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JOINT ACCEPTANCE AND SCIENTIFIC CHANGE: A CASE STUDY

ABSTRACT

Recently, several scholars have argued that scientists can accept scientific claims in a collective process, and that the capacity of scientific groups to form joint acceptances is linked to a functional division of labor between the group members. However, these accounts reveal little about how the cognitive content of the jointly accepted claim is formed, and how group members depend on each other in this process. In this paper, I shall therefore argue that we need to link analyses of joint acceptance with analyses of distributed cognition. To sketch how this can be done, I shall present a detailed case study, and on the basis of the case, analyze the process through which a group of scientists jointly accept a new scientific claim and at a later stage jointly accept to revise previously accepted claims. I shall argue that joint acceptance in science can be established in situations where an overall conceptual structure is jointly accepted by a group of scientists while detailed parts of it are distributed among group members with different areas of expertise, a condition that I shall call a heterogeneous conceptual consensus. Finally, I shall show how a heterogeneous conceptual consensus can work as a constraint against scientific change and address the question how changes may nevertheless occur.

Recently, a substantial literature has developed that discusses the social aspects of scientific knowledge, including the notions of collective knowledge and collective acceptance. One of the currently popular accounts of joint acceptance drawn upon in this literature is the analysis provided by Gilbert (1987, 1989, 2000, 2003). According to her analysis, members of a group jointly accept the claim that p if and only if it is common knowledge in the group that the group's individual members have openly expressed a conditional commitment to jointly accept together with the other members that p (cf. Gilbert 1987, 195). Admittedly, Gilbert originally introduced her analysis to explain joint *beliefs*, claiming that "a group G believes

Episteme 7.3 (2010): 248–65
DOI: 10.3366/E1742360010000973
© Edinburgh University Press
www.eupjournals.com/epi

https://doi.org/10.3366/epi.2010.0206 Published online by Cambridge University Press

that p if and only if the members of G jointly accept that p" (ibid.). However, several scholars have adopted the notion of *joint acceptance* by a group while bracketing the question of whether the group as a plural subject believes that p.

One of the scholars that have analyzed joint acceptance and its importance in science on the basis of Gilbert's model is Wray (2007). He argues that groups bound together by functional dependence may draw conclusions from their data that differ from the conclusions each individual member of the group would be inclined to draw, and in this way the group has the capacity to adopt views that are irreducibly the group's views. However, Wray leaves open an empirical question to be answered on a case-by-case basis, namely, which kinds of groups have the capacity to adopt views that are irreducibly the group's views. Further, Wray says little about the details of *how* in arriving at a position on a scientific claim the various members of the group depend on the other members.

Another scholar arguing for the importance of joint acceptance in science is Rolin (2008). She adopts a contextualist theory of epistemic justification, according to which justification takes place in a context of assumptions that have been adopted with a commitment to defend them if challenged. If certain assumptions are challenged, they must be investigated, again in a context of assumptions, and in the end they may be defended, modified, or abandoned. On the basis of this contextualist theory, Rolin argues that groups whose members share the aim of epistemic responsibility are motivated to accept views jointly, because the commitments to defend background assumptions can then be distributed within the group. The joint commitment relieves the individual group member whose capability of defending a particular background assumption is limited, for example, because it is outside his or her area of expertise. However, Rolin adopts a very liberal interpretation of how a joint commitment is formed and of which kinds of groups can form joint commitments. Thus, she suggests that individual scientists can express their willingness to participate in a joint commitment by simply using in their own reasoning those assumptions to which a group is jointly committed, but she does not discuss whether this suffices to become part of the group's joint commitment, or how in this process defense requirements are distributed.

Although these accounts include arguments to the effect that functional dependence and distributed responsibility are key aspects of the capacity to form joint acceptances as a group, neither of the accounts specify how the cognitive content of the jointly accepted claim is formed, including how cognitive resources possessed by different group members may be combined to form a new jointly accepted claim.

Recently, Staley (2007) has attempted to bridge the divide between analyses of the social processes through which joint acceptances are formed and cognitive analyses of how the content of the jointly accepted claim depends on the knowledge of the group members participating in the joint acceptance. His focus has been on evidential collaborations that exist for the purpose of evaluating evidence for scientific claims. In his account, these collaborations aim, on the one

https://doi.org/10.3366/epi.2010.0206 Published online by Cambridge University Press

hand, to make novel and significant claims that will sustain further scrutiny, and, on the other hand, to avoid making claims that subsequent work reveals to be false. According to Staley, this emphasis on making novel and significant claims means that, operating at the frontiers of accepted knowledge where some uncertainty must be accepted, it will sometimes be necessary to advance new claims on the basis of a heterogeneous consensus, that is, a consensus in which members of the collaboration all accept the conclusion, despite disagreement as to which of the premises actually support that conclusion. Staley's account only covers evaluation of evidence for a given claim, not the conceptual developments that may have played a crucial role in the group's arriving at the claim, such as introducing new concepts, ascribing new characteristics to existing concepts, introducing new relations between concepts, etc.

