ALBAN BOUVIER

PASSIVE CONSENSUS AND ACTIVE COMMITMENT IN THE SCIENCES

ABSTRACT

Gilbert (2000) examined the issue of collective intentionality in science. Her paper consisted of a conceptual analysis of the negative role of collective belief, consensus, and joint commitment in science, with a brief discussion of a case study investigated by Thagard (1998a, 1998b). I argue that Gilbert's concepts have to be refined to be empirically more relevant. Specifically, I distinguish between different kinds of joint commitments. I base my analysis on a close examination of Thagard's example, the discovery of *Helicobacter pylori*, and two other historical cases involving the Copenhagen school of quantum mechanics and the Austrian school of economics. I also argue that it is difficult to fulfill the condition of common knowledge, even in Gilbert's weak sense. I conclude by raising serious doubts about the very possibility of a certain type of joint commitment, which I refer to as an implicit joint commitment.

My aim in this paper is to evaluate both the theoretical soundness and the empirical relevance of Gilbert's concepts of *joint commitment* and *collective belief* for understanding science. Gilbert argues that scientists might be *collectively or jointly committed* to scientific theories or scientific propositions different from their deeply held personal views. She also argues that such commitments can impede scientific progress. In the wake of arguments by Coady (1992), Goldman (1999), Hardwig (1985), Hardin (2002), etc., it is generally acknowledged that epistemic trust and epistemic deference to intellectual authorities may be inescapable in science. But, this does not mean that there is an *obligation* to accept the opinion of somebody merely because he expresses himself as the spokesman of a group. It only means that it seems reasonable to trust someone who is an expert in a field if one is a layperson. Departing from this widely held view, Gilbert draws attention to the obligatory aspect of scientific collective life.[1]

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But in this paper I argue that with some further clarification Gilbert's concepts can be made more empirically relevant and accurate than they are. My approach to epistemology and philosophy of science is similar to Thagard's (1998a, 1998b). That is, I believe we need a naturalized epistemology. Consequently, I aim to compare my proposals with historical data.

I argue that we need to distinguish more clearly than Gilbert does between explicit joint commitment and mere consensus, consensus and implicit joint commitment, and, finally, between two kinds of implicit or tacit joint commitments.

1. EXPLICIT JOINT COMMITMENT AND CONSENSUS

In this first part, I would like to make a clearer distinction between consensus and joint commitment than Gilbert does.

1. Gilbert notes that in certain occasions one can ascribe to a group certain beliefs that are not shared by everyone in the group. Although Gilbert does not take real contracts as paradigmatic examples, I believe that the simplest and clearest cases of such collective entities and collective beliefs are met in contractualist situations, where, as a member of the group, everyone who participates in a vote has to accept the collective decision as his own decision, even if this collective decision strongly conflicts with his own deeply held views. In fact, as far as the collective decision is procedurally valid, every member is *jointly committed* with every other member to this collective decision. This assumes that the group commitment is more than a mere collection of individual commitments, where each one can decide by oneself whether or not to follow the decisions of the group. If somebody disagrees with the decision, she has to wait for another assembly or, if the procedures allow such an initiative, to act in the way appropriate to convene a new assembly. One can say that the subject of the collective belief is the group or that the collective belief is the belief of the group since it is not necessarily the belief of each member in the group. In Gilbert's terminology, it means that a "summative account" of the decision would not fit in with the effective procedure in such a case. Gilbert calls the group in such a case a "plural subject."

2. According to Gilbert, many collective beliefs should be understood as the result of joint commitments. One can disagree with the relevance of the phrase "collective belief" regarding the result of contracts, and several authors in fact have contested this terminology (see Meijers 1999 and Wray 2001). It might be more appropriate to speak of collective acceptance, given the usual meaning of the words and the famous conceptual analysis given by Cohen in *Belief and Acceptance* (1992). Even Gilbert often speaks of acceptance instead of belief, but without giving specific attention to these lexical variations (2002). According to Cohen, *belief* is a passive mental state – that is, something that happens to you rather than something resulting from your conscious activity. Belief is, for example, the deeply held conviction that members of a jury might have regarding the guilt of a

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defendant at the very beginning of a trial, without really being able to justify this conviction. *Acceptance* is an active mental state, something that members of a jury can reach only after a personal deliberation at the very end of the trial, that is, after having weighed the arguments pro and con for the defendant's guilt throughout the trial. Even if a member of the jury might still feel deep down that the defendant is guilty, he may accept the conclusion of his personal rational deliberation that she is not guilty.

