive and critical resources that are at the disposal of the experts it studies.

Where is expertise?

Abbott (1988) employs the concept of “jurisdiction” precisely in order to convey the image of a well-defined boundary surrounding the work of experts. Medicine has its own jurisdiction, and

so do social work or library science. Even as they coexist, compete and may be forced to share a jurisdiction, the interest of professions is to draw a clear boundary around themselves and their tasks. Bourdieu, as well, locates the work of experts within nested “fields” – juridical field, scientific field, etc. From this point of view there would be indeed a field of security expertise, and perhaps a separate field of risk management, or one may view them as different jurisdictions or “task areas”.

We think this is too simplistic. Bounded concepts are useful for a sociology of action, for explaining the interests and orientations of the *experts*. Jurisdiction refers to the sphere of competence and authority claimed by the experts. A field is often useful in describing the universe of significant others that shapes a particular *illusio*, and thus can resolve the false antinomy between externalist and internalist explanations of action. (Bourdieu 1985; 2000) But when the problem is explaining how a form of *expertise* is put together, how expert statements and performances are produced and circulated, these concepts become a hindrance to thought. The pioneers and entrepreneurs of political risk analysis crossed several jurisdictional or institutional boundaries in order to put it together. It is composed of the contributions of academic experts, think tank *modus operandi*, crucial inputs from state agencies, and a tight link with commercial consultancies. Political risk analysis is neither strictly in the marketplace, academia, the field of think tanks or the state, but in a space somewhere in between them.

For the same reasons we do not think it is useful to sharply distinguish political risk analysis from security expertise. They should be grasped as coexisting in a common in-between space of opportunity and mediation. No doubt Political risk analysis is used for making business investments, and it has strong ties to other techniques of risk management which have nothing to do with security (e.g. credit ratings), yet at the same time, as we noted earlier, the Coplin and O’Leary system is used also by government and military agencies such as the Intelligence and Research Bureau of the Department of State, the Army of Corps of Engineers, and CIA. This seems to confirm Aradau et al’s (2008) argument that security is increasingly understood against a background of risks as well as threats/dangers, or some hybridization of the two. An obvious example of such hybridization is profiling, a technique of *qualitative* risk analysis used widely by security organizations. Similarly, what is the Copin and O’Leary system if not a mechanism for converting experts’ qualitative judgments of danger into quantitative assessments of risk? And there are other such mechanisms. In short, threat/danger and risk should be considered as two poles of a continuum along which all sorts of combinations are possible. On the one hand, the inclusion of risk within security expertise entails a multiplication, a larger field of relevance for security expertise: “pandemics to organized crime, global warming, failed states, terrorism, poverty and nuclear proliferation.” (Aradau et al 2008, 147) On the other hand, risk analysis often draws upon intelligence reports, i.e. upon qualitative evaluations of danger/threat, to calculate, assess and manage risks, as we’ve seen with the Coplin-O’Leary system.

Put differently, this in-between space is populated by multiple practices or forms of expertise for handling the unknown. Brian Wynne (1992) suggested 4 types of unknowns: *risk* is a future danger which can be statistically calculated; *uncertainty* is when it is impossible to calculate the probability of a known potential danger; *ignorance* is about “unknown unknowns” (to quote Rumsfeld, but one should note that ignorance itself could be socially constructed and produced, i.e. that something is unknown is not necessarily a given [Funkenstein and Steinsalz 1987]); finally, *indeterminacy* is about unknowable unknowns, where there is no predictable pattern to things. (Arnoldi 2009, 91) This typology could be extended. While a great deal of the risk literature has concentrated on how uncertainties may be converted, via calculation, into risks, thereby leaving the two terms intact and heterogeneous, we note that in between the two there is a whole family of forms of expertise that involve non-numerical ways of working with probability and uncertainty. One example we already mentioned – profiling. Another is the legal theory of proof, employing a standard such as “beyond reasonable doubt”. (Arnoldi 2009, 30) Another more pertinent example is the style in which intelligence officers compose their assessments. Often, as a matter of policy, they are required to attach degrees of likelihood to their assessments. The result is typically to rank order threats by their degree of certainty, i.e. neither quantifying them as risks nor leaving them as unconverted uncertainties. For these and other reasons, we think that the distinction between danger/threat and risk is not as clear-cut as Aradau et al (2008, 148) present it. One could argue, in Schmittian fashion, that security ultimately points at a situation of indeterminacy, because the universe of friend/enemy relations is ultimately pattern-less, but even this extreme rendition of security expertise aligns it with calculative practices that are meant to increase preparedness and bolster the resilience of a system to external shocks: scenarios and simulations, stockpiling, metrics for readiness assessment, etc. (Lakoff 2007) 4)

