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DISSENSUS IN SCIENCE AS A FACT AND AS A NORM

ABSTRACT

Dissensus – incompatible theories co-existing for an extended period – has been traditionally viewed as a rare accident or else as a stage in the progression of scientific inquiry that is bound to terminate: on these views, consensus is the stable state to which science tends. Following Miriam Solomon's reconsideration of dissensus as rationally on par with consensus, it is argued that the persistence of dissensus is compatible with the pull towards the resolution of inconsistency. While the social turn in philosophy of science goes some way towards relieving the tension, the key move is to go one step beyond and to distinguish between the social-psychological level, where the pull towards resolution is in force, and the public level, where it does not operate directly and can be counter-balanced by other mechanisms. An added benefit of this approach is to provide a more realistic picture of the scientists' predicament, at both the individual and communal levels, who face not only Nature but public science that stand in need of interpretation. Finally, it is suggested that dissensus enhances the ability of public science to quickly overcome impasses.

Science tracks truth: it aims at knowing, for any proposition of interest P, whether P is true or whether not-P is. As they cannot be both true, science cannot accept for any length of time a situation where some scientists hold P and others non-P. In other words, science cannot countenance dissensus, and if and when dissensus in fact arises, science seeks to eliminate it by putting more effort into finding out which of P or not-P is true. In fancier terms, "consensus is the *telos* of science" (Alan Richardson[1]). In fact, or so Richardson argues (in Peirce's and his own name), inquiry would not *make sense* if consensus were not the *goal* of science. Without going that far, we are at least strongly inclined to think that inquiry being constitutively the search for truth, and truth being indivisible, inquiry necessarily leads either, in case of failure, to ignorance or error, or, in case of success, to consensus.

    Against this view, arguably the majority view in philosophy of science, Miriam Solomon and a few others before her have argued that dissensus is not the

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1   Alan Richardson, "Solomon's Science without Conscience, or, On the Coherence of Epistemic Newtonianism", in: *Perspectives on Science* 16, 2008, pp. 246-252.

delendum of science[2]: although, all things being equal, consensus might be preferable to dissensus, this preference should not trump other norms; in fact, dissensus may well be rationally preferable to consensus on some occasions, and not merely for instrumental reasons, as a means to facilitate the realization of science's ultimate goal.

Now there is a way of accommodating the view of science as truth-tracking and consensus as a desirable (or ultimately inevitable) state of affairs with the view that dissensus is acceptable (or inevitable) in science. It involves tampering with the concept of truth, making it relative, partial, perspectival etc. (so that P and not-P can both be true). This – which I'll label 'relativism'– is a path that I will not follow in this paper. Instead, I aim to show that of the three posits:

1. Science aims at truth.
2. Dissensus is normatively acceptable.
3. Relativism is false.

none needs to be discharged.

I will rely on two moves to establish the consistency of the three: (1) I will distinguish between subjective and public scientific knowledge (in a sense which I will explain); (2) I will attempt to discredit the picture picture of science. On the way, I will demolish another picture, in which the scientific process is seen as a confrontation between the scientist, alone or as part of a team, and Nature. Finally, I will briefly suggest a reason for thinking of dissensus not just as a fact of life (be it a normative fact of life), but so to speak as a guardian angel of science.

1. THE CURRENT VIEWS OF DISSENSUS

Dissensus (as I will be using the word) refers to a state of scientific knowledge where two (or more) incompatible theories co-exist. Dissensus implies non-agreement of course, but not any case of non-agreement counts as dissensus, which requires a certain depth and a certain permanence; dissensus does not necessarily go together with criticism, which requires acting argumentatively against a different view; let alone with controversy, which involves two camps combating one another for an extended period; or even with dissent, if dissent is understood as focusing on a view different from one's own and rejecting it.

There are three main accounts of dissensus which are currently visible, although the first and second tend to recede under the pressure of the more recent third.