What is conceivable at the individual level is also conceivable at the collective level. And, in the case when collective decision-making is ruled by a formal procedure and in which a majority is required, as is the case in European countries where the Napoleonic Code is in force, the members of a jury will be jointly committed to the collective acceptance, whether the individuals believe deep down in the verdict reached or not and whether they rationally accept it or not. For example, even if somebody has personally accepted that the defendant is not guilty, she must accept the collective acceptance that the defendant is guilty as her decision as a member of the jury if the majority (under the Napoleonic Code) says he is guilty. Following Gilbert's account but using Cohen's concept of acceptance, we should say that it is a matter of collective or *joint acceptance*, based on a *joint commitment*.

3. In a paper devoted to joint commitment in science, Gilbert (2000) used this concept to account for the fact that a medical hypothesis was accepted only very slowly by the scientific community. In the particular case Gilbert discusses, the two scientists who first advanced the hypothesis, Warren and Marshall, were eventually awarded the Nobel Prize in Medicine. It was the hypothesis that gastric and duodenal ulcers are very often the effect of an infection by the bacterium *Helicobacter pylori*. Gilbert argues that this might be because biologists were jointly committed to the thesis that even normal levels of gastric acid would prevent any bacteria from surviving in the stomach, which was exactly opposed to Warren and Marshall's hypothesis, and to an alternative explanation – that a gastric ulcer was caused by excess gastric acid.

Gilbert's account implies that though not every scientist might have believed or personally accepted these claims, every scientist felt committed to collectively accepting them. Although Gilbert recognizes that the word "consensus" has different meanings, in this paper she chose to use the expressions "consensus" and "joint commitment" interchangeably. Gilbert suspects that the fact that one of the first papers written by Marshall on this topic was rejected when it was submitted to the 1982 Australian Gastroenterologist Society Congress might express such a joint commitment. Thagard (1998a, 1998b) has previously given a very detailed account of the discovery and acceptance of this new medical hypothesis in two papers. Scrutinizing Thagard's report leads me to disagree with Gilbert's account of this case. The significance of the examination of this case goes beyond this case itself because it is a very common type of case and it induces us to refine the concept of joint commitment itself.

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4. Thagard does not focus on the earlier rejection of the paper by the committee of the Australian association, but when he comes to this issue, the most plausible scenario that emerges from his account is the following one. Many and maybe all the members of the committee had learned during their university education that the stomach was too acidic for any bacteria to survive. Most of them had just learned this from their professors or textbooks without being able to personally verify it, so it was a passive belief, something they believed in without more examination, merely trusting their professors. But specialists in gastroenterology had been able to verify that the claim was true, so they personally and rationally accepted the statement that they had already learned at university. It was not a matter of joint commitment for anybody – they did not have to accept a statement adopted by an explicit procedure in an assembly, and there is no other sign or indication that they were implicitly committed to accept this statement. On the other hand, all scientists now acknowledge that the first empirical data provided by Marshall claiming the existence of a new bacterium were weak. In particular, Marshall could not dismiss the counter-hypothesis that his own microbiological data had been contaminated by bacteria coming from an external source. Thus, there is no reason to think that those who rejected Marshall's first paper about the role of *Helicobacter pylori* in the development of gastric ulcers did it because they felt jointly committed with the rest of the medical community to some other hypothesis. A more plausible account is that most scientists shared common views on this issue, without having thoroughly examined these views. Thus, most scientists passively "believed" what they had learned at university, which is an example of what I call a *consensus* or, to avoid any ambiguity, a *passive consensus*. But a minority of scientists (gastroenterologists) personally rejected (for good reasons) Marshall's statement, so their behavior can be accounted for as collective acceptance understood as a *sum* of *individual acceptances*.

5. What can be misleading is that Thagard himself speaks of "consensus" also; more precisely, he discusses *medical consensus conferences*. In such conferences, the members of the panel deliberate on the issue at stake and finally make a collective decision to which everyone is committed. It is clear that, following the conceptual distinctions I have stated above, the "consensus" here is very close to a contractualist situation and is based on an *explicit joint commitment*.

