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Chapter 2 'Which Way Does It Go between You Two?' Modes of Interdisciplinary Intervention

Interdisciplinarity is often imagined as a specific mode of practice, one that works by carefully separating different sets of expertise from one another (e.g. wherein a philosopher does conceptual work, and a neuroscientist simply collects data). This chapter, by contrast, radically opens up the range of legitimate modes of interdisciplinary practice. It analyses, and works through, three different, entangled, modes of interdisciplinary practice that have undergirded our own research – co-authorship, co-experimentation, and co-organization. The chapter ends with a call to retire formulations of interdisciplinarity that delineate sets of expertise and practice from the outset.

Felicity and a scientific collaborator are giving a joint presentation at a conference on social consequences that arise from the neurosciences. They have a carefully choreographed set of slides, which demand them passing the ball – as it were – a number of times, one to the other, so as to ensure that each presents different kinds of empirical and conceptual data. Each ventriloquizes some of the scientific figures who feature in their analysis of the emergence of the field of resting state fMRI; at times, one completes the slide that the other has started out presenting. It's perhaps difficult, if you didn't know either of them, to discern who is the social scientist, and who the neuroscientist. Then it's time for questions. Someone in the front raises her hand, looks from one to the other with a somewhat baffled expression, and says, "Which way does it go between you two?" She looks at Felicity. "I mean, do you study him?" She looks at Felicity's collaborator. "Or … ?"

Introduction

What does a meaningful interdisciplinary intervention involving the humanities, social sciences, and neurosciences look like? Certainly we have been in spaces, and read papers, where there has been an assumption that it means someone not trained in the neurosciences participating in a project where some measure of brain activity is deployed (e.g. with philosophers parsing the concepts at stake such that phenomenological distinctions are made empirically testable; see, for example, Farrer and Frith 2002; Gallagher 2000). In this chapter, we will suggest that such configurations should not be seen as the only way in which 'interdisciplinarity' across the social sciences, neurosciences, and humanities might happen – not least because such models tend to leave unperturbed the usual epistemological divisions of labour between different disciplines.

Some of our own interdisciplinary and collaborative interventions have indeed oscillated around the fMRI scanner (see Fitzgerald et al. 2014a); others have involved in-depth work on the history of concepts (see Callard and Margulies 2014); some have taken the form of shared workshop organizing, or paper planning, or grant writing or advice seeking; still others have involved simply *conversing*. In what follows, we are going to shy away from providing any definitive answer *vis-à-vis* what a 'good' mode of interdisciplinary intervention might look like. Instead, as an attempt to broaden some assumptions about what constitutes an interdisciplinary moment in the first place, we outline three modes through which we have intervened within the capacious field of interdisciplinary neuroscience. Some of our attempts worked well; others, quite a bit less so; at least one other ongoing effort could still go either way. In another case, a good collaboration got off the ground only after the two of us had moved out of the way. But the key is that, in each case, something took place *outside of* a model in which representatives from a number of disciplines lay their knowledge next to one another without any serious entanglement in the methods, logics and principles of those other disciplines. The three modes we will consider here are: (1) co-authoring; (2) co-experimenting; (3) co-organizing. Only one features the work of a brain scanner, although even here the constellation of experimental subjects, experimenters, and technologies – alongside the interpretations of what they and we collectively produced – is rather different from what is usually attempted via an interdisciplinary intervention.

Let us stress two things in advance: (i) These modes do not constitute a full palette of options. Notably absent here, for example, is the 'ELSI' mode of intervention (i.e. in which social scientists and others gather around a scientific

project in order to do the 'Ethical, Legal, and Social Implications' part of it). This is left out not because we think it unimportant. We sidestep it partly because we have never inhabited this position, but also (and not unrelatedly) because we retain a strong suspicion that, in its careful parsing of duties and expertise, the ELSI mode is not only a weak form of interdisciplinarity, but is in some ways precisely the opposite of what we always taken *interdisciplinary collaboration* to consist of (see Balmer et al. 2012 for a lament; see Strathern and Rockhill 2013 for an interesting analysis). (ii) We make no claim that we have initiated these modes: in multiple ways, we have built on and learnt from others' practice. Additionally, we have a strong sense – which has been further consolidated by numerous people asking us for specific details of how we ended up doing the kind of projects that we have become involved in – that it might be useful to provide some more informal accounts of, and reflections on, our own experience of various collaborative modes, which might allow readers new to such formations to peer behind the curtain of interdisciplinary collaborative work. In short, we want the chapter, first, to augment and organize the *variety* of methods, and modes of collaboration, that any of us might undertake when conducting interdisciplinary work involving the mind and brain; and, second, to offer some honest reflections on how, and for what purposes, we and our collaborators initiated and then persisted in carrying out interventions across those modes, despite frequent frustrations and setbacks.