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2   Miriam Solomon, *Social Empiricism.* Cambridge, MA: MIT Press 2001; Miriam Solomon, "Norms of Dissent", in: Damien Fennell (Ed.), Contingency and Dissent in Science Project Discussion Paper Series. Technical Report 0908, CPNSS, LSE, 2008.

i. Dissensus as short-lived accidents

As Marcello Pera writes at the beginning of his contribution to an important, fairly recent volume on scientific controversies, "the Founding Fathers[3] were deeply attached to the idea that science is uncontroversial"[4]. The Fathers' View, as I'll call it, in a nutshell, is this:

> Dissensus happens only as brief episodes–epistemic accidents at it were–caused by error or blocked access to the full set of available evidence. They belong to the context of discovery and leave no enduring mark on science. Science would remain essentially unchanged and intelligible if dissensus disappeared from the history (actual processes) of science.

ii. Dissensus as the permanent state of science

In the Fathers' View, there is a reason why consensus is the necessary end-state of any scientific inquiry: the scientific method demonstrably tracks truth, and scientists are professionally committed to following the scientific method. In 'post-Legendary' philosophy of science, both assumptions are put in question, and through the lens of constructivist/historicist sociology of scientific knowledge, consensus in science, far from being the normal outcome of inquiry, appears as socially imposed discipline on a state of permanent cacophony – it is nothing but 'procedurally enforced consensus'[5] which bears no relation to the rational convergence to truth which the Fathers envisioned. The Bad Sons' View can be summarized thus:

> Dissensus is the natural state of science in the making, and yields only to political force exerted by one camp, deploying a mix of rhetorical and institutional maneuvers, either at the time of discovery (science in the making) or at the time of evangelical reconstructions (at the stage of pedagogy, for both lay and professional audiences).

iii. Dissensus as essential but transitory stages of the growth of scientific knowledge

As is well-known, the pendulum has swung back and the main trends in contemporary philosophy of science seek to integrate the justified objections to the 'Legendary' picture of science and in particular to the Fathers' View of dissensus as accidental, while holding onto a rationalistic conception of science. This had led to a rehabilitation of consensus, conjoined with a novel respect for dissensus. The resulting Good Sons' View goes something like this:

> Dissensus is a fact of life. It reflects the imbalance between the complexity of the world and our cognitive abilities: science is hard and makes consensus a protracted process. It is therefore an enduring feature of developing science,

3   The philosophers-scientists who gave birth to modern science.

4   Peter Machamer, Marcello Pera, and Aristides Baltas, *Scientific Controversies*. New York, Oxford: Oxford University Press 2000, p. 50.

5   Stephen Fuller, "The Elusiveness of Consensus in Science", in *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 1986, Vol. 2, 1986, p. 111.

which leaves a trace in its crystallized forms. It also has instrumental value (as an antidote to dogmatism and error, as a heuristic device …). Still, what scientists aim for, and eventually achieve, is consensus, either of the Fathers' type or pluralistic.

This view seems to point to two possible resolutions of the problem, which I want to acknowledge while denying that they actually settle the matter:

(i) *The instrumental value of dissensus.* The dialectical tradition stemming from Aristotle gives critical dialogue a crucial (in fact in some versions constitutive) role in rational inquiry. Criticism is the key methodological maxim in Popper's critical rationalism. But dissensus is not criticism, as we saw: it is a state of enduring plurality of views. Still, dissensus can, and often does lead to sustained criticism; it is thus an important, perhaps indispensable error-correcting mechanism that keeps science on the right track. There is yet a second line of thought, which runs from Mill to Feyerabend,[6] which grants dissenting opinions a protective role against not only error but dogmatism, smugness and tyranny: even a correct theory gets to lose some of its intellectual and practical virtues if it remains unchallenged. I have no objection to these views, except if they are presented as final: there is much more to dissensus than being the midwife of consensus or the guardian of honest thinking.