The crucial point is that the consensus conference discussed in Thagard's paper did not happen *before* the discovery that *Helicobacter pylori* is the cause of ulcers. On the contrary, a consensus conference was organized in 1994, that is, *after* the discovery of the role of *Helicobacter pylori* in gastric ulcers and after its acceptance by the majority of specialists. And the conference was actually organized in order to *recommend the use of antibiotics* to all physicians.

Therefore, in this case, Gilbert's concept of joint commitment proves to be relevant. But, it does not apply to the very slow acceptance of the important medical discovery by the scientific community. Rather, it applies to the medical consensus conference long after the discovery was made.

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2. TYPICAL TACIT JOINT COMMITMENT

In this second part, I want to focus on a slightly different notion, the notion of tacit or implicit joint commitment. I will show that the concept of joint commitment, more specifically the concept of tacit joint commitment, can provide us with a new account of a famous historical issue: what does one mean when one speaks of the Copenhagen interpretation of quantum mechanics? I argue that Gilbert's account of groups is especially illuminating in situations in which there are *not* explicit contracts.

As I have elaborated at length on this case in previous articles (Bouvier 2004, 2007b), I will be relatively brief here.

1. For a long time, speaking of the Copenhagen interpretation was very common, and it still is not rare. Some scholars, like Beller (1999) and Howard (2004), recently argued that the idea of a unified conception (the Copenhagen interpretation) is mainly due to a retrospective view from philosophers of science, especially Kuhn and Popper. They added that, nevertheless, the very idea of such a unified conception comes from Werner Heisenberg, who seems to have coined the term "Copenhagen interpretation" in 1955 in a volume of essays in Niels Bohr's honor and extends back to the 1927 Solvay Congress.

2. Most recent scholars now deny that there was any unified conception of quantum mechanics in Copenhagen shared by Bohr, Heisenberg, Dirac, Pauli, etc. during the group's most creative period (namely in the twenties and thirties). Beller (1999) and Howard (2004), for example, have established that Bohr and Heisenberg already strongly disagreed in the twenties, both before and after the Volta Congress in Como and the Solvay Congress in Brussels, which were both held at the end of 1927. What I contend is that the idea of a unified group sharing a coherent conception is not a misrepresentation constructed by certain philosophers of science like Kuhn, but that the members of the Copenhagen group, *as early as 1927*, provided themselves with such a representation, although more or less involuntarily. I also argue that this is a clear case of joint commitment.

3. To better grasp this point, it is useful to refer to Gilbert's subtle examples of everyday contexts in which a person in a group takes the floor as if she were the spokesperson of the whole group so that this opinion is ascribed by a larger audience to the group itself, even if the other members of the group did not express agreement. This results from the interpretation of the absence of explicit disagreement as tacit agreement between the group and the spokesperson (Gilbert 1994).

4. Something similar seems to have occurred at the Como Conference in September 1927 and at the Solvay Congress one month later. Thus the idea that the physicists who regularly met in Copenhagen shared a collective conception of quantum mechanics seems to have emerged in these particular places. I will focus on the Como conference, more specifically on Bohr's lecture because this case is the most typical. In this paper, Bohr gave the first expression of

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the complementary principle. In all the numerous versions of this paper, even the last one (which, however, benefited from many exchanges with Pauli), his formulations are considered by scholars as quite obscure (Mehra and Rechenberg 1982; Chevalley 1991). Beller's interpretation is that the obscurity does not come only from the subtlety of Bohr's still emerging ideas about intrinsically complex physical phenomena, but from the fact that Bohr took the floor as a spokesman of the Copenhagen group as if each member shared the same conception he was setting out or should have shared it after it had been set forth by Bohr publicly.

Actually, Bohr seems to have even tried to suggest a conception of quantum mechanics so unified that even the "enemy," Schrödinger, might have accepted this viewpoint. In this Como paper, Bohr tried to set forth the role of Heisenberg, Dirac, Jordan, Pauli, Max Born, and even Schrödinger in the construction of quantum mechanics, so that the listeners' and then the readers' impression should at least have been that there was a intensive collaboration between all these physicists to reach a common interpretation, which Bohr was formulating in his paper. Schrödinger did not participate in the Como conference, so he could not agree explicitly or implicitly or disagree explicitly. But Heisenberg and Pauli were present at the Como conference, and neither of them expressed any public disagreement, not even Heisenberg, whom Bohr mentioned several times regarding the significance of the uncertainty paper to their collective enterprise.[2]