Co-authoring

A collaborative bond – like any relationship, we are tempted to say – can come from anywhere, and be pursued in any variety of ways. For one of us (FC), the most important collaborative relationship, with a neuroscientist, emerged from the discovery of a range of shared intellectual affinities during a residential workshop. Immediately on leaving the workshop, the two of them began a correspondence, based around a call for articles that had just emerged, in which they considered how they might work together to make an interesting contribution. What is striking, in retrospect – and here our account is of course written from the perspective of FC – is that their shared interest in the call was an attempt to ensure ongoing *social* contact; notably, the implicit offer from one of them to the other to co-write an article was only one of various mechanisms that could have achieved such a purpose. Even more striking in the formation of this collaboration was that there was no identified research problem that the proposed co-authorship was being called upon to fill; this runs counter to many formal encomia for interdisciplinarity, in which different disciplines are often imagined as coming together to answer particular (already identified and identifiable) problems. Nor was there any legible reason why someone working in neuroanatomy and someone who was at that point based in a health services department and working on patients' and family members' conceptualizations of genetics (FC) would feel that the other was especially well placed to enrich his or her line of research inquiry. Indeed, the only real shared point of contact was some interest in a field (psychoanalysis) that was marginal to both of their primary research priorities (as well as marginal to the practices of 'science' *in toto*).

They ended up circling around the neuroscientist's field of research – the field of resting state fMRI (which measures low-frequency fluctuations to investigate spontaneous activity in the brain and hence to understand the functional architecture of the brain). In the course of the next two years, these collaborators wove tight webs of social and intellectual interest around that point of conjuncture. Without significant external grant support, they co-wrote papers, and co-presented talks, that investigated the intriguing and heterogeneous origins of resting state fMRI research (for an internalist history of this field, see Snyder and Raichle 2012). For FC, this collaboration comprised an extraordinary, new opportunity to read and think – and then write – with a neuroscientist whose generosity and willingness to open up, and explain to her, the theoretical and methodological complexities of his field allowed her to become proficient in a wholly new corner of the neurosciences. FC conjectured that this collaboration, for the neuroscientist, potentially offered the opportunity to go back to identify and (re)interpret some of the foundational papers within his field in conversation with someone 'coming from the outside'.

As the collaboration developed, these two researchers developed a paper on the current status of the 'default mode network' (DMN) (widely understood to be the set of brain regions that are engaged when people are in a 'resting state' – i.e. left to themselves in a scanner, with no explicit task instruction). In it, they suggested that the DMN would not necessarily remain at the centre of future research that attempted to understand the relationship between brain dynamics and the mechanisms of thought. In other words, they raised the possibility that the current, apparently foundational, concept would not necessarily remain at the centre of its field. Co-authoring a publication such as this opens different possibilities, and carries different risks, depending on one's disciplinary location. For a cultural geographer and historian of science (FC) to question the ontological consistency and durability of a scientific object is one thing – in many respects, it is one of the indispensable tools of her trade. However, it is often far easier to do such a thing after a scientific object *has* disintegrated and decayed – when such disintegration is clearly recognizable as one looks back at a time now past – rather than become deeply immersed within the *current* scientific and

