How Parents of Autistic Children Became Experts on their Own Children: Notes towards a Sociology of Expertise

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How parents of autistic children became “experts on their own children”:  
Notes towards a sociology of expertise  
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This paper aims to use materials from an empirical project on the social origins of the autism epidemic to develop a case for the usefulness and timeliness of the sociology of expertise. Accordingly, we will begin with an empirical story, the story of Bernard Rimland and the creation of NSAC, the National Society for Autistic Children. It is a story, as the title indicates, of how the parents of autistic children won the status of experts on their children’s condition and upended the hierarchies of expertise in this field. It is also the story of the making of autism into a protean spectrum encompassing multiple forms of disability and variable degrees of severity. Weaving in and out of this story, we will attempt to draw from it a more general analytical grid for the study of expertise and develop its implications for the diffuse area of knowledge, science and technology studies.

Rimland and the formation of NSAC

In the late 1950s, Bernard Rimland was a psychologist employed by the US Navy. He was also the father of an autistic boy. Unconvinced and offended by the prevailing psychogenic explanation for autism (the notorious “refrigerator mother” hypothesis that posited unloving mothers as a the cause of autism), he began researching the matter himself. Out of his reading and extensive correspondence with researchers came his path-breaking book, *Infantile Autism* (1964), widely credited to have completely changed the field of autism research and treatment. Within a week of publication, Rimland began receiving letters from parents. They consisted of pleas for help with severely afflicted autistic and “psychotic” children as well as completed E-1 checklists (Rimland 1964, 1971). The checklist had been published as an insert in Rimland’s book and was intended to be filled-out by parents and sent back to the author. It consisted of questions about the behavior of their three-to-five year old child, closely following Leo Kanner’s (1943) classic description of autism. Rimland was excited about the enthusiastic response, but bewildered about what to do next. He had very little to offer parents, since the thrust of his analysis in the book was to establish autism as an inheritable neurological disorder for which there were no known medical interventions. At this critical moment, he heard talk about O. Ivar Lovaas, a behavioral psychologist at UCLA, who was developing a method for the treatment of autistic children. Within weeks he made the trip from San Diego to meet Lovaas. He was invited to observe Lovaas at work with patients or in training sessions with students. He spoke with some of the patients. Utterly impressed with what he saw, Rimland returned home and tried the method on his own autistic son, Mark, with instant, if modest, success. Further, Rimland brought Lovaas along with him to a dinner with local parents, thereby forging a connection between the researcher and the parents and paving the way for Lovaas, a few years later, to incorporate them into the therapeutic process (Rimland 1972; Lovaas 1971).

Like Rimland, Lovaas was being inundated with letters from parents, as was Rosalind Oppenheim, a mother who in the summer of 1961 published an article in the *Saturday Evening Post*, titled, “They Said Our Son Was Hopeless,” describing her success in treating her autistic son (Silverman 2004; Lane 2008). Both passed the letters along to Rimland, who added the
names to his by-now very long list. In 1965, a number of television and news outlets ran features on autism and autism treatment, including a *Life* magazine feature introducing Lovaas’ work to a popular audience (Moser and Grant 1965). During this time, Ruth Christ Sullivan – herself a registered nurse – saw a television special on autism and, recognizing characteristics of her son Joe, contacted Rimland (Warren 1984, 102). She and Rimland then decided to organize a meeting of parents and began contacting names on Rimland’s list. To the emerging alliance between Rimland and Lovaas, Sullivan brought her considerable experience lobbying for the League of Women voters (Park 1971a). The two men, in fact, were soon joined by a formidable cadre of women activists who had already accumulated experience in the women’s rights movement.1

The founding meeting of the National Society for Autistic Children (NSAC) took place in Teaneck, New Jersey, on November 16th, 1965 (Warren 1984 101-103; Lane 2008).2 Rimland acted as chairperson. He passed out a list of things parents could do with their children (Warren 1984, 102), and gave a speech based on his observations of Lovaas and his own reading of the behavioral literature titled, “Operant Conditioning: Breakthroughs in the Treatment of Mentally Ill Children.” “It was intended to be, and succeeded in being, a summons to parents to come forward and insist upon the abandonment of Freudian theory and the adoption of a totally new and different concept in the treatment of autistic-type children,” he later wrote (Rimland 1972, 573). Most importantly, Rimland noted that operant conditioning was a “technique that parents could learn with demonstrable success” (Rimland 1972, 573). This speech was highly influential, and in the seven years before it was published, copies were circulated via informal channels to nearly all English-speaking countries and translated into six different languages (Rimland 1972, 573).

*Why expertise?*

Our first order of business is to explain why and how we think this story could be analyzed from the point of view of expertise. Clearly, it is possible to analyze this story from a “political” perspective, with NSAC as a social movement, a lobby, an advocacy group concerned with de-stigmatization and resource mobilization on behalf of a constituency (King 2008). There are, however, several details of the story that do not fit this frame of interpretation. We do not mean they contradict a political interpretation, but they are orthogonal to it and appear relatively insignificant unless one thinks in terms of expertise.

First, there is the seemingly trivial point that Rimland’s book was written and published before the movement was formed. In a political analysis, lobbies or social movements are formed to articulate collective interests, and they recruit experts who write books as part of resource mobilization. Here the sequence was inverted, and not only in the superficial sense that Rimland wrote his book before the movement was formed. More importantly, as we shall see in a moment, more than the movement needed Rimland’s book, the book needed the movement as its

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1 S. Clarence Griffith, the second elected NSAC president, noted that his wife used what she had learned working with the League of Women’s Voters in order to lobby Georgia lawmakers to include autism in their 1968 “Exceptional Child Act.” He told the NSAC crowd in 1970 that they should join forces with the League, the Federation of the Women’s Club, and the Jaycees in order to lobby effectively for services (Griffith 1971).

2 Both the name and later the puzzle-piece logo were borrowed from the British Society, founded three years prior by Lorna Wing – psychiatrist and mother of an autistic girl, with whom Rimland corresponded (Warren 1984, 102).
sequel. Yet what a book to begin a movement with! This was a dry, pedantically “scientific” book that conducted a polemic against the psychogenic hypothesis with the detachment of an entomologist. Compare it with the books that were published prior to the formation of the National Association for Retarded Children (NARC) – Perl Buck’s moving memoir of her daughter and Dale Evans Rogers’ *Angle Unaware* (Trent 1994, 231) – it had none of the passion or appeal that make for rabblerousing. Even with respect to the main political goal of de-stigmatization, it was strangely ambivalent. We do not mean that Rimland conceded any part of the psychogenic hypothesis; there could not have been a more uncompromising enemy of psychoanalysis. But he affirmed (and exemplified) nonetheless the main empirical evidence supporting this hypothesis, namely the claim that parents of autistic children were unique and different than other parents, typically intellectual, of higher intelligence, and perhaps, he conceded, also cold. He insisted, however that the correlation was evidence of genetic inheritance, not emotional trauma. Autism was a “rare recessive trait” that was over-diagnosed. From a political point of view, this is curious to say the least. De-stigmatization would require setting maximum distance from the psychogenic hypothesis, not treading dangerously close to its slippery slope. Movement building would require a wide definition of the potential pool of parents to be organized, not a restriction that could potentially involve the fledgling society in a bruising conflict over who is “really” autistic and who is not. But Rimland had his reasons. He was concerned above all to avoid any implication that autism was a form of mental retardation. Autistic children, he wrote, “were to have been endowed with unusually high intelligence” (Rimland 1964, 124). We can snicker at what seems like special pleading, but this was a crucial maneuver to navigate autism and autism expertise into a space between mental illness and retardation. We will see later its significance, but for the moment let us simply note that for all these reasons Rimland’s book was a curious and unlikely candidate to have started a movement.

The second orthogonal detail is the E-1 checklist. What does it have to do with resource mobilization? Everything, it would seem, since this was how, concretely, the ball got rolling. It was Rimland’s E-1 checklists that first connected him with parents and furnished a good part of the list used to summon the first meeting. However, Rimland did not devise these checklists with organizing in mind, but in order to advance his (and other parents’) claim to expertise. Here he was, a navy psychologist attempting to compete in the same field with psychiatrists and MDs. His knowledge of his subject matter, however extensive, was – despite occasional references to his son, and even to himself – bookish. *Infantile Autism* was the book of an outsider, marked by the stigmata of looking from the outside in. It was admittedly speculative. He summed up the results of others’ research, but had very little of his own. Without a clinic, like the psychiatrists, or a laboratory, like Lovaas, how could he make them stop and listen to him? What tools did he possess to support his claim for expertise? He turned to what he knew best – psychometrics, the E-1 checklists – and this brought him in touch with other parents. Activism and the formation of a parents’ association were an extension of this quest to forge a new type of expertise, not vice versa.

Finally, there was the role played by behavioral therapy. From a political perspective, it is not surprising that a discussion of therapy was the centerpiece of the very first meeting of NSAC. One would expect, however, that the meeting would serve to articulate this collective interest and to press a demand – as a lobby should – to provide and fund these therapies. This was not, however, the main thrust of Rimland’s talk and of the proceedings that followed, which were
about something else: the dissemination of “Do It Yourself” knowledge among parents so they
could treat their children at home with a sort of an implicit barter offered by Rimland: “give me
E-1 checklists, and I’ll give you our collective experience with what works in terms of therapy.”

So much for the limits of a political approach to this story. We hope the reader is convinced of
the possible fruitfulness of considering NSAC from the point of view of expertise. But hasn’t this
been done already? Isn’t the story of NSAC simply another example of the construction of “lay
expertise,” analyzed in Epstein’s (1995) path-breaking study of AIDS activism? There are
obvious similarities between the two cases: ACT-UP activists fought to remove the stigma of
AIDS; parents of autistic children fought to remove their own stigma as “bad parents.” ACT-UP
activists targeted the medical establishment; NSAC parents the psychiatric. ACT-UP activists
developed working knowledge of the relevant science, and ultimately were accepted as credible
experts in policy forums; parents of autistic children read voraciously and translated their
knowledge into effective lobbying. But here the similarities end. Epstein’s analysis, we feel,
remains wedded to a “political” approach. The expertise of ACT-UP activists is mobilized as a
resource in their struggle against the medical establishment. It is agonistic. More importantly, the
oxymoron of “lay expertise” merely signifies that at the end of the day, when the dust settles, the
two are left quite distinct, experts on one side, and laymen on the other. It is not surprising,
therefore, that Epstein ends his story on the rueful note of “cooptation,” and that Harry Collins
and Robert Evans (2007) classify the expertise of ACT-UP activists as “interactional” – i.e.
merely the capacity to converse seriously and engagingly about technical matters with the
actually practicing scientists who possess superior “contributory” expertise. They would be hard
pressed to do the same, however, with NSAC. To quote psychologist Eric Schopler:

If you rack up the contributions of parents like Rimland, Wing, Park, Kysar, May,
Eberhardy, Junker against the professional contributions to the literature on autism, these
parents may easily have made a more lasting contribution than the professional group (in
Sullivan 1971, 2).

ACT-UP activists were trying to get a foot in the door, to plug into an existing network. NSAC,
by contrast, took the existing network apart and, as we shall see, wove a wholly new and
alternative network of knowledge production and dissemination and then invited the established
experts in. No less importantly, the expertise developed by NSAC was not, at least not primarily,
agonistic. It involved the hybridization of identities, blurring of boundaries between expert and
layman, and crucially “co-production” of common objects of inquiry and treatment. NSAC is
similar not to ACT-UP, but to the French Muscular Dystrophy Association (AFM) analyzed by
Vololona Rabeharisoa and Michel Callon (2004). And for good reason: in both cases, parents
and researchers were dealing with intractable, incurable conditions. In both cases, the struggle
was to define a level of intervention between cure and neglect, and to develop a form of expertise
adequate to this level.