In order both to examine the plausibility of the claims made about joint acceptance by Wray, Rolin, and Staley, and to extend their accounts to include details on the cognitive content of jointly accepted claims, in this paper I shall present a case study and examine the cognitive processes involved when scientists with different areas of expertise collaborate. Based on my previous work on the cognitive structure of scientific change (see especially Andersen, Barker, and Chen 2006), I shall introduce the notion of a heterogeneous *conceptual* consensus to describe the situation where an overall conceptual structure is jointly accepted by a group of scientists while individual parts of the conceptual structure – such as substructures related to specific subdisciplines – are distributed among group members with different areas of expertise.

I am here adopting and elaborating on a Kuhnian account of concepts. In its basic form, this account treats a scientific lexicon as a general sort of categorization module: a lexicon is a hierarchical conceptual structure that divides objects into groups according to similarity and dissimilarity between perceived objects. This family resemblance account of concepts enables a graded structure of concepts that allows instances to differ in their typicality (Andersen 2000). As Hoyningen-Huene has argued, such conceptual structures contain both ontological knowledge and knowledge about empirical regularities (1992, see especially chapter 3.7). Further, as Andersen, Barker, and Chen (2006) have argued, correlations of the features involved in establishing similarities and dissimilarities are not simply empirical generalizations. Instead, theories will usually be developed that explain the correlations. In this way, concepts and theories are inextricably intertwined. Again, this enables a graded internal structure of concepts where features that play a causal role in explaining other features tend to be seen as more important. Finally, taxonomies are interconnected in complicated criss-crossing patterns, both because concepts are related to other concepts embedded in different lexicons, and because the features used to distinguish between nodes in one taxonomy are themselves nodes in other taxonomies.

An issue that has remained opaque in the literature is how conceptual structures relate to communities and individuals during the process of conceptual change.

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Kuhn originally expected that members of a scientific community must share the overall lexical structure enabling them to pick out the same referents for the same terms, while the criteria by which they do so can differ. Thus, for him overlapping conceptual structures were linked to incommensurability. In similar ways, most of the literature on conceptual structures has been concerned with communities of scientists whose members have identical or highly similar areas of expertise, and the focus has been on how an initially shared conceptual structure may change over time.[1] In contrast, the concern here will be on groups or communities whose members have non-identical areas of expertise and therefore have only overlapping conceptual structures, and I shall focus on conceptual developments that happen in the overlapping area.[2] Based on this conceptual analysis, I shall argue that mutual conceptual dependencies may enable a group to adopt views that are irreducibly the group's views.

CONCEPTUAL PROCESSES INVOLVED IN JOINT ACCEPTANCES

As described above, existing accounts of joint acceptance reveal very little about the conceptual processes through which a new claim is collectively formed and eventually accepted jointly by all group members, and how this conceptual development is related to distributions of conclusions drawn from the collected evidence as well as to distributions of defense requirements.

Early analyses of knowledge integration in research indicated that some forms of interaction provide research outputs that are the common intellectual property of the entire group, while other forms produce research outputs that are the intellectual property of individuals only (Rossini & Porter 1979). However, this work did not address what it means for the output to be the common intellectual property of the entire group or what implications the fact that this research output is property of the entire group might have.

Recently, work in cognitive science has started to address how concepts and cognitive processes can be distributed within groups. This work analyzes the cognitive processes taking place in distributed cognitive systems that consist of people combining knowledge not initially shared and that produce cognitive outputs that none of the individuals involved in the system could have produced alone (see, e.g., Giere and Moffat 2003; Hutchins 1995; Nersessian 2005; Nersessian 2006). However, these analyses have either remained unconnected to the discussions of joint acceptance or have been guided by skepticism towards the ontological implication of the notion of joint belief (Giere 2007). Hence, an important step in combining work on joint acceptance with work on distributed cognition will be to examine when collective cognition is founded upon joint commitments.

To provide the basis for such analyses, I shall introduce a detailed case study on the work performed by a number of closely interacting groups as their research developed from the initial formulation of a claim that a new phenomenon had

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been discovered until a series of increasingly severe anomalies led to a revision of the claim and the discovery of another new phenomenon. Thus, the case will cover two closely related aspects of joint acceptance in science, first, the formation of the joint acceptance of a new claim, including both the conceptual processes involved in forming the claim and the processes involved in arriving at its joint acceptance, and second, the obligations to defend a scientific claim that follow from the act of jointly accepting it and the possibility for the group to later change its position with respect to the jointly accepted claim.