technological practices of a field. FC speculated that for a neuroscientist, who might well be, in certain respects, importantly committed to that same scientific object (not only for his own research, but perhaps also for his lab members' stability, as well as for the longevity and collegiality of collaborative relationships with other scientists), publishing such an intervention potentially carried some risks. There is, of course, a long tradition of debate and critique by scientists who are internal to their field (see Poldrack 2006; Vul et al. 2009 for two different kinds of critique within the field of fMRI). But in such cases, critique is most commonly routed through (and is legitimized around) questions of methodology; research that gets to the heart of the historical, theoretical, and conceptual frameworks of the science are, we suggest, rarer. FC recalls that both authors expended significant energy, over the course of many drafts, adjusting the wording so that it carried enough punch for the cultural theorist and historian, and enough nuance for the neuroscientist: in particular, the many (many) annotated drafts of the article reveal, for her at least, ongoing, shared anxieties about the politics of word choice; about how, or whether, normative claims might be made; and about if and how to include sentences that pointed to the future of the field. And yet despite these risks and efforts, co-authoring brought, for FC, significant pleasures. For a cultural theorist engaged with the history of science, the opportunity to co-author and publish in a scientific journal shifted her position of annunciation from the usual disciplinary one of external commentary to one of audible authority *within* a field. She surmised that for a neuroscientist, the opportunity to participate in meta-conjecture, and to intervene in the overarching frameworks and potential ontologies at work in one's own field, might offer a somewhat unusual, and yet potentially productive, intellectual position *vis-à-vis* one of the scientific objects that is the focus of much of his research.

What collaborators cannot control, of course, is how they get read, or how they will be subsequently positioned against one another. At one stage in this process, FC followed up a shared invitation, to an interdisciplinary event, to ask whether it would be of interest to hear a linked presentation (from both FC and her neuroscientific collaborator) that included some reflection on how this cross-disciplinary partnership had developed. She received a warm reply – but one that warned her of the preponderance of 'cutting-edge neuroscience' at the event, and thus likely scepticism about a talk that would go 'too deeply into the genealogy of a field'. Some historical context was welcome, she was told – but perhaps not too much. The key was not to work too hard at 'reaching across the divide' (between the sciences and the humanities), but simply to present cutting-edge (neuroscientific) work. Her respondent lamented, additionally, that humanists were sometimes 'quite lazy' and not up to speed with the science (this writer, we note, came from the humanities).

There are many important points embedded here – not a few of which we are inclined to agree with. But what is especially fascinating is the way in which a concern about an ostentatious reaching across the divide ended up not only reinscribing an unequal dyad of science (cuttingedge, worthy of admiration, and excitement) and the humanities (that which provides social and historical context, not cutting-edge), but also found humanists to be potentially at fault in relation to a practice that neuroscientists need not imagine themselves as required to undertake. At the same time, the interdisciplinary was positioned as being at risk of weakness and lack of rigour in relation to the virility of 'cutting-edge' science. Notably, there was no articulated need for either the professional neuroscientists or the humanities people in the audience to hear any 'cutting-edge' social science or humanities scholarship. This exchange thus became a moment that revealed to FC how her own way of envisaging her intellectual relationship with her neuroscientific collaborator, as well as the relations between their respective disciplines, was quite at odds with many other, better established spaces of/for interdisciplinary exchange. Her and her collaborator's jointly authored manuscripts marked, for her, an attempt, by contrast, to disrupt the assumptions that undergirded the usual models of co-authorship and co-presentation – not least by situating the context and progress of the collaboration *itself* at the heart of any shared inquiry, and refusing to disentangle such context from the cutting-edge of the research object in question.

Co-experimenting

In 2010, one of us (DF) attended a workshop on social neuroscience, organized by the European Neuroscience and Society Network, at the University of Vienna. What attracted DF to this event was that it promised not only a *workshop about*, but also a shared *experiment in*, social neuroscience: everyone who came to the event had to precirculate a design for a real experiment, drawing on whatever account of experimental logics was available to him or her. On arrival, attendees were split into teams with a loosely equal disciplinary mix; they attended some workshops on experimental design, as well as on the actual physical logic of the scanner – and were then more-or-less left to their own devices: the task was to settle on one question between them, and design an experimental paradigm around it – with the organizers holding out the inducement that one group would be supported actually to carry out its experiment. In the group that DF was placed in, by far the most compelling proposal was provided by Melissa

Littlefield, then an Assistant (now Associate) Professor of English at the University of Illinois at Urbana-Champaign. The idea was centred on the neuroscience of lie detection: Littlefield's proposal was to turn the experimental logic of fMRI lie detection away from its fascination with 'deception', and to redirect it towards the nuances of 'truth'. Given that the dynamics of truth-telling and deception are fundamentally social in similar ways – deception is, after all, always a relational disposition – the collaborators designed an experiment that would take a brain measure when participants told a 'socially-awkward' truth, in order to see how brain regions associated with that truth would map on to regions already associated with deception (see e.g. Langleben et al. 2005).