Whatever its limits, the term “lay expertise” serves to indicate the timeliness of the sociology of
expertise. The context, the provocation for the sociology of expertise, is no doubt provided by
what Collins and Evans (2007) call “the problem of extension”: namely, given that contemporary
politics hinge on the analysis of ever-more complex technical questions, yet given also that the
authority of science and the professions has been relatively destabilized, who is qualified to
intervene in such matters? And on what basis? The term “expertise” is good for addressing this question, because it does not prejudge the answer. Expertise, according to the Oxford Dictionary, is “an authority by reason of special skill, training or knowledge.” It could derive from credentials or from experience, from professional status or personal involvement, it could be abstract or practical. In jurisdictional terms we could say that the object of the sociology of expertise is narrower than the sociology of knowledge – there is a distinction between lay and expert, not everybody can be expert and not all forms of knowledge are of interest here; but it is also wider than the sociology of professions or the sociology of science – it does not prejudge where and how the boundary between lay and expert is drawn.

*Multiplying the “n of 1” cases*

Out of the meeting between Rimland, Lovaas and Sullivan came a highly successful organization. But perhaps something even more important and lasting emerged from it as well: a new type of expert, a parent who was also a researcher and an activist like Rimland or Lorna Wing; a parent who was a self-taught therapist, like Oppenheim or Amy Lettick, both mothers who treated their autistic boys at home and then went on to found and lead their own schools for autistic children. We think, moreover, that NSAC represented something larger and more long-lasting than the contributions of these few charismatic individuals, namely the invention of a whole new modality of expertise based in a new economy of exchanges between parents, activists, researchers and therapists.

We have already seen why, in part, Rimland felt compelled to reach out to other parents and barter with them. It is fairly self-evident why a parent-activist like Sullivan, interested in creating a movement – she served as NSAC’s first elected president – would do the same. But why would Lovaas, the academic researcher, be interested in such extensive contacts with the laity? One has to appreciate Lovaas’ predicament and isolation in the mid-1960s. The diagnosis and treatment of autism was firmly controlled by psychiatrists. Within their frame of reference behavioral therapy at best had no relevance to autism. At worst, they said, like Bettelheim, that behavioral therapy was harmful to children, akin to lobotomy. Behavioral psychologists were nonetheless making inroads into psychiatric jurisdiction (e.g. Nicholas Hobbs and Project Re-Ed to treat “severe emotional disturbance”), but working with autistic children was so time-consuming and labor-intensive that Lovaas developed the principles of Applied Behavioral Analysis (ABA) working with only one autistic girl. He had no numbers to make even his fellow psychologists listen to him. More than twenty years passed before he felt he had accumulated enough observations to report on the success of his method in a fairly marginal psychology journal (Lovaas 1987), and even then the number of subjects was very small, the results not statistically significant, the assignment into groups not really random, and potential biases from time period and parental work unknown. No wonder he was roundly criticized, even accused of fudging his results. By that time, however, ABA was by far the leading recommended therapy for autism, practiced by armies of paraprofessionals and parents all over the world. This discrepancy between its minor academic status and its major practical (and commercial) status is what renders ABA particularly instructive to study from the point of view of expertise.

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3 NSAC later became the Autism Society of America (ASA) with currently 173 chapters across the US.
With colleagues hostile, Lovaas had no allies. As he put it, “you do not get tenure” doing this type of intensive, academically uninspiring, theoretically dull work. As he came to appreciate the amount of work and persistence involved, it became clear that it would be impossible to replicate what he managed to achieve in his laboratory without a whole army of paraprofessionals. To transform ABA from a laboratory technique to a viable intervention required finding this army and training it, a task for which Lovaas did not yet have the requisite capital – scientific, economic, symbolic.

What he did know was that the very idea of treatment had to change. Lovaas was critical of the existing therapies for autism, all of which were focused on one or another deficit, which they considered key, i.e. once fixed it would generate change across a large number of behaviors. In autism, claimed Lovaas, no such generalization takes place. One needs to intervene on all behaviors, in all environments, with the help of all significant persons, all waking hours, for a good part of one's life. There are not enough professionals to deliver this.

One can understand, therefore, why Lovaas would turn to parents. Rimland and NSAC offered Lovaas a way out of his predicament. In Barron Lerner’s (2006) phrase, NSAC’s model of exchange among parents multiplied the “n of 1” cases so they could stand up to scientific scrutiny. Each parent reported on results of work with one child, and by aggregating all reports, Rimland had enough numbers in his database to make his arguments strong. This device of multiplying the “n of 1” cases produced an alternative mode of research which potentially could completely bypass and undermine the circuit of exchange upon which depends the legitimacy and viability of experimental science. Within this alternative circuit of exchange, Lovaas’ claim to autism expertise and the efficacy of his therapy were quickly established. His “n of 1” could be multiplied a thousand-fold by diligent parents who followed his instructions and set up quasi-experimental situations at their homes, working with their children. This was the only way Lovaas could replicate and generalize his results and thereby acquire enough “weight” to be recognized as an autism expert. We can understand, therefore, why he would declare that “this means that we will have to give away our professional skills to lay people, and the sooner the better” (Lovaas 1993, 628; Silverman 2004, 190).

Who was, then, the new autism expert? Lovaas, the university researcher? Rimland, the parent activist with a Ph.D. in psychology? Or the parents, like Lettick and Oppenheim, who taught themselves how to treat autistic children and corresponded with both Lovaas and Rimland? These are rhetorical questions, for you can see that not one of them could do it alone, and that we are pushing you to think of autism expertise as a network connecting all three figures, and many more besides. The most distinctive feature of the expertise network represented by NSAC was the knowledge exchange and dissemination that took place within it, among parents who were also activists, researchers and therapists, in a way that bypassed the scientific literatures outside. This is why we emphasized the innovative nature of Rimland’s E-1 checklists. They involved a model of knowledge exchange around which the whole network was organized, and which served to bypass the reigning authorities in the field of autism research. In Collins and Evans’ (2007) terms, it transmuted Rimland’s (or Lovaas’) interactional expertise into contributory expertise, and by the same token downgraded the expertise of the psychiatrists into an interactional one; or in our language, this model of knowledge production involved taking apart the existing network of expertise – in which parents reported to clinicians – and rewiring it to go
through the parent-activist-researcher-therapist. Rimland’s gambit was to treat the parents as credible witnesses of their child’s development and symptoms. We will see below that this was by no means self-evident and required a great deal of work to invert the assignment of credibility from clinician to parent. The checklists Rimland received allowed him to generate an independent database with which to challenge the psychiatric establishment – wedded to the clinical case method, the latter lacked a similar resource and was an easy target (Lakoff 2000). Rimland fulfilled his own side of the bargain by publicizing his findings first in parents’ forums, and especially by creating the link with Lovaas and disseminating information about “operant conditioning” directly to parents. As in the case of the French Muscular Dystrophy Association analyzed by Callon and Rabeharisoa (2004), this meant placing at the center of the exchange not the unreachable and ever-receding goal of “cure,” but the workable problem of managing an intractable condition.

Rimland’s exchange became the model for the communication and expertise network built by NSAC. Clara Claiborne Park, the Society secretary, put her graduate training in English to work editing NSAC’s newsletter, informing parents directly of advances in research and treatment. NSAC also supplied parents with information and referrals (a service run by Ruth Sullivan); it sold books and articles about autism; it compiled and provided bibliographies and lists of relevant films. Most importantly, parent-activist-researcher-therapists like Lettick, Oppenheim and Rimland set up quasi-experimental programs at their homes, and later in the schools they founded, and exchanged information directly with each other and to other parents at NSAC conferences about what treatment methods they found to work with their own children, and what did not.

This meant that NSAC’s parent-activist-researcher-therapists were poised to collaborate with the established experts from a position of strength, on their own terms. They did not come knocking on their doors begging for advice and help, nor even – as in ACT-UP – banged on their doors and demanded attention, in order to ultimately plug into the existing network (Epstein 1996). Rather, they invited the established experts in, to plug into their own network, into the protected space they created in NSAC, wherein parents were credible witnesses and a different type of expertise was recognized. In 1969, NSAC began hosting a national meeting and conference for autism research and treatment. Researchers, clinicians, educators and therapists were invited into this forum, where they had to speak directly to parents. They were thus compelled to come face-to-face with parental needs for concrete solutions and the parental point-of-view about autism and its history. The new terms of discussion were set already in the inaugural meeting, when Leo Kanner – the dean of American child psychiatry, who first described and named autism in 1943, and who also coined the phrase “refrigerator mother” in 1949 – famously “exonerated” parents of any blame or guilt in their children’s disorder. In return, one could say, he was rewarded with the stewardship of the new Journal of Autism and Childhood Schizophrenia (JACS), inaugurated two years later by two members of NSAC, both parents of autistic children, one of whom owned a publishing house.

Networks of expertise

The preceding analysis, before it even reached the point of suggesting to consider expertise as network, was premised on the following point: experts and expertise are not the same thing; they require two different and irreducible modes of analysis. This distinction is the main difference
between the sociology of expertise and the sociology of professions. The latter, for reasons that reach back to American sociology’s response to Mannheim’s sociology of knowledge, 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].” Using this lens to look at Epstein’s case, we could say while it is possible to argue that medical experts have lost some jurisdiction, or at least perceived such a danger, what is obvious is that medical expertise as a whole has gained, because its former critics have now become fierce defenders of controlled, double-blind, clinical trials, and are much more efficient in this task than the medical experts themselves.

The sociology of professions, in fact, reached its end, its logical conclusion, in what is without doubt its definitive masterpiece, namely Abbott’s The System of Professions (1988). Once you read this book carefully, you realize that it ends the sociology of professions and opens onto sociology of expertise. Despite the title, the book is in fact neither about a system nor about professions. It is not about a system because the demonstration of system-like properties relies on the analysis of vacancy chains. As one profession extends its jurisdiction, some tasks it previously controlled inevitably become less well guarded. A vacancy opens, into which moves another group, and so on. It is easy to imagine a vacancy chain connecting, let’s say, physicians, nurses, lab technicians, pharmacists, even administrators or patients. But could you really say that a vacancy chain connects doctors and lawyers or architects and librarians? Vacancy chains and system-like properties obtain not in the whole “ecology” of professions, but within what Abbott calls “task areas,” which are something like “fields.” Similarly, the book is not about professions per se because Abbott’s methodology is designed to do away with “ideal types” either of profession or professionalization and to include in the analysis all relevant actors within a task area, even quasi- or non-professional actors, whoever can lay a claim to expertise. Hence the subtitle of his book – An Essay on the Division of Expert Labor – is a lot more accurate in describing what the book does.

Finally, and closer to the matter at hand, Abbott criticizes the mode of narration of the sociology of the professions, which followed the ideal typical “life history” of a profession (e.g. Willensky), and says that we need a history of tasks and jurisdictions, not a history 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 (the absent hero of the narrative). Now, if there is one history of tasks and jurisdictions, and another of groups – though obviously they are intertwined histories – then we think this is tantamount to distinguishing experts from expertise, and allowing that each requires a different mode of analysis. There is a history of expertise which is not reducible to the history of actors. It’s 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 delivers on the promise to write a history of tasks and jurisdictions. He defines 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 the 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, with his call for a history of tasks and jurisdictions Abbott certainly suggested the necessity of a mode of analysis in which expertise was not reducible to the 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. 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 (Collins and Evans 2007, 2), and most importantly it has very little to say about what experts actually do. ABA, we are sure, did not owe its success merely to social recognition. In fact, as we saw, it was denied recognition by some very significant others. But neither was it successful because it somehow possessed the truth about autism. On the contrary, Lovaas (1981, x; 1979, 317) denied that there was such truth to be had, and called autism a “hypothetical” or “speculative” entity. Sociology of expertise must pay attention to what experts actually do, how they render problems visible and actionable, and not to take shortcuts either via “social construction” or “reality.”