(ii) *Pluralistic consensus.* It might seem that an extra step is taken when we accept the possibility that several theories applicable to the same realm of nature can be simultaneously correct without being inter-reducible. But this is getting the dialectic back to front: scientific pluralism is, in the main (there are conflicting versions sharing the label), a purported solution by elimination to the problem of dissensus. The thought is that different perspectives on a complex domain yield different theories, all of which can be true or faithful representations at the same time: by letting go of the Fathers' demand for a unique all-encompassing theory, we can re-interpret cases of apparent enduring dissensus as two-tier consensus: consensus on each of the co-existing theories, and consensus on the legitimacy of their co-existence. No trace of dissensus left. Now there may well exist among conceptions of scientific pluralism some that *also* countenance genuine dissensus – such a theory would in fact have my own preference. But the mere move to pluralism is not enough to settle the issue of dissensus; in fact, it tends to push it under the rug.

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6   Elizabeth Lloyd, "Feyerabend, Mill, and Pluralism", in: *Philosophy of Science*, 64, Suppl., 1997, pp. 396-407.

2. The (merely) social turn

Up to this point, I have tacitly assumed an individualist perspective, that of the Fathers. It should be mentioned in this respect that individualism does not force the acceptance of the Consensus Thesis. Nicholas Rescher, for one, seems to have anticipated some of Solomon's conclusions: he defends the idea that there is nothing intrinsically rational about seeking consensus, and moreover that dissensus is the inevitable consequence of the fact that "equally rational inquirers placed in different experiential situations will come up with variant answers to the question of how things are. […] Reason is exercised from particular and differentiated places"[7].

Rescher shares with the pro-consensus classical thinkers a purely individualist conception of the epistemic situation in science, which may be summarized thus:

> [Ind] The Scientist inspects Nature and establishes that Nature has feature F.

In the last twenty years however, philosophy of science has taken a social turn and now tends to favor the following picture:

> [Com] The Community of scientists inspects Nature and establishes that Nature has feature F.

which can be construed in several rather different ways, actively explored in social epistemology. First, the Fathers' understanding, which is also Popper's and Rescher's and antedates the social turn, places the collective moment in the beginning, during the discovery phase: tasks may be parceled out for greater efficiency, and/or critical exchanges between participants can contribute to the emergence of truth. From then on, a 'Principle of Individual Recapitulation' brings the result of this collective labor under individual jurisdiction: some member of the collective at least can, in principle, gather the entire evidence and master all the inferential steps leading to the desired conclusion, in effect severing all dependency links with her collaborators. As far as I can see, this reading of [Com] presents with respect to [Ind] no difference that would be relevant to the issue of consensus.

By contrast, the social turn is really taken when the Principle of Individual Recapitulation is cast away: social in its genesis, scientific knowledge remains social at all stages. Our principle is then open to two main interpretations, according to our understanding of the nature of the relevant group:

> [C-Com] The Concrete Community of scientists inspects Nature and establishes that Nature has feature F.

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7   I confess to not having read Rescher's book: *Pluralism: Against the Demand for Consensus*, Oxford: The Clarendon Press 1993. This is a gloss by a reviewer: David Archard, "Review: The Morality of Pluralism, by J Kekes, and Pluralism, by N. Rescher", in: *The Philosophical Quarterly* 45, 1995, pp. 400-403.

> [A-Com] The Abstract Community of scientists inspects Nature and establishes that Nature has feature F.

How best to characterize and contrast concrete and abstract communities is a topic of lively discussion in social epistemology. The first roughly correspond to a collective of active individuals with interpersonal connections, possibly somewhat remote or indirect, but with a sense of constituting a self with specific abilities, competencies and responsibilities.[8] The second sort lacks these connections and this sense of self. Of course there is a continuum ranging from, say, a pair of scientists co-signing a paper to the (fuzzy) set of all scientists since Galileo, and where to usefully draw any sort of line depends on what issue one is trying to address. Regarding consensus and dissensus, we want to understand what it means for there to be a consensus or not, given that, or insofar as we have ceased to believe that individuals are the sole loci of scientific beliefs. In the Fathers' paradise, it was straightforward to attribute consensus in a community, anywhere along the concrete/abstract line: there was a consensus on F whenever every member, or almost every member of the community believed (or was justified in believing) F. One way to go in the present setting might be as follows: (1) Define a concrete community as one to which there are good reasons to attribute beliefs of its own. It might be, for example, a group with such a rich background of shared beliefs and practices that, given a feature F which the group believes to belong to Nature, any of its members can convincingly fight back any challenge to the effect that she doesn't justifiably believe that Nature has feature F, even as she might rely on the rest of the group to fill out the meaning of F or the reasons to think that Nature has F. (2) Now take any abstract community C in science with a concern for F, and say that there is a consensus regarding F if every concrete community within C has a collective justified belief that Nature has F.