Formation and Acceptance of New Scientific Claims

Gilbert's original account of the formation of the joint acceptance of some claim p by a group was based on analyses of everyday situations, like a poetry reading group jointly accepting an interpretation of a poem, or a university committee jointly accepting a policy. One of her aims in these analyses was to show that a summative account of the group's beliefs would not be able to explain scenarios such as group members being personally inclined to disagree but nevertheless deciding – for some reason or other – to let a certain view stand as the collective view of the group. While situations similar to these certainly exist in science, the focus here shall be on different kinds of scenarios, namely those in which the scientists in a group are dependent on each other as experts in different fields for combining the conceptual information necessary to form a position on the matter. I shall examine when this is a collective process leading to a joint position.

In his case study on high-energy physics, Staley (2007) describes how members in a collaboration may jointly accept a new scientific claim in a heterogeneous consensus in which there is agreement on the conclusion despite disagreement in the group as to which evidential premises support the conclusion. Similarly, I shall argue that members in a collaborative team may jointly accept a new claim in a heterogeneous conceptual consensus in which there is a consensus to accept conceptual changes, such as the introduction of a new concept, despite differences between group members in their conceptual substructures. In the case study below I shall examine such a conceptual development, and I shall show how the notion of heterogeneous conceptual consensus can provide a conceptual underpinning for Wray's and Rolin's accounts of functional dependence and of distributed background assumptions.

Defense Commitments and Change of Position

One important implication of a commitment jointly to accept some claim that p is that "group members are personally *obliged* not to deny that p or to say things that presuppose the denial of p in their ensuing interactions with each other" (Gilbert 1987, 193f; italics original). In this way, the collective acceptances act as constraints on the possibility for group members to change their view or

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in other ways deny that p. This differs markedly from a summative account in which individual group members may unrestrictedly change their view. Those who collectively accept some given propositions understand themselves to be party to a joint acceptance of propositions as a body. If they speak contrary to the collectively accepted propositions, they have violated obligations that they had with others and they will be regarded as acting out of line. The other members of the party have the standing to take responsive measures, from mild rebuke to ostracism (cf. Gilbert 2000, 42).[3]

This indicates that a joint commitment can only be rescinded by the parties to it acting together to form a new joint acceptance of a revised view (40). This mutual obligation not to rescind individually the accepted claim seems to go at least some way to explaining the stability of normal science, but as indicated by Gilbert in her paper on joint belief and scientific change (2000), this leaves us with the question of how to account for scientific change.

Although Gilbert has argued that studies of science in general and of scientific change in particular need to be sensitive to the nature of collective belief, her own focus has been limited to arguing that including an account of collective belief in the analysis of science could explain the alleged importance of outsiders and neophytes in the process of science, an importance that other scholars have shown to be a chimera (see, e.g., Wray 2003). Further, even if it could be argued that outsiders and neophytes were more willing to buck the consensus, Gilbert does grant that they might not be the most potent in bringing about a new consensus, and she notes that "precisely how such changes occur, and one belief becomes established as the collective one in another's stead, is a good question," "worthy of study" (2000, 46).

One key to answering these questions has been laid out by Wray in his remark that "one who seeks to change the view of a plural subject must show how the obligation that the group has to accept the view is inappropriate. This may involve providing evidence that suggests that the collectively accepted view is false, but it need not" (2001, 326). Thus, an important aspect of understanding scientific change is to understand how new evidence that challenges a jointly accepted claim or the background assumptions that have formed part of its acceptance – in other words, an anomaly – is received in a group that has jointly accepted the claim, especially in the situation where the claim has been accepted on the background of a heterogeneous conceptual consensus with distributed background assumptions and defense requirements.

In the case study below, I shall examine how anomalies may challenge background assumptions that are distributed in a scientific group. I shall argue that in those cases where results are accepted in a genuinely collective process, the joint acceptance does act as a constraint on the possibility for the group to change its view, and that a new heterogeneous conceptual consensus has to be formed to accommodate the changed view of the group. Further, I shall use the case study to show that not all results produced in groups are of this character, and that studies

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of scientific change in groups have to be sensitive to both the question whether a claim is accepted by scientists with different areas of expertise in a process based on a heterogeneous conceptual consensus, and the question whether the claim is accepted jointly in a special collective process.

CASE

As a first step in developing an understanding of how conceptual substructures may be distributed in a group and related to an overall conceptual structure jointly accepted by the group in a heterogeneous conceptual consensus, I shall present a case study in some detail: research on induced radioactivity in heavy nuclei during the 1930s led to the discovery of transuranic elements, and later to the discovery of nuclear fission.[4] The case study displays scientific research in all its messy detail and it may seem to raise more questions with respect to joint acceptance and collective views than my account immediately answers. However, to reach a better understanding of joint acceptance in science, it is necessary to examine real cases of science closely.