Security expertise, therefore, takes place in a “space between fields”. There are two layers of meaning to this concept of space between fields, two analytical and conceptual tasks it can perform in the service of sociology of expertise. The first one follows immediately from the considerations developed above. The practices we describe for handling unknowns straddle multiple fields. They stretch between academia, the marketplace, the state and the field of think-tanks. Actors can be represented as moving from one arena to the other, but the form of expertise itself, the modality of activities, for example how a risk score is put together, links all these different arenas and takes place as it were between them. Put differently, instead of imagining fields as strictly bounded spaces, we suggest thinking of the boundary as a thick zone of interface and overlap (Eyal 2006), a fuzzy frontier or “trading zone” (Gallison 1997) that both separates and connects fields.

Let us give an example, taken from prior research on the relations between military intelligence and Middle Eastern studies in Israel. 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; Toledano 1989) What happens, therefore, to the distinction between the academic field and the state? 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. We 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 us as part of the object of study, but not as reliable ways to conceptualize it. The important point, however, is not so much that the experts themselves circulate between the two spheres. From that angle, we have no doubt that the notion of boundary is still useful. The important point is that their enunciations, whether military intelligence assessments, academic articles or media commentary, share a series of features – for example, they cite materials collected by military intelligence, or they seek to decipher the "intentions" of the leaders of a neighboring country, or they assign degrees of likelihood to their assessments - and work to reinforce one another. The form of expertise, therefore, is neither academic nor military, but must be seen as native to the interface between the two.

The second analytical meaning of the idea of "space between fields" is as a space of opportunity, an underdetermined space where one can do things that are not possible within more established fields, to combine things that normally are kept apart, such as military intelligence and risk management perhaps? We intend this idea as corrective to field theory, but it is possible, of course, to consider it as a merely temporary phenomenon, as a field-in-the-making, whether ultimately successful or abortive. This is how, for example, Bourdieu (1996, 51-53) analyzed the functioning of the Salons of mid-19th century French High Society Ladies. He characterizes them as "bastard institutions" that served as "genuine articulations between the political and artistic fields." Within their perimeter it was possible for writers and politicians to rub shoulders and to exploit proximities that that were not possible elsewhere. The politicians, typically second tier, could acquire powers of influence that were not available to them in the political sphere; the

writers could intercede and act as pressure group for material or symbolic rewards. Out of these exchanges and “shady deals,” says Bourdieu, the structures and oppositions of a nascent literary field began to emerge.

Interstitiality plays a similar role in Medvetz’s (2007) analysis of the field of think-tanks in America. He shows that think tanks inhabit a space in-between academia, politics, business and the media. Think tank personnel are recruited from these four realms and this leads to competition between various forms of expertise within and between think tanks. Finally, think tanks are parasitical on these other fields in the sense that their output is strongly shaped by the need to respond to demand in the political and media fields. Yet, in Medvetz’s analysis, none of these features are permanent. While dependent on inputs from other fields, and constrained by the need to shape output to external needs, the collective of think tanks is gradually acquiring some forms of weak autonomy; gradually becoming the site of production of specific capital and form of expertise; gradually, in short, is being made into a field.