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 (or lack thereof). 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. They 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 the fact remains that however neutral they wish to appear, their model of expertise inevitably is biased in a particular way, marked by the second context, the second provocation to which the sociology of expertise arises in response, namely the challenge posed by “expert systems” and “artificial intelligence.”

Witness how, 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. Yet a robust sociology of expertise would need to be able to account also for the actual implementation and considerable successes of expert systems.
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 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, but should refer rather to the full chain of associations that connect the two and the full set of actors, tools, machines and transcription devices necessary to convey a statement up or down the 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 (Latour 1987; 1999). 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) opts for the 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, de-buggers, 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.4

The network approach has the potential of reconciling “attributional” and “substantivist” accounts of expertise. From a network point of view, attributional struggles – we will analyze one such struggle in the following section – 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

4 This Heideggerian point is certainly not lost on scholars like Dreyfus. But we think this aspect is underplayed his and Collins’ work in service of a critique of expert systems and artificial intelligence.
thing – and herein we see how Bourdieu’s and Latour’s approaches may converge – is that in these sciences the chain of associations 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 associations (Dumit 2004). In less heavily capitalized sciences or forms of expertise, however, these chains of associations are shorter and fragile, more vulnerable to wholesale rearrangements. In the next section, we will see indeed how parents of autistic children, who seemingly possessed only interactional expertise, were able to emerge out of the “attributitional” struggles with upgraded “contributory” expertise, while the expertise of dynamic clinicians and therapists was downgraded. This is good evidence, we argue, that it is useful to treat expertise as a network property, since the parents’ coup involved taking apart a set of relations within which they were subordinate, and their expertise merely “interactional,” and “rewiring” it, so to speak, by redefining almost everything about the expertise (its goals, techniques, subjects and objects) relevant in the field of autism.

The problem of credibility and its resolution in a new economy of blame and worth

For the network of expertise to be rewired, however, the problem of the credibility of parent testimony had to be resolved. Witnessing is the ideal of empirical science, but it can never really be achieved. Even in public experiments, there is the background of failed attempts in the private laboratory. Thus, testimony has to replace witnessing, which raises problems of credibility. Steven Shapin (1988) has demonstrated how, in seventeenth-century experimental science, the problem of credibility was resolved by moving from the juridical model of examination to one of gentlemanly “virtue,” by restricting access to gentlemen witnesses and relying on their status and code of honor to legitimate their testimony. From then onwards, spatial arrangements – from the gentlemen’s house to today’s laboratories – have served to legitimate empirical science by controlling access, constructing visibility and creating a smooth flow of “virtual witnessing” (Ophir, Shapin and Schaffer 1991).

How could parents, each in their own private home, each with an obvious interest, be taken as credible witnesses whose testimony could be trusted? To make matters even more difficult, by the mid-1960s, parents of autistic children were among the least credible, the most discredited, of all witnesses. They were caught in an economy of blaming that made their very attempts at accurate and credible testimony serve as the basis for, so to speak, downgrading their credit-worthiness. As Ruth Christ Sullivan noted, parental testimonies were, in fact, the ground on which Leo Kanner built the syndrome in the first place at the same time as he used them to discredit the parents. The detailed diaries, reports, and remembered developmental minutiae, Kanner wrote, “furnish a telling illustration of parental obsessiveness” (Kanner 1943, 250, cited in Sullivan 1984, 239). “Yet,” Sullivan points out, “when he is struck by the same phenomena and writes thirty-three pages of detailed description of these children’s unusual behavior based on those mothers’ notes and good recall, he is called the ‘father of autism’ and hailed as a gifted observer, which, of course, he was” (Sullivan 1984, 239). Thus, over the two decades after Kanner’s article, parents became an erased or inert node in the expertise network that gave rise to clinical acumen. Through the discrediting of parental testimony, the clinician was positioned in an obligatory point of passage in this network, from which he could appropriate parental reports, erase the work that went into them, and accumulate them in his hands as scientific capital. Put differently, the “contributory” status of the clinician’s expertise depended on downgrading the
contributory element in parental reports. The “obsessiveness” of the parents was taken as testimony to their overly-intellectual approach to their children, proof of their pathogenic coldness. In this economy of blaming, every argument or observation that a father like Rimland might come up with, could be turned against him and used to establish his culpability and justify his exclusion, just as it served at the same time to buttress the objectivity of the syndrome and the authority of the clinician.

No doubt this was precisely why Rimland was able to create NSAC so quickly. People like Rimland, Jacques May, Oppenheim and Lettick had a common enemy in dynamic psychiatry, and a concrete goal to reinstate parents as credible witnesses so as to project their observations, opinions, and interests into public and professional discourses about autism. Help came from an unexpected corner. Or maybe it was not so unexpected after all. It came from the other side of the emerging parent-practitioner network, from another actor interested in rewiring relations in the network of expertise, another psychologist who turned to parents as allies in the struggle against the domination of the field by psychiatrists and clinicians. Eric Schopler, who earned his doctorate in child developmental psychology at Chicago during the height of Bruno Bettelheim’s popularity there, initially accepted the psychogenic hypothesis but sought to temper it, to achieve some compromise in the impending struggle over the allocation of blame. Over time, however, as he drew closer to the nascent parents’ movement and drew on parents as his allies, he devised a more radical narrative which completely inverted the economy of blame and worth and with it also the relations of expertise.

In 1965, Schopler’s analysis still placed him halfway between the parents and the psychiatric establishment. Like Rimland, he accepted as proven the claim that the parents of autistic children were different from other parents, that they were colder and overly intellectual. But he sought to split the difference between genetic and dynamic explanations. Both cold parenting and constitutional deficit were necessary conditions (Schopler 1965). This analysis explains why in 1966, along with Robert Reichler, Schopler began a five-year pilot study in North Carolina, treating autistic children in collaboration with their parents. Collaboration with mothers, specifically, was necessary because the therapy involved providing adequate tactile stimulation, as well as swinging and patting games. Reichler and Schopler were not the first to collaborate with parents. Several therapies on the margins of the fields of mental retardation and emotional disturbance were experimenting, already in the late 1950s and early 1960s, with various forms of “outsourcing” therapeutic work to parents. Nonetheless, Schopler was the first to introduce this innovation into the treatment of specifically autistic children. The initial impetus for this collaboration, however, was not quite parent-friendly, still guided to some extent by a theory of maternal deficit.

This collaboration with parents, however ambiguous to begin with, proved decisive in shaping Schopler’s trajectory. After a few years had passed, he sought to turn this pilot study into a statewide treatment program known as Treatment and Education of Autistic and related

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5 Bettelheim served as Director of the University of Chicago’s Sonia Shankman Orthogenic School, where he treated autistic children in out-of-home placement using a form of milieu therapy, in which children could form strong attachments with adults within a structured but caring environment. His theory and approach to autism were propounded in his classic 1967 The Empty Fortress, which followed in many respects Kanner’s imagery of the “refrigerator mother.”
Communication-handicapped Children (TEACCH). Schopler needed parents as allies, not only in the conduct of therapy, but in order to establish the credibility of his treatment program, lobby on its behalf, and provide it with extra support against possible attacks from the better endowed psychiatric establishment. While not parents themselves, in 1970 Schopler and Reichler helped parents establish a North Carolina chapter of NSAC, which would play an instrumental role in lobbying the state legislature to fund TEACCH as a statewide autism treatment program, a status it still enjoys today. Reporting the tremendous political success of Frank Warren and other parents and specifying a model of parent-professional collaboration, Schopler put it simply to the NSAC audience, “parents have more political clout than professionals” (Park 1971b, 4). And he wrote a seminal article, drawing on the pilot study experience, to suggest that parents serve as “co-therapists” in autism treatment programs (Schopler and Reichler 1971). The article was first circulated as an NSAC publication, and later published in the new outlet of the combined parent-practitioner expertise network, the Journal of Autism and Childhood Schizophrenia (JACS).

Schopler would later describe the relationship between the parents and professionals as necessarily involving a “common front” when it came to community advocacy (Schopler et al. 1984, 79).

It is hard not to see the new narrative that Schopler devised in the early 1970s as part of this “common front.” Through his collaboration with parents, he too, like Rimland, was now keenly interested in reinstating parents as credible witnesses, and he came up with an effective strategy that involved inverting the whole economy of blame and worth, which, as we saw, allocated clinical acumen to clinicians and pathogenic blame to the parents. First, Schopler now argued that the “obsessive” or “cold” parental behavior described by Kanner was not even a partial cause of autism, but its consequence. He no longer split the difference, but bent back the arrow of harm—which meant he did not need to restrict the scope of the disorder as Rimland did. In the “refrigerator mother” hypothesis, it was the unloving mother (and perhaps father) who harmed the otherwise normal child. Schopler argued the inverse. The parents of autistic children differed from other parents precisely because they lived with an autistic child, a child who did not reinforce the mother’s initial emotional overtures and presented unique behavioral problems. The mother’s difference was not a cause of the child’s difference, but rather an effect of it. In his words, “it was increasingly clear that parents of psychotic children are disorganized in reaction to their disorganized child” (Reichler and Schopler 1976, 355). No less importantly, there was no longer any reason to be suspicious of the trustworthiness of parents’ testimony. It was the best data to be had, as Schopler and Reichler’s (1972) research seemed to indicate.

Second, Schopler (1971b) introduced the notion of “scapegoat.” Why were clinicians unable to see that the arrow of harm went from child to parent and not vice versa? Because, he argued, they were baffled by autism, secretly frustrated and needed to find a scapegoat on which to pin the blame. Thus, he provided a dynamic interpretation of dynamic therapy, downgraded the clinicians to the status of patients, and discredited their testimony. Autism was such a recalcitrant, untreatable disorder, and autistic children were so hard to reach so as to frustrate even the most optimistic and indefatigable play therapist. The behavior of autistic children was confusing. It resisted dynamic explanation and interpretation by even the most inventive psychoanalytic minds. Backed into a corner, unconsciously compensating for their failure and frustration, psychoanalysts blamed parents and promised unrealistic outcomes. The economy of
blame and worth was thus completely inverted, with parent testimony becoming credible and clinical insight discredited.

Schopler’s analyses of parents as impacted by the child rather than vice-versa, as “scapegoats” and “co-therapists,” were immediately picked up and echoed in the parent literature and at parent meetings. This was indeed his intention. No longer the scapegoats of psychoanalysts, parents would now be co-therapists in the establishment of a new regime of treatment and care focused on the individual child’s needs. NSAC, he argued, would provide the umbrella under which they could all come together to develop the critical mass needed to resist psychiatric accusation and develop this new regime of treatment (Schopler 1971b). The articles articulating this new narrative were made available to NSAC members. But not only parents embraced this inversion of the economy of blame and worth; professionals did, too. Schopler’s etiological argument and idiom of parents as co-therapists became a sort of mantra for many professionals at the early NSAC conferences. Even Kanner would write, in a 1971 speech read in absentia, “parents are beginning to be dealt with from the point of view of mutuality, rather than as people standing at one end of the parent-child bipolarity; they have of late been included in therapeutic efforts, not as etiological culprits, nor merely as recipients of drug prescriptions and of thou-shalt and thou-shalt-not rules, but as actively contributing co-therapists” (Kanner, cited in Park 1971a, 7-8).