Research on induced radioactivity started in 1934. At this time, nuclear physics was still a young research field and a new realm of phenomena opened up when researchers discovered that radioactivity could be induced in heavy elements when bombarding them with neutrons. The Italian physicist Enrico Fermi had previously been working primarily as a theoretician. He then began collaborating with his old friend, the experimental physicist Franco Rasetti, who knew how to prepare neutron sources. Fermi later asked two additional physicists, Edoardo Amaldi and Emilio Segrè, to join the collaboration, and finally also recruited the chemist Oscar D'Agostini to make the chemical separations necessary for identifying which elements were produced in the disintegration processes. Segrè much later recalled the division of labor in collaboration:

> We organized our activities in this way: Fermi did a good part of the measurements and calculations; Amaldi took care of what we now call electronics; and I secured the substances to be irradiated, the sources, and the necessary equipment. This division of labor, however, was by no means rigid, and each of us participated in all phases of the undertaking, proceeding at very great speed. It soon became apparent that chemical separations would be essential for the investigation, and we searched for a professional chemist who could help us. We were lucky: Oscar D'Agostino had been a chemist in the laboratory of Professor Trabacchi and at the time we began our neutron work he held a fellowship in Paris at the laboratory of Madame Curie, where he was learning radiochemistry. (1970, 74)

After a series of experiments in which the collaborators systematically irradiated all the elements they could get hold of, the group reported that for a large number of elements, neutron bombardment would produce unstable elements that emitted β-particles. The group then decided to concentrate on the heavy nuclei thorium and uranium, since their general instability might give rise to successive disintegrations.

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For uranium, which was by then the last known element in the periodic table, such a series of β-emissions would lead to elements that had never been encountered before, and Fermi's group attracted much attention when it reported that chemical analyses had shown that one of the decay products could not be any of the elements from atomic number 86 to 92 and that the group therefore expected it to be a transuranic element.

Some of the background assumptions used in this identification came from nuclear physics. Gamow had shown that if nuclear disintegration was viewed as a tunneling phenomenon, it was only particles up to the size of the α-particle that were energetically capable of tunneling through the potential barrier, and the produced elements therefore had to be close in the periodic system to the element bombarded. Hence, it was assumed that the physically possible decay products would lie within this narrow range and using this knowledge the analytical chemists could identify which precipitation processes could be used for the identification of the various elements. Thus, for each element produced, identified first by its half-life period, a number of precipitation experiments were performed to establish its chemical characteristics and thereby its position in the periodic table. Although it was at the time uncertain whether there would be a second series like the lanthanides in the periodic table, it was still assumed that the transuranic elements would chemically resemble the transition elements and the chemists conducted their chemical identifications of the produced elements on this assumption. The results of these chemical analyses were then combined with measured data on the cross-sections of the reactions in an effort to establish a pattern of nuclear reactions that made sense within the accepted physical theory.

This work relied on physicists and chemists each bringing with them a set of background assumptions necessary for reaching the conclusion: the physicists assumed on the background of nuclear physics that the nuclear changes would be small, and that assumption limited the range of possible elements that could be produced. The chemists assumed that uranium and neighboring elements were transition elements and that the transuranic elements would therefore chemically resemble the transition elements rhenium, osmium, and so forth. That provided some of the important features by which the produced elements could be identified. In this way, the concept of transuranic elements was related both (i) to a physical conceptual structure related to possible nuclear decay processes that limited the conceptual range of possible elements to look for, and (ii) to a chemical conceptual structure related to the periodic table of the system which specified the chemical characteristics that could be drawn on in identifying the transuranic elements. The actual identification of transuranic elements thus drew on both underlying structures, and it was their combination that provided the background for concluding that transuranic elements had been produced. The conclusion drawn here could only be drawn by combining the cognitive resources of both physicists and chemists.