Consequently, Bohr and the large majority of the people present at the Como conference might have sincerely thought that it was a conception shared by all the members of the Copenhagen group, if not by Schrödinger, and that all of them had worked together in Copenhagen "as a body" (as Gilbert sometimes likes to say) to express a joint commitment relationship. However, the letters exchanged by the members of the Copenhagen group show that they strongly disagreed with ideas expressed in Bohr's paper, and their own personal articles confirm that none of them could have accepted all the views or claims stated by Bohr in Como, even if none of them *explicitly* disagreed with Bohr's ideas in their articles. Beller contends that even Bohr could not really personally accept all the ideas that he assumed as a self-proclaimed spokesman of the group or of the entire community of physicists (1999, 119). According to this reading, Bohr was not entirely sincere in his lecture.

Thus, assuming that Beller is right, my conclusion is that the Copenhagen interpretation is a collective acceptance of the Copenhagen group, a set of principles to which they were implicitly jointly committed, although this interpretation was not personally shared by any single member of the group. This case is a particularly clear case of a view ascribed to a group as a group or plural subject, even if the content of what was then ascribed to this plural subject was confused and misrepresented the personal views of the members of the group.

I would like to focus in the third part of this paper on another distinctive aspect of the Como conference, the fact that the implicit joint commitment seems to have

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happened *at a determined and very precise time*, the day of Bohr's lecture, as if it were an explicit commitment with a formal procedure.

3. PROGRESSIVE TACIT JOINT COMMITMENT

One can take a further step in considering cases in which the implicit joint commitment is not performed at a determined and precise time but instead occurs progressively and slowly. This step moves us away a little more still from typical contracts. Not only are these commitments implicit, but one cannot even say precisely at what time they occurred. Nor can one really say when these commitments cease, since they may progressively disappear as well.[3] Consequently, there can be numerous misunderstandings concerning the existence of these commitments and on their content. There might not be *common knowledge* even in the everyday sense of the expression, which is far less demanding than the technical one developed by Lewis (1969). Conceptually speaking, this kind of implicit joint commitment, of relatively undetermined duration, is still more interesting than the previous one because it looks more like a passive process, so that it may be empirically difficult to determine whether one is dealing with an implicit joint commitment or with just a passive consensus when people agree on an issue.

1. Again, it is convenient to start by referring to Gilbert's phenomenological characterizations of joint commitments. Gilbert does not always focus on the same features. Rather, her focus changes depending on the specific issues she wants to tackle. Thus, in *On Social Facts* (1989), Gilbert focuses on situations in which a joint commitment is progressively, although slowly, emerging between two people. These situations, very common in everyday life, also happen in scientific life, where they might be very common as well. The main problem is to know if the condition of common knowledge (even in the weak sense) is ever satisfied.

2. I will consider two examples, the first one borrowed from the later Copenhagen group, the other one from the later Austrian school in economics. I add this latter example because this case is even clearer than the former on this issue. The two groups are very similar in numerous aspects, so the comparison may be particularly significant. In both cases, the earlier group was a group renowned for its great intellectual freedom. Thus, Bohr, Heisenberg, Dirac, Jordan, and Pauli are now almost unanimously regarded as having had very different ideas on many issues, for example, on the relevance of the complementary principle or on its meaning, on realism and pragmatism, on causality, on the exact relevance of the matrix theory, etc. (Beller 1999). As for the economists Carl Menger, Friedrich von Wieser, and Eugen Böhm-Bawerk, although they shared some basic assumptions about the relevance of the marginalist revolution in economics, of the principle of methodological individualism, and so on, they disagreed on many other topics, e.g. on the theory of money (Boettke 1994a, 1994b).

In both cases, the later periods are reputed to have been much less creative than the earlier periods and the social structures of the groups are said to have also

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exerted a negative effect on younger researchers. But it does not seem that it was due only or even mainly to external personal pressure. On the contrary, in both cases the situation looks more as if the leading figures thought that the younger scientists were as committed to the same collective scientific goal or view as they were themselves. Actually, in both cases, one encounters clear evidence of students or younger scientists having experienced *guilt* due to the fact of merely having thought differently from their mentor or the leader of the group, which reveals that they had felt that there was a sort of obligation to assent to certain specific ideas. Furthermore, it seems that they acknowledged that this obligation was legitimate. And in at least one case, in the later Austrian school, one also meets the reciprocal situation, a case of strong *indignation* on the part of senior scholars. But the most important aspect of these two cases here is that there was *not a specific context* in which one can say a sort of implicit joint commitment had been contracted in this situation, as in the case of the Como conference. In both cases, the commitment seems to have happened much more progressively.

