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The Importance of Describing Participatory Design *in the Making*

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DESCRIBING DESIGN IN THE MAKING

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Abstract

This article is a call to describe Participatory Design (PD) projects *in the making*, *i.e.* to show how the heterogeneous elements in the field are gradually organised in a participatory manner as the projects progress. It is based on two arguments. The first is a negative argument. Very often, PD projects are not described *in the making*. As a result, the landmarks to be used to evaluate them remain unclear or invisible. The second argument is of a more positive nature. The articles that do describe projects *in the making* enable landmarks to be defined that can be effectively used to evaluate PD projects. The notion of *emerging groups* is one of these landmarks.

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1 Evaluating Participatory Design is no easy task

Since the beginning of the 1980s, most research in the field of Participatory Design (PD) seems to have concerned the development and improvement of PD techniques (Kensing *et al.* 1998, Muller 2002). The most representative are as follows:

- **Future workshops** (Kensing and Madsen, 1991). These are aimed at stimulating ideas in order to change a situation considered to be problematic. The participants first make a list of the negative aspects of the situation, followed by a list of its ideal characteristics. They then draw up a plan to bring about the necessary changes. Throughout the workshops, the facilitator invites the participants to use metaphors to describe the different situations.
- **Cooperative prototyping** (Bødker and Gronbaek, 1991). This is based on work groups in which the designers and future users of a tool test an alpha version. The idea is to test the tool as early as possible under real conditions of use in order to rapidly identify its limits and make the necessary corrections. The work is organised so that there is rapid alternation between the test phases and the development phases of the tool.
- **Cooperative mock-ups** (Ehn and Kyng 1991, Brand 2006). The work sessions in this case are similar to those of cooperative prototyping. The difference is that the participants work on a version that is very far removed from the “real” tool e.g. a cardboard box to represent the functionality of a future printer. The advantage of this technique with respect to prototyping is that the participants gain “hands-on” experience which stimulates their creativity.
- **User scenarios** (Carroll and Rosson 2002). Here, fictive characters are used to describe the future uses of a tool as accurately as possible. The idea is to develop a scenario for each stage of the design in order to explore the advantages and disadvantages of each of the options envisaged. The first scenario is thus used to analyse the initial situation. It is then adapted and transformed to explore alternative concepts, available technology, appropriate interfaces, and so on.

- **Design games** (Brandt and Messeter 2004, Ehn and Sjögren 1991). These are used to collectively explore the different aspects of a design situation. They combine material from field studies, such as videos, photographs and lists of available tools, with game materials, such as playing cards, game boards and Lego pieces. The players take turns to use the game materials to develop different visions of the initial situation and explore ways in which it can be changed.
- **Ethnography** (Hughes *et al.* 1994, Harper 2000). With this technique, an activity is observed in its day-to-day environment and described as accurately as possible. The aim is to give a detailed account of the specific experience of the people who carry out the activity. Once the ethnographic analysis has been completed, it is used to stimulate design ideas and anticipate their repercussions in the field.

All the above techniques share the common aim of creating hybrid spaces (Muller 2002), that is, spaces that recombine elements from different worlds. Each involves the displacement of elements from one world to another: the use of metaphors in future workshops displaces the language normally used by the participants; prototyping and cooperative mock-ups transfer the prototype from the computer scientist's world to that of the user; the user scenario shifts the technical description of requirements to the world of fiction while ethnography literally transports the realm of future users into that of the designers. This hybridation is what opens up the world of design to the non-specialist and makes it participatory.

The techniques for participatory design are therefore well-documented, but what landmarks can be used to evaluate their success? How do we know if a design project has been sufficiently opened up to non-specialists? In this respect, opinions diverge. For some (Bødker K. 2004), a PD project can be considered successful when representatives of the different "impacted groups" participate in the design work and arrive at a prototype that satisfies all the participants. For others (Beck 2002), a PD project is successful when it enables "dominated groups" to decide for themselves which tools they need. We are not in a position to decide which of these two criteria is more appropriate. We will simply observe that, in each case, their application poses a problem. How is it possible to ensure, first of all, that all the "impacted groups" are correctly represented? What is to be done with informal, loosely defined or emerging groups - in fact, any group whose representation poses a problem? What is to be done in situations in which the identification of "dominant groups" and "dominated groups" is ambivalent or impossible? To which group should preference be given? It would seem, in fact, that Participatory Design lacks the landmarks needed for its projects to be properly evaluated.