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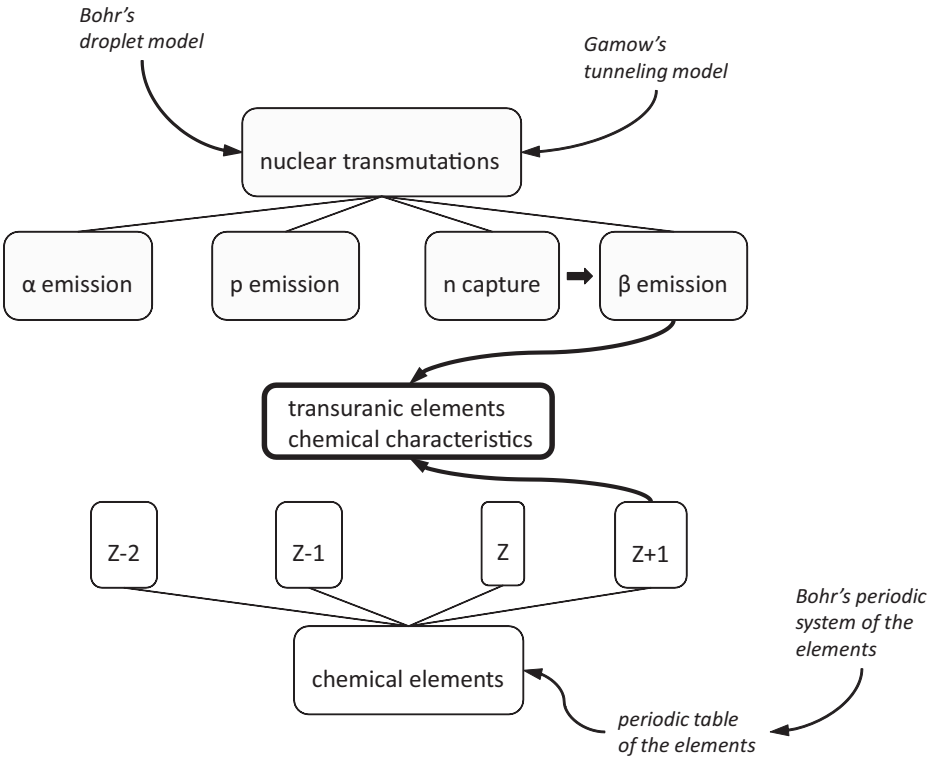


Figure 1. Based on Gamow's tunneling model and later Bohr's droplet model, physicists assumed that induced radioactivity had to be a transmutation process in which only small particles could escape the nucleus. Chemists assumed that the transuranic elements were transition elements. This assumption had its background in Bohr's periodic system that did predict the existence of a second series like the lanthanides, but also predicted that it would begin somewhere after uranium. Identification of the transuranic elements was made on the basis of the two assumptions together.

The claim to have produced transuranic elements is clearly collective cognition in the sense that multiple people combine their individual knowledge to produce a cognitive output that none of them could have produced alone. But the question remains whether it involved a joint commitment.

As we would expect in cases of joint commitment, there seems to have been a high degree of mutual trust in adopting and relaying results. For example, Segrè recalls how results produced by some members could be communicated by others and how results later seen as mistakes would be seen as a joint embarrassment:

> [Amaldi and I] thought we had found another clear example of the (n,γ) reaction in aluminum and immediately communicated this result to Fermi, who was still in London. He mentioned our experiment at one of the meetings. Shortly after this I caught a cold and could not go to the laboratory for several days, but Amaldi repeated our experiments and found a different decay period for irradiated aluminum, which indicated that our

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> so-called (n,γ) reaction had not occurred. This was hurriedly related to Fermi, who was angry and embarrassed at having communicated an erroneous result. On his return to Rome he scolded us for our apparent carelessness. We were not only unhappy, but confused, because we could find no fault with the various experiments that gave contradictory results. (1970, 78)

It does seem from such sources that the process of collective cognition involved in this case displays the features characteristic of joint commitments, such as each party being answerable to all for any violations of the commitment, being committed to promote the object of the joint commitment to the best of their ability, and being committed to constitute a single body that accepts the claim about the discovery of transuranic elements as its own.[5]

It has been suggested that Gilbert's requirement of an open expression of the commitment jointly to accept a claim can in science simply be tracked through the jointly published papers. However, this view may be too simple. In this case, the research was reported in a series of articles from the group in Italian. Significantly, not all the group members were co-authors of all the publications. Rather, the publications displayed a criss-crossing pattern of co-authors. The English review paper in the *Proceedings of the Royal Society of London* was, however, co-authored by all the group members, while the two earlier, brief reports in *Nature* were authored solely by the group leader, Fermi.[6] Further, it should also be noted that the mutual trust in adopting and relaying results did not necessarily impede the possibility for the group to change its views. On the contrary, the quote above also shows that *some* claims – particular details on particular processes, but not overall claims on the general existence of the process type – could be changed on the basis of new experimental evidence. These changes could then subsequently be conveyed by testimony to other group members not participating in the experiment in question. Thus, in this kind of group-based research, there both may be (i) results that are obtained through a process of joint acceptance and that need similar collective processes to be changed, and (ii) results that may change by individual group members changing their view and conveying their results to others.