Our own usage of the concept of “space between fields” is set against the teleology of “field-in-the-making.” We think this space is interesting and socially consequential often precisely *because* of the durability of qualities like permeability, fuzziness, hybridity and weak institutionalization that the concept of field tends to obscure. Consider Lisa Stampnitzki’s (2008) study of 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. Yet, despite Sisyphean attempts to institutionalize the endeavor of terrorism expertise it has remained fairly ill-defined, open to various interlopers and, as Stampnitzky predicts, unlikely ever to possess its own jurisdiction. Yet, for all its permeability and instability terrorism expertise is no less socially consequential.⁵

The following is a brief and incomplete list of the characteristics of spaces between fields understood as spaces of opportunity and not as fields-in-the-making:

⁵ Adriana Petryna’s (2009) study of the globalization of pharmaceutical trials provides another instructive example. She describes how entrepreneurs constantly seek out deregulated niches across the globe where they can outsource labor and risks, and in the process blur the lines between clinical care and experiment, research and marketing, academia and big business.

1. *Permeability*: entry into this field from all the other fields is relatively easy, which means that its boundaries are blurred and invites analysis in terms of network connections that stretch across field boundaries.
2. *Under-regulation*: within this space, the rules about what one can legitimately do/combine are relaxed. In the case of terrorism expertise, one notes that academics can study an object that is essentially defined by state activity – “terrorism” – while state officials can engage in research that does not have clear policy implications.
3. *High stakes*: typically, the prizes to be had in the space between fields are relatively large – government money, media fame, connections – and cannot be had normally in the other fields, precisely because this is, as Bourdieu says, a space of articulation between the fields, where exchanges and alliances are contracted that cannot be done within fields.
4. *Weak institutionalization*: these marginal actors engage perhaps in field building. In the case of terrorism expertise, for example, the academics produce chronologies of “terrorism events” amass bibliographies of terrorism-related knowledge, write retrospective histories of the field’s emergence, and collaborate with state officials to create institutes and centers of terrorism studies, etc. Yet, typically these attempts founder and no clear division of labor or hierarchy of worth emerge. To some extent, this is the result of a stalemate. Each actor has brought with it a different type of expertise, dependent on a network that stretches in a different direction, and none is able to impose itself on the others. But there are more positive reasons as well.
5. *Raid*: When we combine the qualities of permeability, under-regulation and high stakes, we see that such a space between fields may be valuable for actors in other fields, because it provides them with the opportunity for a “raid” strategy: incursion through blurred and penetrable boundaries, rapid amassing of profits in an under-regulated space with high stakes, and no less 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.
6. *Strategic ambiguity*: finally, weak institutionalization may be neither a sign of the nascent status of a “field-in-the-making,” nor the result of a stalemate, nor the effect of multiple raid strategies, but because the resultant ambiguity is itself productive. As David Stark (2009, 15) notes, “entrepreneurship is the ability to keep multiple evaluative principles in play and to exploit the resulting friction from their interplay.” He calls this “asset ambiguity,” signaling both the uncertainty about which order of worth and which asset are in play, and the fact that ambiguity itself is an asset. There are great advantages in staying liminal and ambiguous. Take, for example, 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 desired by new forms of expertise. But the sociology of expertise would note that a no less plausible trajectory, especially for those

who enter the in-between space from the marketplace or the laity, 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. Like chiropractors, they can wear white gowns and run “clinics,” yet escape the close scrutiny and subordination that comes with entry into the medical field.

What is expertise?

It should be clear by now that we are thinking of expertise neither as an attribution, nor as a set of skills possessed by an individual or a group, but as a *network* connecting together actors, instruments, statements and institutional arrangements. An expert, by the same token, is not an individual or a group, but using Callon’s (2005) term, an *agencement* or assemblage. We can return to Collins’ analysis of computers as “artificial experts” provided that they are continuously supported by humans that – at a minimum – digitize their input and repair their output. To put it bluntly: did “deep blue” beat Gary Kasparov? No. It was the team of chess masters and computer programmers that prepared Deep Blue for a match specifically with Kasparov, and that continuously monitored its performance. Without them, Deep Blue is deeply disabled. They serve as its prostheses, so to speak. But to be fair to the computer, Kasparov too had his own team, as every Chess grandmaster does. It was that team, another network, another *agencement* - which lost. We are all disabled, to quote Callon. To be experts, we all need prostheses of various sorts – computers, subordinates, equations.