3. In Bohr's case, Beller says that certain scientists felt guilty for not thinking like Bohr. "Bohr's unpublished correspondence discloses the overwhelming guilt experienced by those physicists who dared to challenge him" (1999, 274). She refers more specifically to Weizsäcker's case. Weizsäcker was already respected as an excellent physicist and was no longer Bohr's student. Besides, he belonged to the younger generation, and he could not have attended the Como or the Brussels conferences. Consequently, he could not have felt jointly committed with the other members of the Copenhagen group to Bohr's or Heisenberg's statements during these particular events. He was just a regular participant in the later Copenhagen seminars. But Beller reports Weizsäcker once remarked, after having met Bohr and understood hardly anything, "What must I understand to be able to tell what he meant and why was he right? I tortured myself on endless solitary walks" (275). Weizsäcker's reaction seems strange since it might have seemed more rational to trust oneself and be skeptical of the relevance of Bohr's statements, even if epistemic deference is necessary. It is true that it might just have been epistemic deference, but the idea that he *should* think that Bohr's thoughts were true at least raises the question whether he did not feel committed to assenting to Bohr's ideas. Beller suggests that this kind of behavior is typically the kind of attitude that may prevent researchers from being innovative. In any case, there was not an *explicit* joint commitment and if the implicit commitment hypothesis is right, this implicit joint commitment does not seem to have been generated in a specific situation.[4]

4. A still clearer case, both with respect to guilt and indignation and in several other respects as well, is the later Austrian school. Von Mises's followers created the von Mises Institutes, which had the goal of advising politicians on economic issues, basing this advice on scientific assumptions. These institutes have constituted cases of explicit joint commitments since they contain explicit statutes, in which it is even written what one ought to believe (or to accept) to be "an Austrian" in economics (Cowen 1991). And potential members have to submit to a

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formal application procedure to join the institutes. But before the creation of these institutes, von Mises himself was the center of a group in which it seems that not only were there implicit joint commitments, but the establishment of the implicit joint commitments was also progressive and slow.

5. Typical instances of guilt and indignation are found, on the one hand, in the memoirs of von Mises's wife (von Mises 1976) and, on the other hand, in the strange homage to von Mises that Fritz Machlup, a former assistant to von Mises, wrote (Machlup 1981). Von Mises's wife reports that Machlup had once expressed an opinion different from Mises on an epistemological issue. Specifically, Machlup thought that the recent "positivist" philosophy of science, like Carnap's, made von Mises's conception of the *a priori* significance of the rationality principle hard to uphold. And von Mises expressed such strong indignation that he refused to talk to Machlup for several years. Machlup himself reported another case, with less lasting effects on their relationship, in which von Mises expressed indignation because Machlup had expressed an opinion contrary to his on the gold standard issue. But in his homage Machlup expressed explicit feelings of guilt at having thought and publicly expressed something different from his mentor. He tried to demonstrate that his position was actually derived from von Mises's principles, going so far as to say that it was von Mises's position on the gold standard that was not coherent. Therefore, it was von Mises, not Machlup, who had broken the joint commitment (Bouvier 2007a)!

6. But the main point here is that there had never been in the Austrian school a circumstance similar to Bohr's Como lecture. I have not found any report that von Mises had once publicly spoken on the behalf of the Austrian group as its spokesman at such a crucial event. Furthermore, Machlup, who was von Mises's former student and was later even von Mises's assistant, is not usually considered as having shared the most typical ideas of the Austrian group. And the von Mises Institutes had not yet been set up. Thus there were not *determined* occasions in which Machlup might have jointly committed – explicitly or implicitly – with von Mises or other members of the Austrian group. It is instead the fact that Machlup had worked closely with von Mises for a certain time that had finally pushed von Mises to think that a joint commitment had implicitly emerged between them. Thus, this case enables us to see clearly that the implicit commitment probably just occurred without a clear act of the will at a specific time from either of the two participants. Consequently, the confusion with a passive process is easy.