Other groups of scientists soon began pursuing Fermi's line of research, among them a group in Berlin consisting of the physicist Lise Meitner and the two chemists Otto Hahn and Fritz Strassmann. The groups that entered the race to discover new transuranic elements included both physicists and chemists who worked closely together in examining the decay series and identifying the elements that were produced in the process. Several sources indicate that there was a division of labor among the physicists and the chemists compatible with a functional dependency in which different group members provided different areas of expertise. For example, Segrè recalled in an oral history interview how the physicists in the group felt that D'Agostino did not understand much physics:

> …D'Agostino, who was our chemist; he was rather ignorant and he would say, "Well, radioactive potassium is going to behave like radioactive sodium because of the periodic system." And we would tell him, "But now, look, the periodic system is in the electrons

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> and not in the nucleus." And he would say, "Well, but there will be a periodic system also in the nucleus, and it will be the same." And Fermi would smile and say, "Well, you see D'Agostino tells you." (Segrè 1967)

Likewise, it is reported that the physicist Meitner said to Hahn, her main collaborator: "Hahn, dear, of physics you understand nothing" (Sime 1989, 373). Thus, although in the work on transuranic elements concepts and assumptions from physics and concepts and assumptions from chemistry merge and have to be drawn on simultaneously by the groups in their interpretations of experimental results, it is important that these concepts and assumptions remain distributed among the various group members. This distribution of expertise in the groups would later be seen to be related to the distribution of defense requirements as anomalous experimental results led to querying of the jointly accepted results.

As the groups moved on, it turned out that many results were complex and required a variety of new hypotheses to be explained. As anomalies accumulated, it became increasingly difficult to integrate them all into a picture that made sense and it was reported in several publications that the results were troubling and difficult to reconcile with standard concepts of the nucleus. A number of physicists suggested various additions to the nuclear models to explain some of the results. In this way an additional layer of alternative conceptual developments was available to the community, without being part of the background assumptions underpinning the core joint acceptance of transuranic elements and their identification. For example, von Weizsäcker's theory of isomers was adopted by the group to explain some particular anomalous results, but was also questioned as additional anomalies appeared. As in Fermi's group, where some results were initially produced by some group members and then accepted by others, a similar pattern can be seen surrounding the Berlin group. Physicists following the group's work produced additional results that may or may not then be accepted by the Meitner-Hahn-Strassmann team, but the acceptance of such results is a stepwise diffusion into the larger community, where new parties accept the result individually and also have the option later to reject them individually. Thus, contrary to what Rolin suggests, individual scientists who express their willingness to participate in an already established joint commitment on a claim by merely using this claim in their own reasoning do not necessarily become party to the joint commitment in the sense that they have to be involved in a rescission. As Gilbert (2003) has emphasized, in order to establish a joint commitment, the dependent individual commitments are interdependent and have to be established together, and this only happens in the core research group itself.

After four years of research, the anomaly appeared that finally led to the discovery of nuclear fission. Hahn and Strassmann had identified a particular decay product as radium in a precipitation process where it behaved like the much lighter element barium. However, they soon discovered that they could not separate the product that they assumed to be radium from its barium carrier. The element they had produced did not just behave chemically *like* barium, it *was* barium. This meant

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that the original nucleus had not transmutated into another heavy nucleus, but had instead split into two much lighter elements. In a series of letters to Meitner, who had fled from Germany, Hahn wrote that although he knew that it was ruled out by the laws of physics, as a chemist he had to conclude that the nucleus had been divided. However, knowing that this would challenge one of the important physical background assumptions, he asked if Meitner could come up with an explanation:

> But we are coming steadily closer to the frightful conclusion: our Ra isotopes do not act like Ra but like Ba … I have agreed with Strassmann that for now we shall tell only you. Perhaps you can come up with some sort of fantastic explanation. We know ourselves that it can't actually burst apart into Ba. (Hahn to Meitner, December 19, 1938, quoted in Sime 1996, 233)

Thus, it is clear how the defense requirements were distributed in the group. When the chemists realized that their results were anomalous and seriously challenged basic assumptions made by the physicists, they knew that the jointly accepted results regarding transuranic elements were challenged, and also that it would require collaboration between physicists and chemists in order to develop an explanation that could be jointly accepted by both parties. It required a new heterogeneous conceptual consensus to fully dissolve the anomalies they had encountered.

Meitner replied promptly to Hahn's letter, saying that although a breakup of the nucleus seemed unlikely to her, "in nuclear physics we have experienced so many surprises, one cannot unconditionally say: it is impossible" (Meitner to Hahn, December 21, 1938, quoted in Sime 1996, 235). Hahn and Strassmann wanted to proceed quickly with the publication of their startling result and submitted a manuscript that, as Sime has noted, "displays the duality that characterized the uranium investigation: the radiochemical data were firm, the physical meaning so strange that Hahn wavered between confidence and disbelief" (Sime 1996, 235):

> Because of the experiments briefly outlined, as chemists we ought to change the designations in the scheme above and insert the symbols Ba, La, Ce instead of Ra, Ac, Th. As "nuclear chemists" in certain ways closely connected to physics, we cannot yet set up our minds to make this leap that contradicts all previous experience of nuclear physics (Hahn & Strassmann 1939, 15, my translation).