There are two connected problems with this proposal. The first is that of dealing with the dead. Do we include them or not? It's hard to include them in concrete communities, as long at least as we think of them roughly along the lines suggested above: dead people are not usually regarded as active, communicating, committed, responsible entities. On the other hand, by leaving them out we deprive concrete communities of important resources: it's not only during the discovery phase that most if not all scientists rest on the shoulders of the giants who preceded them. This dilemma leads us directly to another problem, which is actually the same problem in a wider perspective. It is the problem of acquired scientific knowledge. When Sara ~~is investigating~~ the beer situation, she is no doubt relying on background knowledge regarding the nature of beer, the geometry of bottles, the medium-term stability of refrigerators, the habits of the household and so forth. This

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8   Among the most discussed proposals, Margaret Gilbert's 'plural subjects', of which established groups are an instance, fill the bill of my concrete communities; see her *Sociality and Responsibility*, Lanham: Rowman & Littlefield 2000. I remain uncommitted here to this or any other specific proposal.

knowledge is committed to memory, and in this and similar cases it is in fact integrated in a set of mostly automatic behavioral patterns in such a way that at no time does Sara (the person, not her neurons) have the need to refer to it; in other cases Sara will also consciously refer to stored factual knowledge. But it is still always Sara, carrying her own resources inside her, who is facing the world and asking whether there is any beer left at home.

On an unreflective reading of [Com], when a scientist inquires whether Nature has feature F, like Sara he has assimilated all the required background knowledge so that it is now 'ingrained' , 'incorporated' in his memory and his thought patterns: he approaches Nature the way Holmes approaches the case of the speckled band, with his bare cognitive and sensory organs. Now I believe this is already utterly off track, but I will let it be for a moment and go to the other side of the social turn.

Let us first consider the case of a concrete community, and ask how it is supposed to 'inspect' Nature. Does it come to it with its bare (cognitive and sensory) organs? What are they? Where are they? These are the questions which the area of 'distributed cognition' proposes to answer. It may in fact succeed: after all, once we have secured some workable notions of collective belief and collective intention, the problem of making sense of collective acquired scientific knowledge may be soluble. So, although we are not in better shape than in the individual case, at least we might not be in distinctly worse shape.

Things do get worse when we move to abstract communities. While in a concrete community, there are naturalistic, causal connections between members, such that somewhat like bees which build a hive and collect honey following elaborate collective strategies, scientists build a common home made of shared practices and tools, and collect communal knowledge, these natural links and ongoing processes are lacking in an abstract community. And presumably this is what makes it difficult to grant abstract communities the capacity of forming beliefs and intentions, and therefore of having and deploying established knowledge, except of a summative (and thus impotent) kind. Thus, I submit, although we may perhaps accept [Ind] and (just barely) [C-Com] by assuming that the 'inspecting' and 'establishing' occur in the presence of stored resources, internal, as the case may be, to the individual or to the concrete community, we cannot go further: [A-Com] is not intelligible as it stands (except of course in the metaphoric sense with which we are all familiar).