7. To conclude this third part of my investigation, I would like to focus attention on a point that contradicts what seems to be an essential condition of joint commitment situations if one follows Gilbert's accounts of joint commitment. Often Gilbert states that *common knowledge* is a necessary condition. But for her it does not mean that each individual who is jointly committed knows that each other one is committed as well and knows that everyone knows that he knows, and so on (Gilbert 1989). Actually, it has been long argued by many social scientists that such a demanding condition is psychologically quite implausible and, further,

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that communication nevertheless often succeeds (e.g. Sperber and Wilson 1986). But even if what is required is only that each participant in a situation just knows that the others are committed as he is, it seems to be rather obvious that the possible implicit character of a joint commitment, and, furthermore, the fact that in a given situation one cannot even determine when it starts and when it ceases, leave the real existence of a joint commitment very uncertain. Consequently, empirically speaking, one must expect numerous misrepresentations on the issue of knowing whether in such and such situations there is a joint commitment or not (and on the matters on which there may be a joint commitment as well). Indeed, in Bohr's case, both Bohr and the younger physicists seemed to have thought they were jointly committed even if the commitment was implicit and had progressively occurred. But in von Mises's case, whereas evidently Mises thought that Machlup was committed to the same statements as he, it is questionable whether Machlup thought he was breaking a "joint commitment" when he expressed a position different from von Mises's. It instead seems that Machlup realized only afterwards that von Mises thought Machlup was jointly committed to the same statements as he; and then Machlup understood at this moment he was wrong not to have understood that he was actually implicitly committed. Consequently, he felt guilty, at least for a while. In the gold standard case, Machlup seems to have considered that, in fact, his position was much more coherent with von Mises's core principles, that both might have been jointly committed to these principles, although, again, he had not personally understood that he was committed and that, in any case, it was von Mises who had broken the joint commitment.

Finally, I would like to conclude this investigation by focusing again on the fact that empirically speaking, it is not always easy to know whether one is dealing with joint commitment or consensus. In Warren and Marshall's case, anyway, one can notice that even if Warren was senior and Marshall junior, there is no evidence at all that they were jointly committed with each other in a sense that might have prevented Marshall from breaking this commitment. However, it is a fact that their first common publication was not a joint paper. They chose a very unusual style, which *Lancet* accepted by publishing in the same issue one letter from Warren reporting Warren's own first discoveries signed by Warren only, and another letter from Marshall reporting Warren and Marshall's joint discoveries, signed by Marshall only. It is as if for some reason, they were reluctant (or at least one of them was reluctant) to contract a joint commitment in signing a joint paper (Goodwin and Worsley 1993).[5]

CONCLUSION

In this paper, I have proposed some new conceptual distinctions, particularly between mere consensus and explicit joint commitment, and between two kinds of implicit joint commitment, one still looking like an explicit contract although

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possibly more misleading, and another one whose content and even existence may be much more undetermined still. Finally, based on historical data, I have emphasized the fact that common knowledge, even understood in Gilbert's broad sense, is a condition very difficult to fulfill. Consequently, one should be skeptical about the very existence of groups in Gilbert's sense, that is, as "plural subjects," at least in science.

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NOTES

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2 Dirac could not come to Como (Mehra and Rechenberg 1982).

3 Philip Pettit (2003) has elaborated in detail on the issue of temporal group identity. But he only envisages groups that have been constructed through real contracts (therefore explicit joint commitments; see above). His model has not yet been applied to scientific groups nor has its empirical relevance been evaluated.

4 There was an intermediate period at Copenhagen. According to Beller, as early as 1930, "young physicists … were exposed automatically to the new philosophy …. most of them simply adopted the official interpretation without deep deliberation" (1999, 39). She adds, "Heisenberg was aware of this. In his 1930 book, which he dedicated to the 'diffusion of the Copenhagen spirit,' Heisenberg conceded that 'a physicist more often has a kind of faith in the correctness of the new principles than a clear understanding of them' " (39). Heisenberg and Beller here are giving a very typical example of collective *passive* belief or of what I call a (passive) *consensus*.

5 For another explanation, see Thagard (1998b, 330 n. 3).

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Alban Bouvier (Ph.D, Sorbonne, Paris) has a teaching position at the University of Provence (Philosophy and Social Sciences) and a research position at the Institut Jean Nicod (Ecole Normale Superieure, Paris). He has worked on the relationships between Argumentation Theory and Rational Choice Theory. He is now especially interested in collective intentionality, both at the conceptual and empirical levels.

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