The network approach has the potential of reconciling “attributional” and “substantivist” accounts of expertise. From a network point of view, attributional struggles are about much more than the mere assignment of a label. They are about rearranging relations and rechanneling flows within a network of expertise. Obviously this is harder to do in heavily capitalized sciences, where the price tag for entry and transformation is set incredibly high (Bourdieu 1975) and thus a relational approach would seem to be merely about “attribution” since the only way to possess contributory expertise is to work in a state-of-the-art laboratory (Collins and Evans 2007, 70-76). Another way of saying the same thing – and herein we see how Bourdieu’s and Latour’s approaches may converge – is that in these sciences the chain of transcriptions supporting and conveying statements is exceptionally long, complex and black-boxed. A good example is PET imaging, where members of multidisciplinary teams often do not know the details of what other members do elsewhere along the chain of transcriptions (Dumit 2003). In less heavily capitalized sciences or forms of expertise, however, these chains of associations are shorter and fragile, more vulnerable to wholesale rearrangements. By the same token, the network approach is “relational” but not in the sense that expertise is seen as a mere label or attribution. It pays close

attention – perhaps too close – to what the experts actually do, but it simply refuses to stop looking when other characters or devices step in to do part of the work.

What are networks of expertise made of? What types of relations are pertinent to analysis? We are going to suggest an analytical grid that is composed of 5 dimensions, 5 pertinent questions or topics of investigation, but clearly it is not exhaustive. It draws upon an earlier study of the network of expertise of military intelligence. (Eyal 2002; 2006)

First, there are the relations between those empowered to speak as experts and those things or persons about whom they speak. A network of expertise has to recruit or mobilize or stabilize what it is talking about, to get it to perform what is required of it to perform. This is what Latour (1988) says about Pasteur, that his network had to include the microbes, get them to perform and guard against the possibility of betrayal. This is also what is meant by the performativity of economics. (Callon 1998; Mackenzie et al 2007)

Security scholars might object that this is not applicable to security expertise, because it often deals with the unexpected, with “unknown unknowns” and with enemies who cover their tracks and hide their intentions. How could one speak of network relations with such unknowns? This point is well-taken, but we think the distinction is better understood as a matter of gradations rather than all-or-none. Take the case of military intelligence. It would seem that intelligence expertise is precisely about trying to get information about somebody, an enemy, with whom you have no relations and who is trying to hide the information from you. How is it possible to talk about an alliance, partnership, or a network connecting the two sides? Without denying these obvious facts, we would argue that the image of intelligence work as “passive” – as consisting of monitoring, collecting pieces of information, and combining them into a full picture – is false. This image originates from a particular node in the network of intelligence expertise and is meant to tilt “trials of attribution” in its favor. This node, in the Israeli case, was occupied by academically trained “research” officers, who were seeking to assert their superiority over an earlier generation of field officers, known as “Arabists”. (Eyal 2006) The Arabists were not academically trained but grew up among Palestinians and had many friends and acquaintances whom they used as informers. Note that the image of intelligence as passively piecing information about a distant enemy makes sense for the academics, but makes no sense from the Arabists’ point of view. They were talking to these guys every day!

We cannot delve deeper into this intricate and fascinating history, but what is clear is that Arabist expertise did involve a network that connected them with the enemy. One could whisper certain warnings on the ears of one's informers and see one's "predictions" become reality the following day. Intelligence was also manipulation, mobilization, recruitment and bargaining. In short, it was active. What happened when the Arabists lost the trials of attribution to the academics? It meant that within the network of intelligence expertise, distance and erudition were worth more than proximity and first-hand experience; it meant that the academics were now doing "research", and the Arabists merely "data collection" or "information-gathering", but it did not mean that the reshaped network was devoid of "active" elements that serve to form ties with the adversary (however incomplete and unsatisfactory these may prove to be, as shown by intelligence failures and strategic surprises). Such ties were formed by means of the intelligence assessment itself. These assessments are meant to be secret and for the eyes of the decision-makers, but for reasons that will be discussed below they are invariably leaked to the press. The leak is not a deviation, but a device, one of the means by which the assessment mobilizes or recruits the adversary into the network. This is a little bit like what is called in medicine "diagnosis by treatment" and contrary to orthodox representations of medicine is very common, far more common probably than going to consult the diagnostic manual. The leaked assessments are read by the other side as a message, or more precisely whatever the other side does will be interpreted as a response to the message, this interpretation will be leaked once again, and the game will recommence. Over time, this has the potential to create a stable set of relations that tends to confirm the annual assessments, because the other side as well has an interest in some measure of predictability and an open line of communication. (Eyal 2002; 2006) The expertise of intelligence officers, therefore, their ability to "predict" or "assess" the future, is not some mysterious or magical insight, nor is it most of the time due to inside information provided by secret agents (HUMNET), but is a routine quality of the total network of relations between intelligence officers, political and military decision-makers, academic commentators, journalists and the opponent about whom the assessment is made.