Finally, Schopler saw scientific specialization as a detriment to the care of disabled children. The team he first assembled in 1966 was purposefully interdisciplinary: Schopler himself along with a child psychiatrist, an arts and crafts teacher, an academic teacher, a social worker, and a music therapist.6 Everyone had to know every aspect of the system and of the child. However, he noted, only one person could truly have such a holistic view – the parent. Thus, he told the audience at the 1970 NSAC meeting, “I advocate that parents must become experts on their own autistic child. They have the most complete and relevant information available, from their daily life with the child. They have the highest motivation for helping their child and maintaining their family equilibrium” (Schopler 1971a, our emphasis). Nothing captures better the new economy of blame and worth and the rewiring of the network of expertise introduced by Schopler, than his pioneering use of a one-way screen to teach parents how to work with their children. Parents would first observe the therapist working with their children through the one-way screen; then they would try to replicate at home what they saw. The one-way screen takes away “the mystique and unfounded authority” of the therapist, Schopler told the audience of the 1971 NSAC conference (Park 1971b). Over time, parents would learn to discriminate between superior and inferior therapeutic performances, would become able to criticize the therapist for failures or mistakes they saw, and would acquire confidence in their own powers as therapists. Schopler testified, indeed, that some parents have come to surpass the therapists who originally trained them. The one-way screen complemented Schopler’s critical narrative. It established a relation of transparency between expert and parent, a new regime of visibility wherein no room for scapegoating remained. Only techniques that therapists had themselves proven to be successful – through meticulous record-taking and one-way screens – would be passed along to parents. The therapists could not entertain unrealistic expectations and therefore would not blame parents for lack of improvement.

The one-way screen also demonstrates that thinking of expertise relationally, as a network, is not the same as thinking of it as merely an “attribution,” the assignment of a label which, by implication, is empty and arbitrary. Here was a concrete device, a physical apparatus that rewired the flow of information within the network of expertise, and thus complemented, was strictly analogous with, Rimland’s E-1 checklists. Neither clinician nor therapist was any longer positioned in an obligatory point of passage in the newly rewired network. Now able to observe the therapists, parents became – as Lovaas (1981) was suggesting they should be – “managers of treatment teams,” the supreme “experts on their own autistic children” whose proximity and entanglement with the child no longer made them culpable, but on the contrary, situated them in the only position into which flowed all the relevant information, and wherein synthesis was possible. These parent-manager-therapists, these small nodes or obligatory points of passage, now turned around and communicated directly with larger nodes or obligatory points of passage represented by parent-activist-researchers like Rimland.

The critique of scapegoating represents, if you will, the negative moment of transformation, the moment when the barriers to the development of the new network of expertise were removed. By the same token, the positive moment was represented by the example set by the early founders of NSAC. Put differently, to transform parental testimony into “virtual witnessing” it was necessary, first, to disentangle it from a too-near relation to that on which it was reporting, where the accusation of “obsessiveness” has placed it. In the second place, however, it had to be re-entangled in it again, but this time armed with a shield of unassailable virtue. Just as gentlemanly virtue resolved the credibility problems of early experimental science, a new style of parenting – autism parenting – served as the model on which rested the assignment of credibility to parental testimony. This is fairly counterintuitive. How could parental love, presumably everything that love is – emotional, subjective, one-sided, and dare we say, “blind” – serve to lend credibility to a claim for objective observation and reporting?

NSAC’s early board members, people like Rimland, Oppenheim, Lettick and Park, modeled this new style of parenting for its members. NSAC provided venues for parents to learn from one another, to experiment with and report back on different therapeutic modalities, creating circuits of knowledge accumulation outside of the professional journals. Rimland openly used his home-baked experiments as evidence, as did Oppenheim, Lettick and Park. In fact, in speeches to audiences at NSAC meetings or in newsletter articles, these parents would often describe their personal experience trying certain methods with their child. They modeled, if you will, a fearless experimental spirit, not unlike the heroism of the pioneers of experimental science.

Through these reports and testimonies, a prototype of the autism parent began to coalesce – experimenting, observing, keeping detailed records, varying treatment regimes in accordance with the evidence, contracting with schools (or creating their own schools) to extend further the experimental and prosthetic environment they created at home, communicating with other parents, providing them with advice and lobbying legislators on their behalf. Individual reports accumulated through multiple channels and iteratively demonstrated the importance of parental

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7 See Silverman 2004 for a more extended account of the role of love in the history of autism in the US.
8 For instance, in a newsletter summary of a conference presentation, Park writes, “the editor [Park herself] tried it on 13-year-old Elly, whose passive, iron negativism, as readers of The Siege know, has always been the most frustrating accompaniment of her condition” (Park 1971b, 6). She reported incremental but “unprecedented” improvement.
observations, cumulatively solidified their credibility, and lent weight to the claims to efficacy of particular therapies.

Parental experimentalism was pragmatically oriented, outcome-driven, and hence at least in principle open to anything that “worked” even if it was marginal to contemporary research programs. This pragmatism was conditional on the fact that newly emerging therapies, like ABA, blurred not only the boundaries between expert and layperson, but also the boundaries between mental retardation and mental illness and hence involved a more flexible, more open-ended definition of the goal of therapy, i.e. of what it would mean for a therapy to “work.” As Chloe Silverman points out, parental acceptance of, experimentation with and advocacy for behavioral methods was a necessary ingredient to their dissemination and eventual ascendancy to the place of the recognized “gold standard” in autism treatment (2004, 172). As we saw earlier, in the mid-1960s, Lovaas’ research program was marginal, bordering on the non-existent – limited to one subject, detached from the advances in cognitive psychology that were to become the dominant paradigm a decade later (Nadesan 2005). Autism itself was limited in scope and derivative. The two, autism and ABA, grew and gathered strength together through an alliance between parents and therapists, a network of expertise which blurred the distinction between parents and therapists, brought them closer, and additionally, translated and coordinated their interests by blurring the distinction between mental illness and mental retardation, thus giving them concrete and achievable goals.

The generosity of expertise

How does the sociology of expertise differ from its predecessors, especially the sociology of the professions, in the way it analyzes the relations of knowledge production and expertise? The preceding analysis, as is true of other stories of “lay expertise,” could easily be read through the prism of “jurisdictional struggles,” which is indeed what Epstein does with ACT-UP. He suggests that activists strove for and were accorded “advisory jurisdiction.” Once you consider this concept of “advisory jurisdiction,” however, which Epstein takes from Abbott, you find buried there a whole can of worms. What is “jurisdiction”? Abbott is emphatic that it means control of actual work. 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.

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, lay expertise, co-production of objects, all the dimensions of expertise-qua-network, are deeply problematic. They are anomalies, yet they are in fact pervasive. Abbott (1988) recognized this problem. If you focus on actual control of work rather than organizational form, he said, you see that professions normally do not have sole jurisdiction over their task area, but share it in various ways with other groups. He listed 5 possible arrangements: subordination, division of labor settlement, intellectual jurisdiction, advisory jurisdiction and client-based division. Ultimately, however, Abbott was attempting to have his
cake and eat it at the same time, as becomes glaringly obvious with “advisory jurisdiction.” 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.

Recall Lovaas’ challenge to his fellow therapists: “we will have to give away our professional skills to lay people, and the sooner the better” (Lovaas 1993, 628). Recall, also, that for Lovaas the only way to generalize his findings and demonstrate the efficacy of his method was to collaborate with parents. One could say that Lovaas was sharing his jurisdiction with parents, perhaps in a “division of labor settlement,” but this is to ignore that Lovaas had no jurisdiction to begin with, that the jurisdiction of ABA, if you will, came into being precisely by virtue of it being a collaborative effort and its object co-produced between therapists and parents. One could suggest that this was a shrewd maneuver to unseat psychiatry, and hence qualifies as jurisdictional struggle a-la-Abbott, but then we would face the same problem that led Epstein to consider the lay experts to have been co-opted. Psychiatry, obviously, was not unseated, more like burrowed under. Neither Lovaas, nor the parents, nor the two of them together, gained a monopoly of expertise. Autism expertise became more shared and networked. Jurisdiction did not change hands, but its boundaries were blurred and all the action was taking place on the fuzzy margins.

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 NSAC’s network of expertise — 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 out in his research on military intelligence — precisely by virtue of their capacity to erase themselves altogether so that not only do they have no jurisdiction, their advice is no longer attributable to them. The advisee of his own accord now sees, does and judges precisely as the expert would.

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 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.

*When the relations in the network of expertise were rearranged, autism itself was transformed*

As the new network of expertise was rewired around the exchange among parents, not only the subject of expertise changed – from the clinician/dynamic therapist to the parent-activist-researcher-therapist – but also its object and goal, not to mention the techniques at its disposal. In this final empirical section, we would like to show how autism itself became something else once the relations within the network of expertise were rearranged. Autism today is no longer what Kanner saw in 1943. It was stretched into a spectrum that incorporates a staggering heterogeneity of symptoms and presentations, shading into mental retardation on the one side – in fact, as we shall see, overlaying a good deal of it and transforming its status – as well as into normalcy on the other, while branching this way or that into various other childhood disorders. We will follow only two strands in what is undoubtedly a much more complex and over-determined process: the implications of adopting behavioral therapy as the currency of exchange among parents, and the branching out of an alternative, agonistic network organized around bio-medical interventions.

To begin with behavioral therapy, if its adoption had a transformative effect on autism itself, it is because at bottom behavioral therapy did not discriminate between autism, childhood schizophrenia, mental retardation or any of the other overlapping labels of the day. The children treated were, in Lovaas’ (1980, xi) opinion, simply developmentally disabled persons who scored low on IQ tests and needed help with language. He was completely agnostic about whether autism existed at all (Lovaas 1979, 317). In other words, the object of behavioral therapy was not autism as a unique and rare illness (as it was for Rimland and Kanner), but a continuum of disability. For his part, Rimland recommended the therapy to other parents because, as he put it, “[t]he method has been shown to work on pigeons, cats, rats, dogs (even with half their brain cut away), mongoloid children, blind deaf-mutes like Hellen Keller” (Rimland 1972, 576). While Rimland still believed at bottom that autism was a unique illness, his successor E-2 form – *The Diagnostic Checklist for Behavior-Disturbed Children* (DCBD; Rimland 1968) – permitted him to make sense of the wide application of behavioral therapy. Parents reported to Rimland how their child received multiple, conflicting diagnoses. One mother was given ten different labels for her child! (Rimland 1971a, 165) A table comparing agreement between diagnosticians, Rimland argued, “shows how arbitrarily these diagnoses have been assigned by presenting an almost random pattern of labeling” (166). Thus the checklist, by rewiring parental reports – now made credible – directly to him, allowed Rimland to question whether existing distinctions between mental illness (childhood schizophrenia), mental retardation and autism had any value. If behavioral interventions were useful for all, and if existing classifications were not to be trusted, it was possible that autism-like conditions were in fact a lot more widespread than previously believed and variable in their presentations and severity. Rimland (1971, 162, n.3) begun calling the sub-clinical diagnoses his checklist produced “autistic-type” rather than “childhood schizophrenic” as he did earlier. And this from the same man who in 1964 declared that “there is an absence of gradations of infantile autism which would create ‘blends’ from normal to severely afflicted” (52). The checklists no longer identified autism as a distinct and rare disorder against its nosological other, but instead traced a continuum of conditions with autism being the paradigmatic form of childhood developmental disorder. Correspondingly, Rimland suggested that NSAC’s slogan be inclusive; their letterhead
and newsletters read “dedicated to the education, welfare and cure of all children with severe disorders of communication and behavior” (Rimland, 1993, 3). The British National Society followed suit a few years later. In 1972, Lorna Wing reported to the crowd at the annual American NSAC meeting and conference, “like the National Society in the USA the British Society has broadened its outlook to cover all children with severe disorders of communication and behavior. This is appropriate because techniques of teaching and managing classically autistic children are successful with other children with severe language problems. It soon becomes clear in the field of communication problems how pointless it is to draw sharp lines between autistic and ‘not really autistic’ in educational practice, even if this distinction is of great theoretical interest. The question to ask about each child is – what are his handicaps, what are his skills and what can we do to help him?” (Wing 1973, 118)

The significance of this diagnostic pragmatism, where therapy drives clinical judgment, was to position autism midway between mental illness – the proper object of psychiatry – and mental retardation – by then, with deinstitutionalization, the proper object of special education. By the same token, the goal of autism expertise was positioned between the ethical economies of medical cure, where presumably there is “magnification of effect of small therapeutic change,” and the “thimbleful” approach associated with mental retardation, where one fills a bucket one thimble at a time (Rimland 1972, 581, 583). Autism became a wedge with which to open up a space between the fields of medicine/psychiatry and special education, a space wherein the boundaries between established and alternative medicine, between psychiatry and psychology, between special education and advocacy, between expert and lay, were constantly blurred.