Thus, although they published their anomalous results, this publication did not have the character of a unilateral retraction of the results on transuranic elements. This clearly shows that both parties to the heterogeneous conceptual consensus underlying the transuranic results needed to be involved in finding an alternative explanation of the experimental results.

Meanwhile, Meitner discussed the results with the physicist Otto Frisch. While Frisch was initially sceptical about the strange result and suggested that Hahn might be mistaken, Meitner trusted Hahn's expertise. From Frisch's later recollections of their conversation, it can be seen that the group members' mutual trust of each

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other's expertise was an important factor in their commitment to jointly accepted views, both when they were formed and when they were questioned:

> The suggestion that they might after all have made a mistake was waved aside by Lise Meitner; Hahn was too good a chemist for that, she assured me. (quoted in Sime 1996, 236)

On the basis of a new model of the nucleus that had been advanced by Bohr two years previously and that treated the nucleus as an oscillating droplet, Meitner and Frisch offered the explanation that when energy is added by neutron bombardment, these oscillations could become so violent that the drop would divide into two smaller drops. In this work they also combined their different areas of expertise in mass defects and surface tensions, respectively, and as Frisch later recalled when describing his collaboration with Meitner, their contributions went seamlessly together:

> I'm trying to recall who did what, and that of course isn't easy. But it really was all very much give and take. I believe that it was either Lise Meitner or me who figured out roughly the energy that would be given to two such fragments if they were pushed apart after having separated, pushed apart by that electrostatic field, and the results are well-known – something like 200 MeV. And then Lise Meitner, who had the packing fractions in her head, worked out the energy which one might expect to be liberated and came up with a very similar number. That really got us excited because now we could see that it all hung together (Frisch 1967).

The new discovery had far-reaching consequences for all the previous results on transuranic elements. In the correspondence between Hahn and Meitner in the weeks following the discovery, they came to the conclusion that the new result required that they retract the majority of their previous results. Not only was this a joint conclusion, it also had to be done as a joint act, just as the publications that they were now retracting had once been published as a joint act. As Meitner wrote to Hahn:

> You understand, of course, that the question of the correctness of the transuranes has a very personal aspect for me. If all the work of the last 3 years has been incorrect, it cannot be determined from just *one* side. I shared the responsibility for it and must therefore find some way to participate in the retraction. A joint retraction is surely not feasible [for political reasons], so we should consider *simultaneous* statements, one from the two of you and one from me (the latter perhaps in *Nature*). (Meitner to Hahn, January 1, 1939, quoted in Sime 1996, 240)

Once the results were published, they were quickly assimilated by the rest of the scientific community. The other groups working in the area realized the challenge to their own results on transuranic elements and had to produce retractions. For example, Fermi heard about the new process shortly after he had received the Nobel Prize for his production of transuranic elements and inserted a footnote in the printed version of his Nobel speech stating that the new discovery made

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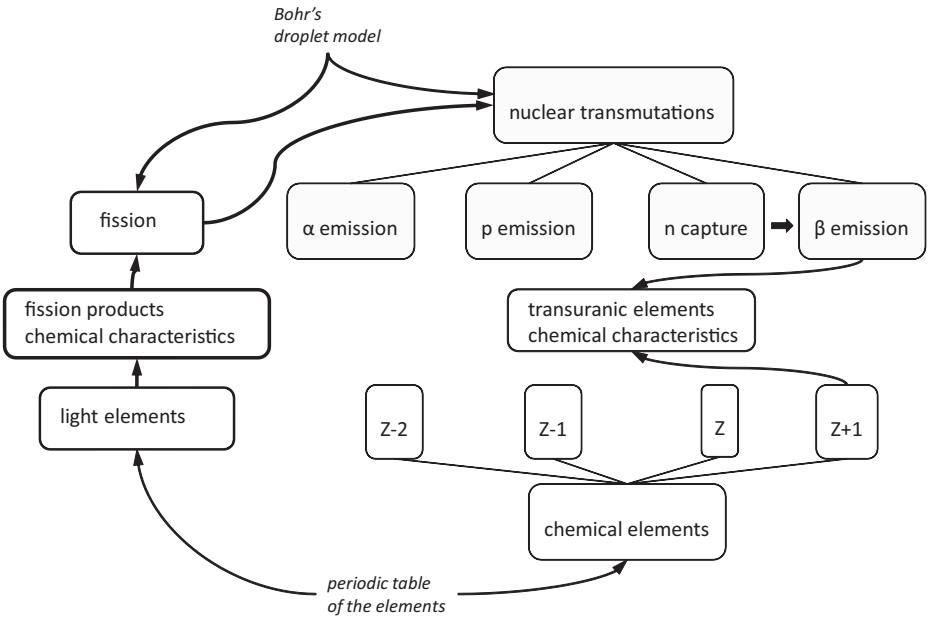


Figure 2. Chemists realized that the produced elements were light elements, and that this result questioned the jointly accepted transuranic elements. To introduce an alternative, it was necessary to change physical background assumptions in a way such that a division of the nucleus could be explained. Physicists realized that Bohr's droplet model could explain this fission process. However, the introduction of this new concept questioned all previous identifications, since they had been based on the assumption that only transmutations of the nucleus into the nucleus of a neighbouring element were possible.

it necessary to re-examine all the transuranic elements since many of them might be found to be fission products instead. Thus, although the different groups that had worked on transuranic elements had referred to each other's work and used those results in their reasoning, each individual group behaved like a social atom during the retraction phase and retracted results jointly rather than as individuals. The larger community of scientists working on the same issues did not display this characteristic of a social atom.