Political risk analysis, by comparison, aims to harness the market in order to create a stable set of relationships that reach all the way to recruit the polities it analyzes. Presumably, if investors all orient their decisions by means of the same set of risk ratings, risky countries will become investment-starved and so even riskier, and safer countries will become safer. The network of political risk analysis will perform its object so the prophecy will confirm itself. It doesn't work like that today, since political risk consultancies do not have this kind of clout, nor is it ever likely to work in this way. As we know about consumer credit ratings agencies, the rated will turn around and bargain with the raters until they obtain the scores they desire. (Poon 2007; 2009) This only means, however that the relationships within the network of political risk analysis expertise are more equal, with both clients and objects wielding as much power or even more power than the experts.

Second, every network of expertise includes a set of relations between those who are empowered to speak as experts and those who produce knowledge but cannot speak, humans as well as non-human devices. Mechanisms internal to the network determine the position from which it is possible to speak authoritatively and represent the whole network, and the actors capable of occupying this position. In Israeli military intelligence this was codified by means of an administrative division between research and information-gathering branches. The expertise of research officers is not a quality they possess by virtue of their education or their skills but inheres in the fact that they won the “trails of attribution” and managed to position themselves at what Latour (1987) calls an “obligatory point of passage” in the network, a point through which must pass all the different flows of information arriving from agents and informers, aerial photos, monitors of the Arab press, electronic eavesdropping, military attachés, and foreign intelligence agencies. By the same token, having lost the “trails of attribution” the position occupied by the former Arabists became mere “information-gathering”, mere data collection, their expertise and worth within the network has been downgraded.

A similar expropriation, a similar rearrangement of network relations, was needed for political risk analysis to emerge. Why should corporate executives pay for political risk analysis? The age-old tradition was to rely on the reports of regional managers. Even intelligence services used to rely on them as sources. So the pioneers and entrepreneurs of political risk analysis had to begin by discrediting regional managers as a valid source of political intelligence. Kobrin (1980) argued that regional managers were biased and subjective. Especially if they were natives of the country in which the firm was operating, they had a conflict of interest between loyalty to the company and to the country. He quotes the Chairman of a Multinational corporation who said of the regional managers: “It is not that they like the establishment, but that they *are* the establishment.” (143) Regardless of nativity, regional managers also had a stake in preserving their own position and power, and their intelligence roles were rarely explicit. In general, he argued, regional managers lack the social, cultural, or intellectual capability to communicate with the top brass, who have their own subjective biases as well. The problem was built into the organization and into managerial relations, and the solution had to be institutionalizing the function of intelligence and separating it from active management, whether by forming an in-house department or outsourcing. So why shouldn't the corporation go ahead and hire its own experts, whether as in-house or freelancers? Political risk consultancies offered to reduce search and transaction costs, but more importantly they guaranteed objectivity by working with a large and trusted group of experts, by pooling their assessments together and computing probabilities, and to the extent that were well-connected, by guaranteeing that one's competitors shared one's risk perception. Ultimately, the network that political risk analysts built is predicated on the silence of both regional managers and academic area experts. The latter produce knowledge, but

cannot speak as their inputs are digested into a quantitative score, and as the political risk analysts occupy an “obligatory point of passage” in the network, between them and the clients.