Schopler seemed to have had a fairly clear vision of this space just as it was opening up. Drawing on a psychiatrist-father’s account (Kysar 1968), he described two camps between which parents were often helplessly caught. One told them the problem was purely medical, the other purely educational. But neither had the full story. The medical camp could not treat autism and the educational camp could not properly classify or predict outcomes (Schopler and Reichler 1976, 351). Instead, Schopler suggested that diagnosis would “not...be definitive, but only...provide a changing, developing language for communication” (349-350). Thereby he was articulating two important dimensions of this space: first, it depended on the cooperation of multiple parties, most importantly requiring experts to negotiate and strike alliances with parents. There was to be no single group of professionals claiming jurisdiction over autism, but a fruitful hybridization among them including parents as “co-therapists” (Schopler and Reichler 1971) or “colleagues” (Lovaas 1993). Second, the blurry boundaries of this space afforded the various parties maximum flexibility to do things they would not have been able to do within more established domains.

Take Lorna Wing, for example. Speaking at the 1972 NSAC conference, she argued that autism can and does occur in concert with mental retardation. Dismissing the common complaint that autistic children scored low only because IQ tests were poorly administered, she argued, “a more cogent criticism is that autistic children do badly because of their perceptual and language handicaps” (Wing 1973, 112). Further, she saw no reason not to extend this argument to the whole of mental retardation. As the earlier quote showed, she argued instead for looking at the specific skills and deficits of each child in order to offer individualized education. “The idea that mental retardation exists as a unitary condition has, on the whole, impeded progress in
education,” she asserted in no uncertain terms (Wing 1973, 113). Note how the hybrid forum, where parents and various experts mixed, permitted her to tread roughshod over multiple disciplinary and institutional boundaries, to dispense with the concept of mental retardation as global deficit, and instead to envision a vast continuum stretching from the semi-normal to children labeled as mental retarded or severely autistic.

Note also how Wing extended the idiom of communication and sensory deficits, which increasingly served to characterize autism, to the whole domain of developmental disabilities infusing treatment with a problematic of radical translation, long before it gained notoriety in contemporary debates about facilitated communication (Biklen 1990), neurodiversity and autistic autobiographies (Hacking 2009). Citing studies in the late 1960s by Hermelin and O’Connor, Wing (1973, 111) wrote: “they came to the conclusion that autistic, meaning ‘socially withdrawn,’ is a completely inappropriate label to apply to these children. They believe (as I do) that autistic children have all the normal emotions appropriate for their mental age, but are severely handicapped in showing them” (1972, 111). In this view, autistic children, and increasingly developmentally disabled children with various diagnoses, have emotions, intentions, preferences and the capacity for choice (which is the hallmark of late modern personhood) like anyone else, but are unable to express these in normative ways, instead emitting a confusing cacophony of autistic behavior and/or idiolect. They cannot represent themselves. They must be represented, or more precisely, translated.

This shows the extent to which the goal of autism expertise has been revised. One was no longer seeking to cure the children, nor merely to enable them to adapt to the everyday demands of “normal” society. The expert, in fact, was working on herself at the same time that she was working with the children, the goal being to establish a channel of communication adapted to the children’s idiosyncratic modes of communication. No less than therapy seeking to break through the child’s isolation and lead him out into the world, it seeks to construct a “prosthetic environment” (Holmes 1990) in which the child can function or even excel and then to extend its perimeter bit by bit. Obviously, this revision of the goal of therapy corresponded to the fact that parents and therapists collaborated with relative equality and the subject of expertise was a parent-therapist-advocate hybrid. Wing, who embodied this hybridity better than any other person, claimed that “many parents who have learned the special language of their own autistic child are of the same opinion [that the handicap is in showing emotions, not having them]” (1973, 111). In other words, the task of radical translation was continuous with the experiences and practices of parents, and while the vocation of autism parenting was modeled after the therapist-experimenter, it was also true that therapy was infused with the parental relation of representing/translating the child. Parents like Clara Claiborne Park modeled this sort of parenting. Through cleverly calibrated social experiments in her home, she devised schemes through which she could break through her daughter’s stoic resistance and involve her in their family life.⁹

⁹ Contemporary therapists, from Stanley Greenspan to Soma Mukhopadhyay, now use videos and training sessions to model this sort of translation work for parents. A session with the child is taped. Then parents and therapists, or apprentice therapists, watch it together and point out moments when it seems the child was trying to communicate. Gradually, one becomes adept at interpreting the child’s idiosyncratic mode of communication (Interview with Barbara Harmon. March 22, 2007).
Let us return now to Rimland and the story of his alternative network. In the mid-1970s, Rimland, who was universally recognized as the global expert on autism, was becoming increasingly marginal from the movement he founded and from mainstream autism research. Rimland’s campaign for megavitamin therapy seemed idiosyncratic and seemed to alienate him from the central thrust of autism research and from the network of expertise that used to be centered around him. Rimland, however, did not go quietly into the night. No doubt his quest for biological therapeutics initially marginalized him and severed at least some of the ties that made his node a central one, yet in characteristic fashion he worked diligently to rewire the network of expertise in another direction, just as he did in the mid-1960s. The research institute he opened in 1967 – originally called the Child Behavior Research Institute (CBRI), then later changed to Autism Research Institute (ARI) – allowed him to maintain close connections to parents and pursue his own agenda, crafting a viable career as an autism research scientist outside academic or government medical institutions.

For the next two and a half decades, Rimland used the ARI as a platform from which to promote alternative biomedical interventions in autism, especially megavitamin therapy, supplements and diets. His first lead on the potential therapeutic use of mega-doses of vitamins came from parents. A nurse who had seen success treating her granddaughter wrote to him. Canadian researchers at the hospital where she worked were experimenting on schizophrenic patients with this treatment, and they were seeing results (Rimland 2006, 17). He was skeptical at first, but “as the letters accumulated,” he told the audience at the 1970 NSAC conference, “I began to get more interested in the reports… As the number of parent-experimenters grew, it began to include more parents whom I knew personally to be highly intelligent and reliable people” (1971b, 57). As with Lovaas’ therapy, Rimland tried it on his son Mark and saw success (Rimland 2006, 22). So he included relevant items on his E-2 checklist (1971b, 58-59). The idea, however, was never able to get traction in mainstream circles.

In 1994, Rimland convened a “think tank” with a physician (Sydney Baker) and a chemist (Jon Pangborn, a fellow parent-researcher) in order to survey the state of the art of biomedical treatments for autism. This lead to the formation of Defeat Autism Now! (DAN!), a coalition of parents, health practitioners and researchers interested in “biomedical treatments” for autism. They held their first meeting the next year and switched to meeting twice annually in 2001. These DAN! meetings are nucleating sites for what Chloe Silverman calls an “experimental community,” “a practical movement based on unlikely alliances and built on experiential knowledge and pragmatic interventions firmly situated within daily life” (2004, 4).

The final chapter in the story of the Rimland network – Rimland himself passed away in 2006 – is the controversy over the link between autism and vaccination. While Rimland played a marginal role in the recent controversies over thimerosal preservative in vaccines and the MMR vaccine (Kirby 2006; Rimland 2003), he began tracking parents’ reports of the ill effects of vaccination already in 1968 and argued early on that autism can be caused by vaccinations. DAN! is certainly one of the major proponents of the theory as well as of detoxification therapies (Edelson and Rimland 2003, 13-18, 20-24, 58-61). More importantly, as we shall argue below, in the vaccination controversy one discerns what is probably Rimland’s most significant legacy.

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10 We refer the reader to Chloe Silverman’s excellent ethnographic and historical work on Rimland and the DAN! movement (Silverman 2004, esp. pp. 350-451; Silverman forthcoming).
namely an alternative network and model of research that is the direct challenger, competitor, enemy and detractor of experimental science’s ethic of “virtual witnessing” – an alternative network which attempts to subordinate virtual witnessing to the ethical vocation of autism parenting.

To follow the story of Rimland, ARI and DAN! is to follow a genealogical thread that does not lead one from the roots to the tree, from heterogeneous origins to a settled identity, but in rhizome-like fashion splits apart, wraps itself around, then spreads out and disappears into the earth. It is to follow not a process of consolidation, but a constant movement of fuzzification weaving in and out of science; back and forth between medicine, alternative medicine and the market; blending therapy, parenting and advocacy; and most importantly, collecting new and heterogeneous symptoms and gluing them to the spectrum until it resembles a tuberous-like agglomeration. If the story of ABA was similar to what Rabeharisoa and Callon (2004) call “co-production,” i.e. where parents and experts (and parent-experts) collaborate to create a new and joint object of discourse, research and intervention, Rimland’s later trajectory, after he split from the mainstream of the parents’ movement, is a story of *dissidence*, of the agonistic production of counter-objects aimed at unseating official dogma. Rimland certainly interpreted his whole career in this light. “Our children are victims of both autism and dogma,” he said and brought as an example the 1960s dogma that behavior modification was futile in autism. The skepticism about his megavitamin therapy he interpreted similarly: “false – just as their view was false that autism was caused by bad mothering” (Edelson and Rimland 2003, 13-18). He styled himself as a renegade parent expert and his arch nemesis was established psychiatry. He had long ago seen, with the psychogenic hypothesis, its “lack of intellectual integrity” and he sought, once again, to create circuits of information exchange that could circumvent it. “Founding the ASA [aka NSAC], as well as the ARI [aka CBRI], were expressions then, as now, of a lack of confidence in the community of professionals who dealt with autism” (Rimland 2006, 17). Not only are they married to dogma and too fond of drugging their patients, but they are simply too slow, too slow to recognize the truth – about the epidemic and about vaccinations – even as it hit them in the face. In 1995 he wrote an editorial in the newsletter he began publishing in 1993, *Autism Research Review International (ARRI)*, replying to explanations of the epidemic that emphasized increased awareness or diagnostic change (Rimland 1995, 3); years later he would remark, “they were wrong – the increase is very real” (Rimland and Edelson 2003, 13-18, 58-61; Rimland 2006, 7, 17-19).