3. Public scientific knowledge

As Popper long ago,[9] and more recently Alexander Bird[10], have proposed, the role which is attributed to the concept of (scientific) knowledge in various contexts cannot be filled by a single concept: besides the concept of subjective knowledge which is deployed in epistemology, of both the traditional, individualist and the contemporary, social kinds, there is a need for something which Popper proposes to call 'objective knowledge', and Bird 'social knowing'. There are differences between the two notions, and Popper's has been shown to run into serious difficulties.[11] They respond nonetheless to the same urge to sever, or to at least seriously weaken the link between people's states of minds and *truly* social epistemic states. I propose to call *public scientific knowledge* the set of publicly available counterparts of individually and socially held representations, methods, instrumental and intellectual skills which together constitute the competence of scientists or (concrete) scientific communities. It is not in the purview of this paper to defend this working definition or to propose a better one: I mean it to fill the role of the disambiguated sense of 'science' in which it becomes (again) quite legitimate to talk about what science knows and doesn't know at a certain moment in time, what it's confident about and what it's unsure of, and so on.

The basic epistemic situation for public scientific knowledge is this:

> [Pub] As a result of a concerted effort of individuals and teams, public scientific knowledge, initially in state S1, moves to a new state S2 which includes an attribution to Nature of feature F.

But it now appears that public scientific knowledge (henceforth Science for short) has a life of its own, and that individual scientists as well as communities of scientists are not just facing Nature, but Nature *and* Science. At this point, my earlier characterization of the individual situation becomes untenable. In its stead we might consider something like this:

> [Ind*] The Scientist inspects Nature and Science [at stage S1] and establishes that Nature has feature F, as expressed and understood (by her) in the context of Science [at stage S2].

It will be objected that Nature and Science are not on the same footing: Science is about Nature, while Nature is about nothing at all; Science acts only via the scientist's cognitive apparatus, while Nature has original causation. The scientist inspects Nature in part by inspecting Science, but not Science by inspecting

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9   In: Karl R. Popper, "Epistemology Without a Knowing Subject", chap. 3 of his *Objective Knowledge*, Oxford: Oxford University Press, 1972, pp. 106-152; first publication 1968.

10   Alexander Bird, "Social Knowing. The Social Sense of 'Scientific Knowledge'", in: *Philosophical Perspectives*, 24, 2010, pp. 23-56.

11   L. Jonathan Cohen, "Some Comments on Third World Epistemology", in: *The British Journal for the Philosophy of Science* 31, 1980, pp. 175-180.

Nature (at least, not in a straightforward way). Finally, but this is indicated, Science changes in the process while Nature remains (essentially) unchanged. On the other hand, if we are willing to consider for a moment what scientists, from graduate students to Nobel laureates, actually do from dawn to dusk, we must I think concede that 'inspecting Nature and Science' is a less inadequate characterization than, say, 'inspecting Nature in the light of Science'. While sounding more respectable, this way of putting it obliterates the fact that far from only helping the scientist figure out what's there or how things work the way a torch helps Holmes detect a speck of dust on a pew, science is so to speak a complex and ever changing cathedral which she is constantly learning and relearning to navigate. The larger point is that Science (public science) is not delivered at the cognitive door of the scientist for instant plug-in (or to use another metaphor, one familiar in the discussion of education, it does not come, ready made, on a conveyor belt reaching straight into the scientist's mind), it is deciphered and interpreted by the scientist.

A more serious objection to this characterization is that, according to the social conception of science, the individual scientist is helpless if left entirely to her own device. So [Ind*], though improving on [Ind], must go. We are lead to one of the following:

> [C-Com*] The Concrete Community of scientists inspects Nature and Science [at stage S1] and establishes that Nature has feature F, as expressed and understood (by the Community) in the context of Science [at stage S2].

> [A-Com*] The Abstract Community of scientists inspects Nature and Science [at stage S1] and establishes that Nature has feature F, as expressed and understood (by the Community) in the context of Science [at stage S2].

But [C-Com*] is reasonable only as a schematic description of a subjective, perishable, parochial process: the social-epistemic dynamics of a given, historically situated group of people. Although Science emerges from such dynamical processes, it doesn't reduce to any one of them. Replacing the concrete communities by an abstract one, which is the move implicitly commended by the traditional perspective, leads us to [A-Com*], which makes even less sense than [A-Com]: [Pub] must take its place.