Third, every network involves a set of relations between those empowered to speak as experts and those who listen to them, their clients. The clients/patients/consumers are also part of the network. Take the intelligence researchers and their clients. Their clients are the top military and political decision-makers, some of the most influential and powerful individuals in Israeli society. This fact carries with it certain advantages, since such powerful individuals are capable of providing the network of expertise with abundant resources, and indeed they have compelled all other intelligence agencies to report to the research branch of military intelligence. Additionally, as noted earlier, the politicians and top brass give intelligence expertise instrumentality, a way to “work” on its object, to secure the performances it needs from the opponent.

On the other hand, there are distinct disadvantages and dangers: the politicians and the top brass are impatient clients who tend to usurp for themselves the right to speak as experts and in many cases limit and minimize the experts’ freedom of speech. Precisely because they are positioned at an obligatory point of passage, a bottleneck mediating between the men of power and the rest of the network, the researchers at military intelligence are especially vulnerable to the danger that their clients will appropriate their assessments and present them as if they were the product of their own thinking; or conversely will present whatever suits their interests as if it was “the opinion of intelligence”.

Two consequences follow from this predicament. First, intelligence officers must forge their own ties to journalists and they must leak (this is why this always happens despite the fact that it is prohibited by law), so they have a check over how their assessments are represented. The second consequence is that intelligence officers must accept to trade anonymity for instrumentality. As one of them said: “If you were right in your assessment and your clients adopted it, very often they will turn it into their own property, as if it derived from their own logic, and your part in it will be forgotten. You should consider precisely this situation to be an ideal accomplishment.” (Oron 1987) The experts, therefore, are weak, but the form of expertise is strong, via generosity.

Fourth, a network of expertise could be analyzed also from the point of view of the exclusions upon which it was established. Put differently and paradoxically, the network includes a set of relations with people or things or concepts who are included in the network by means of

being excluded from it. The form of expertise cannot function without eliciting and suppressing them at one and the same time. A classic example analyzed by Foucault (2003, 203-227) in his lectures on the emergence of the abnormal individual are the convulsions. The origin of the convulsions was in resistance to the new practices of confession and spiritual direction in the seminaries (especially for nuns). At that point they were a blend of performance and religious discourse writ on the body. Eventually, the church transferred jurisdiction over the convulsions to medicine and they became "this immense spidery notion that extends its web over both religion and mysticism on one side and medicine and psychiatry on the other." They became a religio-medical concept and problem area upon which the nascent discipline of neurology formed. The convulsions, in the form of hysteria, hypnosis and dissociation remained the privileged object of neurology for many years. They corresponded to its status within the expert division of labor as a "buffer" jurisdiction, between medicine proper and abnormal psychiatry, between the somatic and the psychic. Yet their original character, original mechanism of production, could not be part of neurology. In fact, neurology was one the main centers of republicanism and anti-clericalism in the Third Republic (Hacking 1998), one would suspect because of the instability of the ground upon which it was established.

The jurisdiction of Israeli military intelligence was also erected upon a characteristic exclusion. For intelligence to become this piecing together of information about a distant and inscrutable enemy, for the research officers to assert their superiority over the Arabists, the dense ties between Jewish and Palestinian society had to be cut. This was done by 1948 war and the expulsions, but not quite, because many of those who left – forcibly or otherwise – were seeking to return. The border region was unstable, and the character of its inhabitants undetermined: were they enemies coming from without, refugees returning to their homes, internal refugees, or citizens of the new state? The war against "infiltration", conducted from 1949 and escalated gradually into a low-level border war until it erupted into full scale war in 1956, aimed to resolve this indeterminacy. During this same period, and by virtue of the same exclusion, the jurisdiction of military intelligence emerged. It now dealt strictly with enemy states and armies that were distant and who could only be manipulated through the mechanisms of assessment, leaks, messages and "deterrence". If infiltrators or refugees were involved, military intelligence explained that they were directed from above, or that the opponent regime was merely opening a safety valve and letting them act on its behalf, and it counseled punishing the opponent regime and military so they would impose order on their own side of the border. You can see, therefore, how the jurisdiction of military intelligence was predicated on the purification and exclusion of the infiltrator-refugee hybrid. The network of intelligence expertise is based on this exclusion, because the devices of manipulation it deploys – the intelligence assessment as a message to the adversary, the analysis of "intentions" – do not work on the hybrids. Hence it is impossible to translate their interests, to form alliances with them, or to secure from them the performances of interest.