PHILOSOPHICAL IMPLICATIONS

As the case study shows, in order to understand how joint acceptance works in science, we need to understand the conceptual underpinnings of the changes of view, including how conceptual substructures can be distributed in a group and related to an overall conceptual structure jointly accepted by the group in what I have here called a heterogeneous conceptual consensus. It can be seen from the case study that the claims to have produced transuranic elements is an example of collective cognition in the sense that multiple people combine their individual

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knowledge to produce a cognitive output that no individual could have produced alone and which can be characterized as a heterogeneous conceptual consensus.

At the same time, the case study also shows complications that available accounts of joint acceptance in science do not fully capture. One requirement proposed by Gilbert for joint acceptance that has been widely adopted is that the parties involved openly express their conditional commitment to it, and they see joint publications as the major form of open expression of commitment (see, e.g., Rolin 2008, 122; Wray 2007, 344). Thus, it is often merely assumed that the relevant groups have a simple group structure with a clear sense of the group's members over time. But, as the case study shows, the structure of research teams can be much more fluid, with series of papers being published with criss-crossing patterns of co-authors. There seems to be a core of people who take joint responsibility for the collection of results. But more than authorships of individual papers is necessary to identify parties to joint commitments.

Finally, it can be seen from the case study that some claims are accepted jointly by the group members in the sense that the group members all are committed together to accept the claim in question and obliged not to violate it. The joint acceptances worked as constraints on the actions and thoughts of the members of each of the research groups in the sense that specific changes could not be made unilaterally by individual group members. The conceptual revision of adding nuclear fission and amending previous results accordingly could only be carried out through a new joint acceptance made by Hahn, Strassmann, Meitner, and Frisch. In this joint acceptance they brought together bits of knowledge that they each individually possessed and that were all jointly needed in order to introduce the new concept of fission. In this way, the new discovery can be seen as a new heterogeneous conceptual consensus in which all members agree on a change to the overall conceptual structure in the form of a new concept, although their individual conceptual substructures differ. Similarly, and in accordance with the characteristics of a joint commitment, it was seen that it was only with the joint acceptance based on the bringing together of their different pieces of knowledge that they were able to retract their previous results on transuranic elements as a result of introducing the new concept of fission. Thus, this study has indicated how work on distributed cognition and work on joint commitments need to be combined. However, more work is needed to examine when distributed cognition results in a joint commitment to accept a new claim and when the cognitive output has other forms.

ACKNOWLEDGMENTS

This work forms part of the project "Philosophy of Contemporary Science in Practice," supported by the Danish Council for Independent Research. I would like to thank Claire Neesham, Susann Wagenknecht, and K. Brad Wray for valuable

https://doi.org/10.3366/epi.2010.0206 Published online by Cambridge University Press

comments on earlier versions of this paper. I thank Isaac Choi for his assistance with the figures.

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NOTES

1 For prominent examples, see, e.g., Nersessian (1989), Nersessian and Resnick (1989), or Thagard (1992).

2 Focusing on groups whose members have non-identical areas of expertise, the analysis is, in a sense, primarily directed towards interdisciplinary science. However, non-identical areas of expertise may still be overlapping in various degrees, and I see monodisciplinary and interdisciplinary science as endpoints of a continuum rather than as two clearly distinguishable forms of science.

3 In accordance with this view, psychologists of science have reported that voicing minority opinions may elicit a range of interpersonal repercussions from the majority, depending, among other factors, on the majority members' commitments to their view (see, e.g., Rosenwein 1994).

4 For details on this history, see, e.g., Andersen (1996, 2009), Sime (1989, 2000), Weart (1983).

5 See Gilbert (2003) for a detailed analysis of the central features of joint commitments.

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6 As already pointed out by Wray (2007), journal editors generally do not consider co-authorship as corporate authorship. However, recent cases of scientific misconduct by individuals publishing jointly with others – as, for example, the case of Jan Hendrik Schön – are likely to attract renewed attention to this issue. For example, starting in 2009, the journal *Nature* has adopted the policy that authors are required to include a statement of responsibility in the manuscript that specifies the contribution of every author.

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