The subsequent experiment (this proposal was indeed the winning one, and the real experiment was facilitated by the Interacting Minds Centre at Aarhus University, Denmark) combined insights from the humanities, social sciences, and neurosciences: the team created a complex ecological situation (involving team-building, the generation of in-groups and out-groups, and dynamics of trust and competition) designed to induce participants, in the scanner, to tell a difficult truth about a teammate (see Fitzgerald et al. 2014a for a longer account of this experiment, from the perspective of science and technology studies; see also Littlefield et al. 2014; see especially Littlefield 2011 for a broader analysis of the science of lie detection). The hypothesis was that a neurobiological measure taken at this point would show activation, at the moment just prior to enunciation, in intensely 'social' brain regions previously associated with deception. At the heart of this experimental design was a suggestion that the fMRI scanner might have difficulty distinguishing between a 'truth' and a 'lie' – because what it means to be deceived, or to confess, is buried within a whole series of fraught social relationships, memories, feelings, sensations, and so on, whose entanglements are not well indexed by the hemodynamic response at stake in fMRI.

Simultaneously, the experiment was an attempt to open up how the adjective 'social' was being mobilized within social neuroscience – and the ways in which some deeply complex social dynamics had become sequestered within a series of physiological effects that happened to be available to brain-imaging technologies. But the form that this perspective took was not historical or social analysis (cf. a number of the essays in Pickersgill and van Keulen 2011); nor was it a critique of either the object or the method of fMRI lie detection; nor still was it a critical theorization of the psychological literature on social cognition. Instead, it was a real experiment (Fitzgerald et al. 2014b). More precisely, it was an experiment that recruited the implicit logics of fMRI *lie* detection into a scenario in which precisely the opposite dynamic was at stake, *viz.* an awkward *truth*. The experiment used the modes of visualization and interpretation, as well as the conceptual leaps that had *made deception visible* in particular neurobiological locations, in order to bring the intersubjectivity of truth to light. In this sense, a critique was made on the basis of an expertise – that is, Littlefield's literary and historical research – that is not always available to people involved in neuroimaging studies. More importantly, this expertise, and the critical intervention it afforded, was articulated exclusively through the internal logics of fMRI brain-imaging. It would not be enough to assert that deception depended on context: the point was to co-design and run an experiment that would work *with* the potent and important reductive force of the scanner, in order to bring something like 'context' into view.

But co-experiment is not without its problems. More recently, DF and FC discussed this research at an interdisciplinary workshop concerned with 'critical neuroscience' – a term signifying the desire for a form of neuroscientific investigation more attentive to its own biases, limitations, and political entanglements, and thereby contributing to a more 'critical' (and potent) analysis of social and political relations (Slaby and Choudhury 2011). The lie detection project was discussed as one instance of what we had by then come to understand as an 'experimental entanglement' – a term that we had introduced (Fitzgerald and Callard 2015) to expand the kinds of interventions that might be understood as 'critical' (see also Viney, Callard, and Woods 2015). A prominent neuroscientist, who happened to be in the room, responded to our presentation by saying (we paraphrase): 'What's critical about that? This is just normal science.' In an important sense, this is very true. Such a question draws our attention to what it might mean to intervene through experimental modalities that do not, in fact, depart from the constrained logics of 'normal science'. What does it mean for the interdisciplinarian to garb herself in experimental procedures, such that her critical impulses might have the hope of passing in intellectual institutions that privilege certain kinds of bioscientific reasoning? (For a canonical account of some of the fraught literatures and histories that we are invoking here, see Chapter 6 in Butler 1993.) And beyond such concerns, there are also of course cognitive neuroscientific experiments that do not consider themselves to be explicitly interdisciplinary, and which have mobilized the resources of 'normal science' to exert forms of critique. The famous 'dead salmon' experiment, for example, critiqued the use of poor statistical corrections that generated false positives in fMRI studies, by running a 'normal' experiment that showed brain activity in a dead Atlantic salmon (Bennett, Wolford, and Miller 2009; see Margulies 2011 for a discussion). More recently, Amir Raz and colleagues drew together cognitive neuroscientific

methods and techniques from magic to demonstrate experimentally the risk of 'neurohype' (Ali, Lifshitz, and Raz 2014). There remains an open question of how – if at all – interdisciplinary experimentation (of the kind that involves the interpretive social sciences and humanities) differs from modes of practice internal to the field of cognitive neuroscience.