Whom should you trust, if you cannot trust the experts? Parents, he argued, were always ahead of the curve, aware of aspects of autism treatment long before medical doctors: “As the years went on, I continued to find, repeatedly, that parents, especially the mothers, were remarkably effective at identifying treatments that were helpful to their autistic children. They were also very observant in detecting factors that caused their children to become worse” (Rimland 2006, 18). By which he meant vaccinations, wheat and milk (the basis for gluten-free, casein-free [GFCF] diets), etc.

This mistrust of the experts and valorization of parents’ observations was given concrete form in how Rimland rewired his network. While the use of Form E-2 appears to have dwindled significantly after 1980, probably because of its poor performance as a diagnostic tool (Morgan 1988), this had very little effect on Rimland’s network, since he already acted to insulate it from
such competition. He probably never intended Form E-2’s chief use to be as diagnostic tool. He certainly did not design it to be plugged into the network of established psychological and psychiatric expertise, but in order to create his own *alternative* network. Unlike what is customary for other diagnostic scales, Rimland never published the scoring rules for the items on Form E-2. So nobody, including trained diagnosticians, could use Form E-2 without surrendering control over to Rimland. Clinicians were unlikely to agree to such a bargain. The form was intended to be used primarily by parents, who had to send it to ARI once completed. The institute, essentially Rimland and an assistant or two, then sent a score and a brief report back to the parents with an explanation about what the answers said about their child. The circuit of exchange excluded the clinicians and established a direct link between the little parent-experts and the big Parent-Expert, Rimland.

What was Form E-2 designed to do, if its chief use was not as diagnostic tool? It was meant to build a database that would allow Rimland to investigate biological variables and biomedical treatments through a direct exchange with the parents. The newsletter, *ARRI*, was meant to serve a similar function. Rimland routinely advertised in its pages, searching for parents who had experimented with specific therapies. 11 “ARI’s basic premise,” he noted, “is that parents are the best source of information on what helps – and what doesn’t help – autistic children” (Rimland and Edelson 2003, 6). Here was a mechanism, first experimented with by NSAC, that provided an alternative to established experimental science with its double-blind controlled experiments, and that in Rimland’s hands was also turned into a weapon against established experimental science, challenging its ideal of virtual witnessing and seeking to subordinate it to the ethical vocation of autism parenting.

“Normal” experimental medical science typically strives to delink the subjects of experiments from the consumers of results. Between them stand three figures: first, the experimenter, protected from public scrutiny by spatial arrangements as well as by the ethos of “virtual witnessing.” At the laboratory the experimenter aggregates the individual complaints in order to produce black-boxed facts about populations. Second, the regulator, typically a whole network of expertise composed of bureaucrats and expert advisory committees. They assess the credibility of the results produced at laboratories and develop policy recommendations, again with respect to populations, “acceptable risk” levels, etc. (Jasanoff 1991). Third, the doctor who is the direct audience for facts and policy recommendations, and who disaggregates them in the form of diagnosis, medical advice and treatment of individual clients. The whole circuit of exchange relies on the production of what Lakoff (2005) calls “diagnostic liquidity,” the ability to extrapolate from research subjects to similarly diagnosed populations by means of general, context-independent, currency of exchange (the black-boxed fact, the diagnostic scale). When experimental subjects and consumers are unified in the body of the same person – as ACT-UP strove to do with AIDS research (Epstein 1995) – the lonely, secure and fortified existence of the laboratory scientist is breached, the monopoly of medical advice is challenged, and diagnostic liquidity is compromised. The subject/consumer is capable of exercising influence on the very construction of experiments, the selection and assignment of subjects, and the interpretation of results. Rimland’s alternative network strove to “eliminate the middleman” (and his liquid currency): first, by making parents themselves the experimenters; second, by reporting the aggregate results of experiments directly back to parents in a mode useful to them, thereby

11 For an example, see Rimland 1988.
eliminating the need for regulatory science, peer-review, NIH or CDC endorsement, and consultation with a doctor; and third, by creating an experimental community which allowed parents to share their results, to query one another about specific problems, and to utilize the specific expertise of DAN! scientists, physicians and parents.

Edelson and Rimland’s (2003, 7, 84-85) *Treating Autism: Parent Stories of Hope and Success*, published by ARI, is a good example. The book is organized in two parts. In the first, Rimland and other researchers lay out their approach. The second part of the book is composed of thirty-one parent-written accounts. This part is preceded by a table, “Autism Symptom Profile.” The horizontal axis lists the thirty-one kids, each in his or her row, and the vertical axis lists their symptoms. The table is meant to be used by parents reading the book. Instead of reading black-boxed facts about populations, the product of experimental research, they can directly compare their child’s symptoms with those of the children described, find one that is similar, and then leaf forward to the parent-written account to find out what worked, and what did not, in treating this child. They can then attempt to replicate the successful treatment given to this type of child, devising their own experimental design at home, and in due course report back their experiences to other parents. Each of the thirty-one kids is treated as a “prototype,” if you will, a best example representing a certain category (Rosch 1978). The “n of 1” is multiplied, but only to be disaggregated in a mode calculated to be consumed directly by parents. No diagnostic liquidity, more like diagnostic and therapeutic potluck.

Similarly, “every child is biochemically unique,” is – along with “autism is treatable” – perhaps DAN!’s central tenet, and it is clearly the polar opposite of diagnostic liquidity. And it is fast becoming a commonplace in autism discourse. Respectable clinicians subscribe to the oft-repeated mantra that “no two children on the spectrum are alike,” “there is no one-size-fits-all autism treatment,” “if you know one autistic child, you know one autistic child,” etc. (Siegel 1996, 14). One must experiment with what might work, determine what does and does not, and eventually over time tailor a regimen to the child’s particular “biochemical profile.” DAN! suggests a whole regimen of tests and supplementation and a cottage industry has grown up around providing these services, including for-profit manufacturers like Kirkman Laboratories, with whom Rimland had longstanding relations. Rimland gives suggestions for making one’s experimentation more scientific. For instance, he advises against alerting the child’s therapists of changes in the child’s medical treatment. This way, the professionals will take accurate and unbiased records of the child’s progress; “each child thus becomes, in effect, a subject in a double-blind ‘mini-study’” (2006, 25; see also p. 10).

Not only is this diagnostic and therapeutic potluck a viable alternative to the circuit of generalized exchange of experimental science, it attempts to subordinate the latter to its own purposes by submitting the results of experimental science directly to parental scrutiny. Rimland published the contact information of researchers in *ARRI*, so parents could find and contact professionals researching children like their own. In short, he was attempting to produce something similar to the “autism symptom profile” table, only with experimenters and their peer-reviewed articles playing the role of the parent-written accounts of individual profiles and

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12 However, a famous DAN! doctor, Jacquelyn McCandless, herself the grandparent of an autistic child, is sure to note as a disclaimer that these treatments will help most but “not all” autistic children (McCandless et al. 2003, 68; her emphasis). See Silverman 2004 (chapter five) for a discussion of DAN!’s “autism is treatable” campaign.
Instead of experimenters reporting black-boxed facts, they have parents peering over their shoulder, looking at half-baked controversies before they are black-boxed and hardened into facts (Latour 1987). Instead of experimenters producing knowledge about populations, to be disaggregated by the doctor middleman, their reports are taken as particular accounts about particular types of children, and judged with respect to the particular needs and experiences of parents. Similarly, experimental science’s “gold standard” of the randomized, double-blind, placebo-controlled trial is subordinated to what McCandless calls the “silver standard” of clinical efficacy. The circuit of exchange among parents short-circuits the lengthy procedures of experimental science. Most therapies first come to meet the “silver standard” of clinical efficacy long before they are ready to be submitted to the “gold standard” test of double-blind placebo trials. By that time, however, says McCandless, it is unethical to withhold treatment (McCandless et al. 2003, 117). Ultimately this leads down the path of questioning the integrity and credibility of the laboratory scientist, especially the government-funded one, who no longer enjoys a secure and lonely existence, and no longer can claim the presumption of virtual and virtuous witnessing.

The idea that parents should try whatever is available, and try it fast, further accentuates the agonistic network’s resistance to the randomized controlled trial method. McCandless articulates a heavily morally-laden image for autism parents, showing how the ethical vocation of autism parenting claims to subordinate the one of “virtual witnessing”:

Imagine you are standing on a pier. Your child is drowning (he or she has developed autism or one of the other autism spectrum disorders) and you can’t swim. You desperately look for help or a life preserver (a physician or treatments that might work). You find a rope tied to the pier (special diets, nutritional supplements, anti-fungal/anti-viral treatments, Secretin, Chelation for heavy metal toxicity – all of which you have learned are safe and help many of these children). However, authorities warn you not to use it because it has not been proven that the rope is strong enough (the treatment option has not received final approval by “authorities” who are waiting for reports of completed scientific studies appearing in peer-reviewed journals). Meanwhile your child is drowning (exhibiting autistic/ASD symptoms) (McCandless et al. 2003, 137).

If you were a parent, you would not wait, she tells her readers. You would grab that life preserver and give it a hurl. The worst that could happen is that it breaks after your child is pulled a bit closer to shore. In the context of this “try everything” philosophy, where can one find a treatment-naïve child? Parents of autistic children are therapy omnivores. Green et al. (2006) have found that parents of autistic children in the US, Canada and Australia use 111 different types of treatments, with a mean number of current treatments per child of seven. In this context, it is plainly impossible to create uncontaminated control groups for research, unless of very young infants, or perhaps children in countries with less developed medical systems. Thus, the agonistic network and the ethical vocation of autism parenting render randomized controlled trials problematic.

Underlying Rimland’s search for biochemical treatments was his earlier understanding of autism as a distinct and rare illness. He claimed, among other things, that children with high E-2 scores responded better to a multivitamin treatment regimen than did those with lower scores, and that
children with high scores also had high levels of serotonin outflow, i.e. that while Form E-2 and other diagnostic tools constructed autism as a spectrum, there was a distinct biological disorder camouflaged by the spectrum, which corresponded to classical Kanner-type autism and which Form E-2 gradually allowed him to identify (Rimland 1971a).

One notes, therefore, a curious reversal of roles. In the 1960s, established medicine and psychiatry conceived of autism along an illness model and strove to cure it. NSAC, on the other hand, of which Rimland was a co-founder, linked autism to the model of lifelong, incurable “developmental disabilities,” which like mental retardation were to be treated with behavioral and educational approaches seeking not to cure but to “habilitate.” Over time, the network of expertise represented by NSAC extended itself into the medical establishment, and now collaborates with it to co-produce autism as a life-long disability for which the treatment “gold standard” is a combination of non-medical therapies, and the goal is not cure but equipping children with habits, skills and prostheses that would allow them to better manage their disability. Conversely, the radical faction of the parent movement led by Rimland has gravitated towards something that looks like an illness model of autism, and their “fringe” treatment protocol is composed precisely of medical and semi-medical treatments aiming at cure or “recovery” (we shall see below the significance of this term). The opposition between these two treatment approaches is, as is true of most such oppositions, more radical on paper than in practice. In reality, many parents use the medical and semi-medical therapies (vitamins, diets, anti-fungals, chelation) with an eye to amelioration of specific symptoms rather than cure. Parents have no qualms about combining medical, behavioral and educational therapies. Whatever works. Indeed, Edelson and Rimland (2003, 9) themselves observe that the biomedical treatments they recommend “are most effective when combined with intensive educational approaches...they ‘open the children up’ to the experience of learning. We strongly encourage parents to pursue educational and biomedical treatments concurrently to maximize their children's progress.” Yet, the opposition is significant for two inter-related reasons: first, the reversal of roles – i.e. the agnostic network stakes a position counter to established medicine precisely by deploying the medical model of illness and cure – operates to constantly blur the lines between “mainstream” and “alternative” medicine, between science and pseudo-science, between medicine and home-baked cures. Consequently, and this is the second point, it serves to maintain the “pull” which suspends autism between illness and retardation, stretching the spectrum between them and permitting it to collect ever more new symptoms in its fold.