With this dual description of the epistemic process, social/subjective, [C-Com*], and public, [Pub], we are at last in a position to untie the dissensus knot. Recall the starting point: it did not seem possible to reconcile the fact that science aims for truth and the fact that science need not aim for consensus without twiddling with the concept of truth, which I have disallowed.

Aiming for truth I have first proposed to construe in terms of what I have called the basic epistemic situation. Going social has meant locating the agency in concrete communities: those are in the business of finding out whether Nature has feature F. But science cannot be described solely in terms of concrete com-

munities: there is a public sense of determining that Nature has F, which involves Science (at successive stages).

The drive towards consensus, the urge to eliminate the 'irritation' (Peirce's term) caused by diverging conclusions regarding F, occur within concrete communities: this is where the classic picture applies. But F seldom, if ever, concerns just one concrete community.[12] When two such communities inspect Nature and Science to find out about F, while we may (for simplicity's sake) assume that they would get from *Nature* the same answers if they asked the same questions, what they draw from *Science* (at stage 1) may be non-identical interpretations S_1' and S_1", leading to different sets of questions and different ways of integrating Nature's answers in S_1' and S_1", eventually leading to S_2' and S_2" which include opposite conclusions regarding F. The two communities' labors are projected onto (public) Science, which thus exhibits the public counterpart of a dissensus regarding F. And although it is possible of course that at some later point, some concrete community will take up as its task to relieve the tension, that tension is not felt by Science, which doesn't feel anything. Time may well pass before a resolution is proposed, either through a deliberate effort to settle the matter, or as a side effect of some development in another area. And if a sufficiently long time elapses without a resolution, the chances are that F will have fallen out of the conceptual vocabulary of science anyway.

In a nutshell, the standard view, re-affirmed by Richardson, is correct when restricted to concrete communities and weakened to leave space for more pressing rational constraints. But as Solomon is right to stress, dissensus can appear and endure on the public plane, when different communities, while interested in the same feature of Nature, follow divergent social-subjective trajectories without feeling any compulsion to blend (or having any way of blending) into one concrete community in charge of resolving their difference (of which they may not even be aware). So that public science contains theories that are plausibly understood as contradictory, and no mechanism to uproot them, neither as quickly as possible after they appear, nor in the fullness of time.[13] Or again: there exist two kinds of dissensus, social-subjective and public; the first is destined to disappear (in principle if not in fact), the second is not.

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12   This is where we need to be clearer on what a concrete community is. It need not be a school of thought, in fact, it better comprise competing schools, for all the Mill-Popper-Feyerabend reasons. On the other hand, it need not be the entire population of specialists of a given area, which may well be divided in communities essentially don't talk to each other, may not even be clearly aware of the others' existence (for example because they belong to different superordinate disciplines).

13   This was one of Cohen's main worries regarding Popper's objective knowledge (ibid.). I think my proposal puts it to rest.

4. THE PICTURE PICTURE OF SCIENCE ABANDONED AND THE NORMATIVE STATUS OF DISSENSUS ESTABLISHED

What we have at best secured at this juncture is an argument against the vanishing status of dissensus: we can see why dissensus occurs and why we have no strong reason to believe it should eventually disappear, but we are in want of an argument showing why enduring dissensus should not in fact be exceptional or at least rare, let alone in what sense it could be a norm, as my title suggests.

It would be interesting to be able to actually count cases of dissensus and compare the figure to the number of cases of consensus, but the prospects of a counting or measurement method are slim. We must content ourselves with the converging impressions of a number of authors with extensive historical knowledge, such as Solomon, Rescher, Laudan, Kitcher, and the many scholars of scientific controversies, who emphasize the non-exceptional character of dissent.