Co-organizing

> *We are at an interdisciplinary networking event, with about 40 other people from various disciplines. Stand up; introduce yourself; give a thumbnail sketch of your strengths; explain what you're looking for; sit back down. The day drags a bit. One neuroscientist, Simone Kühn (SK), in her allotted five minutes, spoke as an expert designer of experimental neuroscientific paradigms; she was at the workshop because she was potentially interested in collaborating with people from the humanities and social sciences who could help her investigate, assess, and interpret subjective experience. Cognitive neuroscience, she noted, was incredibly good at establishing objective responses to tasks designed by the experimenter, but still in its infancy* vis-à-vis *acquiring subjective data that captured the experimental subject's responses to the experimental paradigm* (Jack and Roepstorff 2002). *Her pitch piqued our interest: after all, literatures within sociology, the history of the human sciences and geography have long reflected on the 'problem' of eliciting subjective experience and of adequately conceptualizing the relationality between an experimenter, an experimental subject, and the social and technological artefacts amongst them* (Despret 2004; Morawski 2007). *By the end of the workshop, the three of us were part of a larger group attempting to get funding for a new interdisciplinary programme of research on 'intersubjectivity'.*

With SK, we devoted significant time to planning, curating, and organizing a workshop on that question. We juxtaposed researchers from different fields in the same panel, and forced each to respond to the same provocation. (For example, the provocation 'Where is the introspective self?' was addressed by Russ Hurlburt, the psychologist who has worked for many years to develop a painstaking and extraordinary psychological method called Descriptive Experience Sampling (Hurlburt and Akhter 2006), and the philosopher and critical neuroscientist Jan Slaby. For further details, see Callard et al. 2012a.) And we borrowed from our own experience of European Neuroscience and Society Network (ENSN) events to ensure that the last day was devoted to developing an actual protocol for some kind of empirical study, rather than the event simply remaining at the level of discursive exchange. Such attempts can always fail; indeed this is what happens more often than not. But our intention was to push beyond the endless 'dialogues' that beset interdisciplinary workshops – and that frequently end up simply reinstalling the 'inter-', as well as consolidating existing positions – even at the risk of failure.

Beyond the actual curation of the event itself, some of the value of co-organization as a mode of interdisciplinary intervention was revealed in offshoots from, or tangents to, the main business of the workshop. In the course of the event, for example, SK and her neuroscientist colleague Elisa Filevich presented initial findings from an innovative protocol that they had designed and tested to explore the vexed construct of 'free will'. Working to prise open decades of black-boxed studies that have kept constructs out of the reach of the experimental subject, and that have evinced no interest in what the experimental subject interprets the study to be about, their paradigm focused instead on the subtleties of what it feels like to choose, in which the experimental subject herself adjudicated which decisions had felt more or less 'freely' selected (Filevich et al. 2013). We subsequently co-authored a short commentary on this study for the journal that published their article (Callard and Fitzgerald 2014), in which we reinscribed this nuanced experiment within some other (social scientific, historical, and psychological) literatures that had tried to think through the fraught relationship of the experimenter to her subject. SK's and Filevich's imaginative experimental intervention opened up for us new possibilities for how we might, in the future, 'torque' existing experimental paradigms that are focused on other phenomena, and that also employ complex intersubjective relations between the experimenter and her subject.

At the same event, an entirely new collaborative endeavour emerged. As we noted above, FC, DF, and SK had already ensured that Russ Hurlburt was part of the workshop; this was because SK had mentioned her interest in his work at the beginning of the exchanges between the three of them. On the final day of the event, likely collaborators were more-or-less forced – via highly curated groups – into extended interactions with one another, and instructed to return with the outline of a collaborative experiment. Most of these conversations did not produce anything very concrete (and this was fine) – but a fascinating interdisciplinary collaboration, drawing together very different traditions of