Why is the boundary blurred between mainstream and alternative medicine? First, because the individuals involved in the agonistic network often come from within the medical establishment, yet they are also parents (or grandparents) of autistic children. Rimland formed DAN! in collaboration with John Pangborn, an academic chemist and Sidney Baker, a Yale pediatrician. They were soon joined by Jaquelyn McCandless, a psychiatrist with an autistic granddaughter. DAN! maintains a list of doctors who are affiliated with it, and whom parents are encouraged to consult. To be affiliated with DAN! means that one is willing to consult the DAN! protocol, titled *Biomedical Assessment Options for Children with Autism and Related Problems*. To be clear, it means that one is recommending diets, vitamin treatments, chelation and a variety of other treatments that the medical establishment considers “fringe,” “unproven,” “alternative” or even positively dangerous. It may mean that one agrees with parents that vaccines have caused their child’s autism, which is precisely where the battle lines are drawn between established
medicine and its enemies. Yet, many physicians with autistic children join DAN! So many, in fact, that there was even a special panel for them in the 2002 DAN! conference. Among the parent-contributors to Rimland’s volume there were also a pediatrician and a neurologist (Edelson and Rimland 2003, 19, 24, 123-126).

Second, the boundary is blurred because Rimland and DAN! actively deconstruct the distinction between mainstream and alternative medicine. I do not promote alternative medicine, says Rimland, but “intelligent medicine” (Rimland 2003, 3), and he traces the lineage of his method to Linus Pauling and “orthomolecular medicine.” Jacquelyn McCandless, M.D., writing in a more sociological mood, provides a social constructionist analysis of the distinction between mainstream and alternative (2003, 116-117). The “popular press makes a big deal about the dichotomy,” but an historical view reveals fluid movement in both directions. What was mainstream can become alternative – and vice versa.13 Why is the use of vitamins “alternative,” they ask, and the prescription of drugs by psychiatrists “mainstream”? Rimland (Edelson and Rimland 2003, 40-42) deconstructs the history of DMG’s (Dimethylglycine) introduction to the American market to show that pure historical contingency and regulatory fiat led to its being classified as a food supplement and not a drug. This is good, he says, since it does not require prescription, it is cheap, and parents can use it freely. But it does not mean therefore that its use is “alternative” and not “medical.” He and McCandless target the complex of relations between big pharmaceutical companies and established medicine and accuse the latter of “simply drugging…autistic children [instead of]…effectively treating them” (ibid.). Appendix C in Edelson and Rimland’s book (2003, 364-365) contains the results of a “Treatment Effectiveness Survey” of parents. It compares diets and supplements with drugs to show that the former are safer and more effective. Very early on Rimland developed a close working relationship with Kirkman Laboratories through which he helped foster an alternative supply channel that could respond to increasing demand for multivitamins, DMG, digestive enzymes and other supplements. A small but thriving market emerged, alongside and intermeshed with the market for pharmaceuticals.

Most importantly, however, it is the therapy itself, and the ethical model of intervention embedded in it, rather than what is said about it or who practices it, which serves to open up a whole fuzzy zone between established and alternative medicine. The moderate success he experienced experimenting with vitamin therapy led Rimland to introduce a small, but crucial modification in his understanding of autism. There was a “remarkably wide range of benefits…better eye contact, less self-stimulatory behavior, more interest in the world…fewer tantrums, more speech, and in general the children became more normal, although they were not completely cured.” He concluded, therefore, that “autism…is in many cases a vitamin B6 dependency syndrome” (Edelson and Rimland 2003, 26-28). Conceiving of autism as “dependency” is, of course, not inconsistent with also thinking that it is a genetic illness, but it allows Rimland to activate a principle of fuzzification within the medical model of illness and cure. Dependency, as Mariana Valverde (1998) notes, is a morally laden metaphor, or more precisely it is a metaphor that links a moral image with a somatic condition, it conjures a whole world of hybrid entities, from “diseases of the will” (Ribot 1910) to speaking bodies – if they get

13 For instance, dietary interventions, previously considered “alternative,” are increasingly being recommended by “mainstream” practitioners as a treatment for autistic children, especially children suffering from gastro-intestinal issues.
hyperactive, says Rimland (Edelson and Rimland 2003, 49-50), its “their way of telling you they need more folic acid.” It is a highly flexible term. On the one hand, it can mean something like “deficit,” which is the closest to the illness model – find the correct dose of the missing substance and the child is cured. On the other hand, it may mean a permanent condition and identity, closest to the model of disability and retardation – “once an alcoholic, always an alcoholic.” In between the two, there is the image of a body out of homeostasis, either poisoned from the outside as in the vaccine theory or GFCF diets, or depleted on the inside as in the image of Children with Starving Brains (McCandless 2003), or the two images are neatly combined in the notion of “autoimmune disorder,” a body that poisons itself.

To dependency and its brethren disruptions of homeostasis corresponds not cure per se, but “recovery.” Rimland explains that vitamin therapy is “not a cure,” but it makes a “big...difference” (Edelson and Rimland 2003, 36). The title of the aforementioned 2003 book was changed for the 2006 second printing from Treating Autistic Children to Recovering Autistic Children, to reflect what Rimland claimed was a major breakthrough, an explosion of “recovered” children; an impossibility had become “commonplace” (Rimland 2006, 6). The use of the term “recovery” is significant for several reasons: first, in the hierarchy of treatment outcomes it strays just below the level of “cure.” It connotes a longer and less certain, potentially reversible process, but unlike “improvement” (below it in the hierarchy) it is still within, or on the fringes, of the restorative. Second, it resonates with the image a child, hidden behind the disorder who is snatched back, “recovered,” by his parents from the “empty fortress” of autism. It interpellates, therefore, a particular moral mode of parental agency. Indeed, among the parents writing short reports in Edelson and Rimland’s book is Karyn Seroussi, who in no uncertain terms explains how “we rescued our child from autism” (ibid, 299-305; see also Seroussi 2000).

Recovery, thirdly, is an individual and unique process. A disease may be cured, but only a unique individual can recover. As Rimland (Edelson and Rimland 2003, 20-24) explains, there is no way of knowing if vitamins or diets will help, because “every child is very different from every other child.” Recovery, therefore, is something that only parents, experimenting on their child, day in and day out, can achieve. Recall the “Autism Symptom Profile.” Nobody can tell you what is the right amount (of vitamins, supplements, detoxification), says Rimland, not only because every child is different, but most importantly because “dependency” and its brethren disruptions of homeostasis (deficit, poisoning, auto-immune disorder, etc.) imply recovery as delicate fine-tuning of chemical balance which can only be achieved in individual cases and only through trial and error. What is involved is not a science of illness but an art of healing (Edelson and Rimland 2003, 48-49; Gadamer 1996). The result is a fuzzy zone where established and alternative medicines are indistinguishable, and the expertise of doctors is not superior, but additive to the expertise of parents. It would seem, perhaps, that it is still possible to distinguish between the two, since one prescribes drugs and the other administers vitamins/nutrients/food supplements14, “a drug acts by blocking or interfering with a natural body process, while a nutrient permits or enhances these processes” (Edelson and Rimland 2003, 48). The very nebulousness of this distinction, however, demonstrates the principle of fuzzification activated by the model of dependency/recovery. To begin with, the distinction between “blocking” and “permitting” is altogether fuzzy. Is slowing down the reuptake of a neurotransmitter, for

14 However, in cases where parents research pharmaceuticals and seek prescriptions for their children, the boundary is blurred further still.
example, blocking or permitting a natural process? Isn’t dependency itself a natural process? Moreover, since the target of treatment – recovery – is imagined as enhancing a natural process tending towards homeostasis, then any adverse result can be interpreted as simply failing to achieve the correct balance (and therefore to require a bit more or a bit less of the same).

As a term signaling an art of healing, “recovery” is not only individualizing but holistic. The whole individual is treated through a sort of therapeutic bricolage, not just his or her autism. Or more precisely, as Chloe Silverman (forthcoming) argues, a variety of symptoms, previously understood as “co-morbidities” and not essential to the diagnostic prototype of autism (if they were mentioned at all) – most importantly gut dysfunction, but also yeast infection, allergies, sleep disorder, eating disorder, aggression, “self-stimming,” seizures, hyperactivity – are now translated differently, as relevant symptoms of autism-qua-disruption of homeostasis, as legitimate objects of treatment the reduction of which signals recovery. They can be treated by “correcting body chemistry” (Edelson and Rimland 2003, 52), thereby indicating that one is acting on the underlying imbalance responsible for the total condition of the child. Thus, we agree with Silverman when she argues that this “leads to a blurring or reframing of the diagnostic category of autism itself” (Silverman forthcoming). In particular, the fuzzy zone between established and alternative medicines serves as a channel through which a variety of late-onset or “regressive” types of autism was attached to the spectrum and came to modify its prototype, a process which is still continuing.

Take, for example, the case of anti-fungals. A small number of children exhibiting autistic symptoms were reported recovered following treatment with nystatin, an anti-fungal. Rimland (Edelson and Rimland 2003, 55-57) argues that this shows something he had been arguing already in 1966, that between five to ten percent of children diagnosed with autism, especially the late-onset or regressive type, are actually suffering from a yeast infection, with the candida fungus. They were given antibiotics for ear infections at an early age. The antibiotics disrupted the delicate balance in which the fungus is kept in check by the body and by other competing microorganisms. The fungus began to release toxins which cause pain, disrupt the immune system, and also affect the brain. The children regressed. Treatment with anti-fungals assists the body to restore balance and therefore the children recover. He says that physicians are skeptical of this theory, but mothers have flocked to the therapy. Very few physicians will prescribe anti-fungals, but the result is uncertain because there are many different strains of the fungus, hence one needs to experiment also with diets (that will reduce nutrients favored by the fungus) or even move to a drier climate, he counsels. One can see how in this case the blurred boundaries between established and alternative medicines allowed a whole set of new symptoms (especially allergies, repeated infections, etc.), and with them whole new populations, to infiltrate the spectrum and swell the ranks of the epidemic.

Spaces between fields

After having distinguished the sociology of expertise from the sociology of professions, and the study of expertise from the study of experts, we would like in what remains to recombine them, or more precisely to reflect on the method we used in this study of autism expertise, which clearly took us back and forth between expertise and the experts of various kinds. This method moves seamlessly and borrows from both Bourdieu’s field theory and Latour’s ANT. From our
point of view, these two theoretical and methodological stances are complementary. Field theory, we argue, equips us with the best set of tools with which to analyze experts and their interests. This is what the concept of “field” is designed to do: to overcome the false polarity between interpreting actors’ actions as motivated by material self-interest (externalist explanation), as against construing their actions to be disinterested and guided only by a search for the “truth,” “beauty,” “authenticity,” etc. (internalist explanation) (Bourdieu 1985). What does it mean to “overcome” this polarity? Nothing grandiose or mysterious. For a long time now, everybody knew that this is a false polarity. Certainly Mannheim (1936) already did. “Overcoming” it, however, means to invent a device that permits you to interpret the motivations for action without at any point occupying the Machiavellian position of the master strategist, the prince, who has full knowledge of his interests. Neither is it permissible to explain action as “disinterested” – disinterest, explains Bourdieu (1975), is nothing but orientation to “a system of specific...interests which implies relative indifference to the ordinary objects of interest – money, honors, etc.” – nor is it permissible to explain it as a rational calculation of interests. Bourdieu’s solution, therefore, is to speak of actors as investing in the particular illusio of the field into which they enter, and thus, even as they pursue their interests they are never masters of their own strategies. That this is what the concept of field is designed to do, can be seen, for example, by the quote from Bachelard that Bourdieu (1975) chose as motto for his article about the scientific field: “The training of the scientific mind is not only a reform of ordinary knowledge, but also a conversion of interests.”