In the social-subjective sense of dissensus, we can first agree with Solomon on a negative construal of the norm of dissensus: it may be better, all else being equal, from a rational standpoint, to remain at odds with an established theory and thus, so to speak, follow a dissensus rather than a consensus strategy.[14] This complements the positive sense in which dissensus is a norm: it favors originality and the debugging of errors in established theories; by introducing diversity, it increases the probability of solving problems and overcoming impasses.[15]

But dissensus as a feature of public scientific knowledge is also, I submit, a norm, in the sense where it occurs habitually and not as a fluke, and in the sense where it contributes to the scientific enterprise.

Why would dissensus occur habitually? There is no space left for a detailed argument, so I'll proceed sketchily. I see two structural features of science which generate dissensus. The first is what I'll call for brevity's sake the *fish-scale effect*. The phrase is due to Donald Campbell,[16] who likens the fit between science and nature to that of the coat of scales on the fish's body: each scale fits tightly the patch of skin it protects, the scales overlap thus providing full coverage of the animal, yet together the scales do not constitute a continuous, tight-fitting cover. Now if something like the fish-scale effect operates in science, then dissensus seems bound to arise, as different people will choose different distributions of contact points. One way of fleshing this out might be to think of a contact point as the founding problem or key phenomenon of some research program. In the

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14 As Solomon writes: "it is not important for a scientist to get the opposition to convert or die. […] What matters is that a scientist develop empirical successes – especially unique empirical successes – in their own theory." "Responses to critics", in: *Perspectives on Science* 16, 3, 2008, p. 282.

15 Scott E. Page, *The Difference*. Princeton: Princeton University Press 2007, chapter 6.

16 Donald T. Campbell, "Ethnocentrism of Disciplines and the Fish-scale Model of Omniscience", in: Muzafer Sherif and Carolyn Wood Sherif (Eds.), *Interdisciplinary Relationships in the Social Sciences*. Chicago: Aldine 1969, pp. 328-348.

favorable case, a theory is developed which provides a satisfactory solution to that problem or a scientifically irreproachable account of that phenomenon, but the theory's fit to other phenomena in the vicinity of the starting point turns out to be less satisfactory. Another team might take one of those as its starting point and develop another theory which is bound to conflict with the first.

An escape from this predicament might exist if we could establish that the contact points are objectively determined, that they are, so to speak, marked on the body of Nature (some dual image, of sorts, of the notorious 'joints' of Nature ~~which~~ mark out the true universals) – but I am not enough of a metaphysician to see how that could be done.[17] But at any rate, we can put the proposal thus:

> [Fish-scale] If science provides only local maps of Nature, then dissensus is inherent in its development.

The second structural constraint which I suggest forces dissensus as a normal aspect of science is what I call radical incompleteness. Contrary to what the fish-scale model might suggest,[18] the coat of scales never comes close to covering the whole beast, even in the infinite limit of the end of history. Science does not gradually 'fill out the picture', it doesn't 'complete the puzzle'. There isn't a finite (albeit gigantic) set of empirical facts out there ~~which~~ are gradually brought to light and integrated into ever more encompassing theories. There are indefinitely many ways of chunking Nature, and indefinitely many sets of questions to ask, and although science makes steady progress, it never gets any closer to exhausting its general agenda.

If this is indeed the case, then it seems that the gradual stabilization which occurs in fields of limited scope does not extend to science as a whole: fields keep growing, boundaries shift, empirical facts continue to arise, science undergoes tectonic reconfigurations. In the process, dissensus, smoothed out of one end of the rug, reappears at another: newly formed theories arising in the novel context conflict with established ones, and between themselves.

Let me state this second structural constraint in conditional form:

> [Incompleteness] If science is radically incomplete, then dissensus is inherent in its development.

Let me try to piece things together, tying well-known phenomena which I have hardly touched upon and those which I have tried to pin down. In both social-subjective and public spaces, some permanent dispositions tend to create dissensus, just as other dispositions tend to create consensus.

In social-subjective space, consensus-increasing factors are: the demand for consistency and the need for expediency (which encourages communities to adopt a 'satisficing' view of consensus); the dissensus-increasing factors are: the quest

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17   Of course it remains open to someone, say a radical scientific realist, to reject the metaphor altogether, in which case the issue doesn't arise.