This became evident from 1993 onwards when the Palestinian Authority was formed and the refugees were back, so to speak, at least back into the jurisdiction of military intelligence.⁶ (Eyal 2006)

The final analytic term in the grid we are laying for the analysis of networks of expertise is what Latour calls “translation”. *What transforms a network tie into a more or less stable alliance is the interpretation, which, as Latour puts it, “translates” the interests of the different participants and creates between them a certain level of correspondence and coordination.* In the case of military intelligence, what translated the interests of the different parties involved – the gatherers of information, the researchers, the decision makers, even the adversary – is a very specific account, according to which the role of research officers is to divine the other side’s *intentions* on the basis of the information provided to them. This created a neat division of labor between the researchers and the gatherers of information. It thus translated the interests of the latter and even though they were now subordinated to the researchers, they were equipped with a coherent understanding of their role and contribution. They can even use it to criticize the *hubris* of the researchers, as they often did after intelligence blunders. By the same token, the focus on the interpretation of intentions means that intelligence assessments are in a format that is readily understandable and actionable for the clients. This form of discourse grafts itself productively and with ease onto the operational-strategic thinking of the decision-makers, and intuitively makes sense to them – those who “make decisions” tend to understand the world around them as the product of decisions and intentions. Finally, the premise of the whole game of communication

⁶ This discussion highlights another contrast between the sociology of professions and the sociology of expertise, which we have deleted for reasons of brevity, namely their mode of narration. Abbott criticized the mode of narration of the sociology of the professions, which followed the ideal typical “life history” of a profession. What is needed, Abbott argued, is a *history of tasks and jurisdictions*, not of groups; a history without a protagonist because often the protagonist (the professional group) is an effect of this history rather than its point of beginning, but more importantly one cannot assume that this protagonist is the necessary end point of this history, or the whole narrative becomes teleological. A history of tasks and jurisdictions, as Abbott suggested, but never really delivered, would be a history of expertise which is not reducible to the history of actors, a history of multiple and heterogeneous problematizations (Rose, 1992, 353) taking place in the interstices between established jurisdictions that meet and give rise to domains of similarly constituted objects. We do not think Abbott really delivered on this promise to write a history of tasks and jurisdictions. He defined a jurisdiction in advance in terms that would not have been recognized by contemporaries – the clearest example is the jurisdiction of “personal problems” to which is dedicated a good portion of the third part of his book. Put differently, while he says correctly that one should not tell a story governed by projecting backward the necessary formation of a professional group, the story he tells is governed by projecting backward the formation of a given, objectively bounded jurisdiction. We think precisely this anachronism is avoided by the genealogical method employed by Foucault (2003 [1974-1975]), for example, to reconstruct the emergence in the 19th century of the figure of the abnormal individual and the corresponding form of expertise. Regardless, a history of tasks and jurisdictions would be a history of the conditions of possibility that bring into being a domain, or an “ecological niche” (Hacking 1998), of objects of expertise, and the constitutive exclusions we discussed above would be among these conditions of possibility.

represented by the leaked intelligence assessment is that the assessment reveals to the other side that their intentions are known, that the leak itself is guided by an intention (to send a message, to deter, to escalate or de-escalate), that it is interpreted by the other side in this way, that their maneuvers or pronouncements are guided by an intention to respond to the message, etc.

Conclusions

To summarize, we have argued that for conceptual and historical reasons the sociology of professions has now reached its end and it should be replaced by the more comprehensive and timely project of a sociology of expertise. We have contrasted the sociology of expertise with the sociology of the professions along six dimensions, emphasizing its larger and more flexible scope; the fact that it distinguishes between experts and expertise as two irreducible modes of analysis; its attention to analyzing different modes of abstraction as chains of transcriptions; the analysis of power in terms of both monopoly and generosity; the understanding of expertise itself as a network, rather than a set of skills possessed by an individual; and the fact that such networks of expertise traverse boundaries between jurisdictions and fields and require a topology of intermediate and relatively indeterminate spaces of overlap and interface. We then provided a conceptual grid for analyzing networks of expertise.

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