ANT, by comparison, equips us with a set of tools for analyzing the modality and content of the practices that constitute expertise. This is what the concept of “network” is designed to do: to overcome the false polarity between explaining these practices as reflections and adjustments to an objective reality (internalist explanation), as against explaining them by reference to social forces that thereby “socially construct” reality (externalist explanation). (Latour 1988, 218) What does it mean to overcome this polarity? To invent a device that permits to analyze expertise without at any point occupying the post-hoc position of full knowledge or Truth: neither is it permissible to explain the fruits of expertise as “discoveries,” i.e. that they are somehow verified by our post-hoc knowledge of what is the truth about reality; nor is it permissible to explain them as “social constructions,” thereby assigning “society” with somehow greater reality and agency, and trivializing experts’ actual, practical grappling with recalcitrant materiality. Latour’s (1987, 86-90, 121-132) solution, therefore, is to speak of the expert or scientist neither as discovering, nor as constructing, but as “recruiting,” “mobilizing” and striking alliances with the objects of their study. This means perforce that the boundaries between nature and society, reality and discourse, science and politics, are thick and fuzzy, and that they are crisscrossed by the networks that experts weave in order to recruit allies and convey statements.

This can explain why, aside from the vagaries of the French intellectual field, these two thinkers and their camps were so dismissive of each other. Indeed, we would argue that each is strongest precisely where the other is weakest, and each privileges precisely what the other discounts. Their omissions are, in a sense, symmetrical, and for this very reason, while their theories lead in different directions, they could also be complementary. In order to account for the formation and mutual adjustment of actors’ interests, one would need to keep the concept of field. In order to account for the formation and mutual differentiation of practices, however, one would also need to keep the concept of network straddling the boundaries between fields. If these two concepts are to sit comfortably together, several provisions must be made: the theory of fields must shed
the all-knowing narrator’s voice that Bourdieu adopts, speaking from the point of view of scientific objectivity; actor-network theory, correspondingly, would need to part with the Machiavellian narrator’s voice that Latour adopts, speaking from the point of view of the prince. Field theory will also have to make room for some of the insights contained in the concept of “translation” (particularly the point that distance – that objective measure par excellence in Bourdieu’s theory – could be modified by interpretation); but at the same time, actor-network theory would have to grant more of a reality to the phenomenon of a bounded sphere – which it tends to treat as mere appearance – and accept that to explain it one would need sturdier mechanisms than flimsy attribution. We are not sure we would be able to convince both sides of the value of these complementarities. So for the moment we are content to suggest a truce, encapsulated in the idea of a “space between fields.”

Clearly this idea has been key to our treatment of autism expertise. Autism therapies, we argued, fuzzify the boundary between mental retardation and mental illness and thereby balance autism between the fields of medicine/psychiatry and special education. The network of autism expertise we described cuts across established jurisdictions, spanning also the lay-expert divide. Later developments, especially Rimland’s alternative network, positioned autism expertise so it hybridizes medicine with alternative medicine, therapy with activism, and public health with a for-profit market. Figure 1 presents a schematic diagram of the space of autism therapies as 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 a sociology of expertise. The first one follows immediately from the considerations developed above. It is conceived as a meeting place for the methods of field analysis and ANT. In Bourdieu’s visual depictions, fields typically are represented as nesting within each other, and ultimately within the field of power. This captures important theoretical arguments about the forces determining the trajectories of actors, but it leaves a nagging question: how to decide whether a particular set of activities is “inside” or “outside” the field? And what makes a certain field “economic” and another “cultural” or “literary”? These questions are no longer about the interests of actors, but about the modality of activities. We intend the imagery of a space between fields to deal with these questions. We think of this space as a thick boundary zone secreted by fields as an inevitable part of their functioning; as a fuzzy frontier zone that both separates and connects fields.

Take, for example, Michel Callon’s (1998; see also Mackenzie et al 2007) claim that economics performs markets. What does it mean? Callon points out the crucial role played in economic life by the identification of externalities, i.e. certain objects or activities are identified as factors or consequences of the economic process, but are nonetheless qualified as incalculable, outside the economy. This exclusion from the economic is, however, also a form of inclusion, because the identification of an externality is, of course, the first move in disentangling it from its entanglements in social contexts, a first move towards bringing it within calculation, i.e. re-entangling it within a network of market devices. Callon thus suggests a far more robust and interesting concept of boundary work than Gieryn’s (1999). “Boundary work” is real work. It is
not just attribution, the rhetorical assignment of labels thus drawing a “fine line,” but akin to the work of a border patrol: mapping the terrain, establishing connections to what lies beyond it, transacting with the other side on a daily basis. Moreover, Callon notes that this work of identification, disentanglement and re-entanglement is typically performed by economists: not the elite of the profession, university-based, highly abstract mathematical modelers, but practical, hands-on, rank-and-file economists in government and corporations, who continuously identify externalities and devise means of measuring, calculating and disentangling them. One gets the sense, therefore, that the constitution of the economy, of things economic, takes place in a boundary space between the economic field, the bureaucratic field and the academic field, by actors who have a foot in each of these, but by the same token are also somewhat marginal to each of these. What they do at once connects the economic field with the academic and bureaucratic ones – since to identify, measure, calculate and disentangle externalities, i.e. to qualify things as “economic,” to produce the specific modality of activities in the field, is a collaborative effort that requires the participation of scientists, politicians, administrators, etc. – and yet reproduces their separation since the very product of this collaborative cross-boundary effort is the qualification of things and activities a “economic” or “non-economic.”

Similarly, to constitute autism as a psychiatric object, as we saw, Kanner and other child psychiatrists needed to draw on the work of parents, who recorded, recollected and recounted the child’s development in minute details, yet “mother-blaming” meant that the parents’ contribution was obscured and controlled. Correspondingly, the status of autism within the psychiatric field was a bit like an “externality.” It was identified as a possibility, there were hints that perhaps it underlay many apparent forms of retardation or mental illness, but the consensus was that it was rare. Both Kanner and Rimland agreed on this point. The parents’ revolt, in collaboration with the subordinate professions of psychology, occupational therapy, speech therapy, etc., removed the mechanism that limited the production of autism as an object of expertise, and created the conditions for its rapid multiplication two decades later, yet did so by decidedly locating the production of this object – which psychiatry adopted – at an interstitial space between medicine, special education and advocacy, a space of cross-field collaboration between parents, therapists, psychologists, psychiatrists, activists, educators and for-profit entrepreneurs.

Our final example of the idea of a space between fields as a fuzzy frontier zone is evidence-based-medicine (EBM) and the placebo effect. EBM is clearly an attempt to draw a strict boundary between “real” medicine – scientific, biological – and phony or irrational practices. It does so with rigorous studies utilizing the gold standard of double-blind randomized controlled experiments. Inevitably, however, even in the best-run studies there is a sizable group of patients who get better even without treatment, or when given something they think is treatment but for which there is no evidence that it works. In order to produce clear-cut results and a “fine line” between real medicine and the rest, EBM segregates these cases as “placebo effect.” Alternative medical practitioners then claim the effect as their own, as evidence that the method of double-blind randomized controlled experiments is not applicable to test the efficacy of their therapies, which draw on what EBM itself admits it cannot explain. Hence, the attempt to create

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15 Andrew Lakoff (2007) describes how pharmaceutical companies, unable to dampen the placebo effect, attempt to induce it in order to segregate placebo-responders and find “the right patient for the drug.” In so doing, they unravel the logic of brain-based disease specificity that underpins their endeavor in the first place.
a sharp line of exclusion ends up having produced a grey fuzzy zone where the boundary between alternative and real medicine is not at all clear-cut.

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, just as we saw Rimland and DAN! combined medicine and alternative medicine into a potent (and marketable) package. 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 begin to emerge.

Interstitiality plays a similar role in Medvetz’s (2009) 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.\textsuperscript{16}

\textsuperscript{16} 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.
In conclusion, and as a way of fleshing out this argument, we detail some of the characteristics of
the space between fields understood as a space of opportunity and not a field-in-the-making:

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. In a sense, the space between fields is the exact
conceptual inverse of “field.” In Bourdieu’s (1975) analysis of the scientific field, one
gets the sense that the higher the entry barrier, the higher the autonomy of the field, and
consequently its “fieldness.”

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. Similarly,
the under-regulation of the space of autism expertise permitted Rimland to blur the lines
between medicine and alternative medicine and combine them freely, and it permitted
parents of autistic children to become “therapy omnivores” experimenting with at least
101 different therapies and medications (Green 2006).

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. In
the case of autism expertise, there is clearly a lot of money to be made by contracting
with parents and marketing therapy directly to them, but there are also less tangible
rewards such as the revaluation in the status of relatively marginal occupations such as
occupational therapy, speech therapy, behavior modification, as they assiduously adopt
many of the trappings of medicine – they evaluate (diagnose), prescribe and treat
developmental disabilities.

4. **Weak institutionalization**: these marginal actors engage perhaps in field building. In the
case of terrorism expertise, for example, the academics produce dubious “chronologies”
of “terrorism events” and amass no less dubious bibliographies of terrorism-related
knowledge, they even write dubious retrospective histories of the field’s emergence, and
of course they collaborate with state officials to create institutes and centers of terrorism
studies, etc. Yet, typically these attempts founder and unlike the field of think tanks, no
clear division of labor and hierarchy of worth emerges. 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 other, 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. A sort of “gold rush” image. Let’s consider the development of human
behavioral genetics, studied by Aaron Panofsky (2006). This sub-discipline could be
described as lying on the margins of the scientific field. It is considered by many
practitioners of animal behavioral genetics to be only quasi-scientific, because the sort of controlled experiments performed with generations upon generations of laboratory mice are obviously impossible to perform with humans. On the other hand, this is a research site that generates a great deal of media attention, and into which flows a great deal of funding (from foundations and the government). For this reason, this space that lies between the natural and human sciences, as well as between academia, the media and the foundations field, is the site of constant incursions by actors who are relatively marginal within their own fields, or whose disciplines occupy a marginal or liminal position – failed animal behavioral geneticists, psychologists, professors of education, publicists and science writers. Each perceives this in-between space as the site where combinations disallowed in their own field (for example, identical twin studies from psychology combined with DNA mapping and socio-economic stratification data), are permitted. Each perceives this in-between space as a site of opportunity, where resources could be accumulated and then possibly converted into an improved position within their own home field. For this reason, Latour’s Machiavellian language may indeed apply better to describe the actions of these actors, but only because of how their habitus was shaped by their specific trajectory within given fields. Marginality is the mother of invention and improvisation, of seeing a value in heterodox combinations and in exploiting fuzzy frontiers.

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. Recall Rimland’s deconstruction of the distinction between a drug and a food supplement. Since food supplements are not regulated as stringently as drugs, says Rimland, I am not going to struggle and make the point that my multi-vitamin/DMG therapy is in fact the same as providing a medical drug. I am going to stay in this fuzzy zone of food supplements, where I can still claim that autism is like an illness treated by “bio-medical” therapy, but where I can also create direct contacts with consumers and commercial producers “underneath” the jurisdiction held by medical doctors.
References