18   I doubt it was part of Campbell's thought.

for originality, the search for resolution of anomalies and the underlying mistaken assumptions, the division of labor leading to divergent mindsets and commitments, the inevitable diversity in starting points (no two scientists, and no two communities having the exact same initial set of data, assumptions and tools[19]) and finally perhaps sheer intellectual curiosity. As the Fathers and the Good Sons insist, concrete communities do have consensus as a goal, or perhaps as a hope: they do care and are not comfortable with enduring dissent. But they cannot have as their sole goal the suppression of this source of discomfort.

In public space, over and above the traces left by the communities, which are so to speak the projection of the factors at work in the social-subjective space, there are structural factors which operate independently. I have not examined the case of those which tend to increase consensus. Perhaps there is a general argument to the effect that public science tends toward greater consensus for structural (*e.g.* transcendental) reasons, or that a unifying theory will eventually be seen to provide a general framework within which pockets of dissensus will be reduced one by one; but I have no such argument to offer. On the other hand, there is a sociological factor at work, which is the operation of what may be called the scientific police: its job is to minimize the influence of heterodox voices at all levels of public scientific life. For dissensus-increasing factors, in reverse order, the sociological/institutional one I can think of is the pluralistic organization of academia: each institution attempts to create its own niche. The two epistemic factors I have suggested are the fish-scale effect and the incompleteness effect.

The resulting picture is a far cry from either the Fathers' or the Sons' views. Instead of having a space of public science tending towards monophony, a great book of truth which all scientists eventually subscribe to, or else a space of forever conflicting theories, what we see, or so I suggest, is a dynamic field where areas of varying levels of consensus develop, contract and expand under the combined effect of consensus and dissensus-favoring factors. The normative monopoly of consensus has been displaced in favor of a more symmetric distribution, at both the social-subjective level, as Solomon argues, or at the public level, as I have tried to show.

Finally, I have claimed that the normative status of dissensus is also instrumental. Again, at the social-subjective level the point has been abundantly made by Mill, Popper and other authors, some now active in social epistemology. But dissensus also favors the public or objective life of science, endowing it with resilience in the face of an uncooperative Nature. The reason is that when a theory

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19 According to Andrew Lugg, (i) scientists have different access to data, so that (ii) they are bound to come to different conclusions and (iii) there is no way of eliminating 'access differences' and hence no way of eliminating disagreement in science without adversely affecting one or another aspect of the scientific enterprise itself. "Thus, it would be a mistake to think that disagreement among scientists is incidental to science". Andrew Lugg, "Disagreement in science", in: J*ournal for General Philosophy of Science* 9, 2, 1978, pp. 276-292.

collapses, the existence of an alternative in public space, ready to take over and, so to speak, to hit the ground running, allows science to quickly overcome its failure and continue the inquiry, rather than abandon the project until sometime, somewhere, a new line of thought emerges, if it ever does. Note how important it is not to link the alternative theory to a particular concrete community (for example, as in Kuhn, a rival, younger school, or more generally, one engaged in a controversy with the one which has just collapsed): as long as it is poised for uptake by a scientist (it may be the work of a dead and forgotten author, as in one of Bird's examples), it remains a live possibility. This idea is by no means original: it is a standard theme in evolutionary epistemology à la Hull[20]– keeping alive a diversity of genotypes protects a population from going extinct and helps it move to a more hospitable niche. I said that Nature was the uncooperative one, but the evolutionary scheme suggests a more complex picture: theories don't only face falsifying data, they also co-exist with other theories, and can be pushed out of the picture when irreconcilable differences arise. As this example shows, and that will be my concluding remark, there are many important issues, the status of dissensus being one, where taking the social turn only goes half way: as Popper and Bird insist, it must be completed with the public turn.

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20   David L. Hull, Science and Selection. Cambridge: Cambridge University Press 2000.