psychological and neuroscientific experimentation on introspection, *did* emerge from the conversation between SK, Russ Hurlburt, Charles Fernyhough (CF), and others (Kühn et al. 2014). For FC and DF, the key mode of interdisciplinary intervention, here, was to use this workshop as a vehicle to ensure that likely collaborators were pushed to work with one another – and, critically, for the two of them to get out of the way once this curatorial work was done, and they were no longer required. Interdisciplinarity, here, was located first in the curatorial labour of bringing SK's, CF's, and Russ Hurlburt's different, psychologically-rooted research traditions together (and we are absolutely committed to the view that the joining of heterogeneous psychological and neuroscientific traditions, rooted in profoundly different epistemological histories, is an intensely interdisciplinary endeavour), and generating the space for an experimental conversation to take place. But there was also a significant interdisciplinary intervention in knowing *when to get out of the way*: a more traditionally-minded mode would have insisted that, for this to be a truly interdisciplinary effort, it must include some 'perspective' from the social sciences and/or humanities. Such a view not only flattens the historical and internal multiplicity of such a capacious discipline as psychology, but also makes invisible the vital interdisciplinary labour of simply bringing things together. We are insistent, by contrast, that the generation of space, the curatorial labour of choreographing encounters, and then the willingness to know when your disciplinary perspective is not necessarily adding anything important, remain crucial, if underappreciated, ways to do interdisciplinary collaboration.

So ... which way does it go between you two?

The answer, of course, is that in interdisciplinary collaborations it can go many ways. We have here offered three modes – but no doubt this barely scratches the surface. What we wish to stress is that the tired distinction between whether one labours 'on' or 'in' another discipline, or the desire to pin down who is more likely to be the 'object' of study within a collaboration, is surely long past the age of retirement. Let us dispense, then, with the two-pronged formulation whereby the role of the social scientist/humanities scholar is *either* to critique neuroscientific concepts 'from the outside', as it were, *or* to provide a contextualization of the empirical data that have been acquired by the neuroscientists themselves. Let us, by the same notion, do away with the mirrored image of the neuroscientist either as a sort of crude empiricist, waiting patiently for the philosopher to sort out her concepts, *or* as an external intellectual imperialist, blithely washing sociological histories away with her all-conquering brain machine.

If we understand interdisciplinary interventions, instead, as ways of marking, folding, and perturbing the existing order of the world, then interdisciplinary collaborations might take place in an untold number of ways. These could include written arguments, the performative power of heterogeneous experimental situations, and elaborations of the discursive and paradigmatic constraints within which researchers from both the sciences and the humanities/interpretive social sciences work. They might also include shared applications, the joint labour of workshop organization, shared senses of joy and rage at the decisions of grant committees – as well as the whole, unspoken panoply of ways for thinking, together, about the intersections of culture, society, environment, mind, and brain.

Notes & Queries: 2

Q: Can you give a quick list of other possible ways, however seemingly small or minor, of actually making an interdisciplinary intervention?

A: There is no easily circumscribable list of things that would count as interdisciplinary interventions. Rather, we consider it more helpful to think about an intervention as the act of *stepping in* to affect a course of action, or an issue. This can be through a variety of means – and could mean stepping in and thereby *preventing* something from happening. Many acts might, then, be worthy of consideration, for example:

- acting as the reader of a draft written ?? by someone in another field;
- carefully setting up respondents at workshops or shared meetings;
- watching an experiment that someone in another discipline is conducting (given that the act of observation can often influence what happens next in predictable and unpredictable ways);
- disrupting what you perceive to be the deathly curation of an interdisciplinary event that seems to be able to do no more than encourage 'dialogue between the disciplines';

- putting your foot down about being tagged 'the (one) neuroscientist' or 'the (one) social scientist' at an interdisciplinary event;
- attending more carefully to what might be described as 'interdisciplinary' differences that are working themselves out within an apparently disciplinary space.

This list could go on and on (and for some other examples, see the 'project shorts' in Fernyhough, Woods, and Patton 2015). What we want to emphasize, here, is not the specifics of those acts enumerated above, but rather our general starting point. We want to depart from thinking at the level of a 'discipline' – which then rapidly moves into interdisciplinary situations that bring one discipline 'into conversation' with another discipline – and attend to other scales and logics at which something different might take place. A mode of interdisciplinary intervention, in this view, is constituted by some perturbation in the neatly bounded landscapes of intellectual labour; it is the work of actually refusing those boundaries in the first place.

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