We propose to explore the reasons behind this problem and how it can be overcome. We will first show that the lack of landmarks stems from the fact that very little research exists that describes how PD projects are developed in the field. We will see that most of the articles devoted to PD techniques only give a very partial account of the field tests in which the techniques are defined and redefined. We will also see that the research papers deploring the shortcomings of the articles devoted to PD techniques generally impose analytical frameworks that make it impossible to understand how the projects are developed in the field. We will then show that descriptions of participatory settings *in the making* are necessary to identify appropriate landmarks. We will see how the step-by-step description of a project

conducted in a law firm has brought to light a new landmark: the constitution of *emerging groups*. We will then make a detour via STS (Sciences and Technical Studies) to show how a description of the strategies developed by groups of lay persons in order to collaborate with scientists has led to the identification of landmarks that can be used to organise this type of collaboration.

2 Reporting on *ready made techniques*

Most of the articles devoted to PD techniques do not sufficiently report on the field tests in which the techniques are developed. The reports can be divided into three categories:

1 - First, articles in which testing of the technique is simulated, that is, in which a fictive situation is developed in order to illustrate how the technique is applied and the results achieved. Examples of articles based on this process include Rosson and Carroll (2002) on the use of scenarios, Blythe and Wright (2006) on pastiche scenarios¹ and Kensing and Madsen (1991) on future workshops. Let us take the following example.

Rosson and Carroll (2002) present the different steps of *Scenario-Based Design* by means of a fictive case study – the design of a website for a science-fiction club. In this particular case, the authors explain, the first step in the design project is to analyse the advantages and drawbacks of the meetings usually held by the science-fiction club. The authors then present a scenario based on the experience of a club member when she goes to one of the meetings. The scenario can be summed up as follows. *Sharon is a third-year psychology student at Virginia Tech. She has an examination next day but has nevertheless decided to go to the club meeting at 7 pm. Dinner takes a little longer than usual. Sharon misses her bus and arrives a quarter of an hour late. When she arrives, she is happy to see that the topic she is interested in – the Zeroth law – has already been the subject of a highly animated discussion between Bill and Sara. However, she cannot immediately know what has already been said. So she sits at the back of the room in order to pick up the thread of the discussion. She will then be able to present her reflections on the subject when the session resumes after the pause.* This brief scenario, explain the authors, provides a practical illustration of some of the advantages and disadvantages of the meeting system: (i) it allows both verbal and non-verbal communication but limits the possibility for each participant of being involved in several conversations at the same time; (ii) it encourages a feeling of familiarity among the participants but obliges each of them to go to the meeting venue. This scenario thus helps trigger a reflexion on which characteristics would need to be maintained or modified in an electronic version of the meeting. The same scenario can then be reworked in order to explore the different design options. Would it be better, for example, to create a forum or would a virtual reality site be more appropriate? Should the interface be rich or minimalist? The main advantage of scenario-based design is that it presents each of the design steps in a form that can be easily understood by all the parties concerned.

¹ The idea behind pastiche scenarios is to borrow a character from fiction and write a scenario in which the character is a user of the object that is being designed. According to Blythe and Wright (2006), this technique enables user experience to be explored more deeply.

Rosson and Carroll's article is therefore a perfect illustration of the *potential* contribution of scenarios to the design process. However, it says nothing about what *actually* happens when the scenario is integrated into a *real-life* design situation. What would happen if there was a real science-fiction club with a limited budget and internal dissension? What resources in terms of people and time could be negotiated to produce the scenario-based design? What would Sharon, Sara, Bill and the others say if they were real people? How could they be convinced to contribute to a design research project? These are just some of the questions not addressed in the article.

2 - The second case corresponds to articles that describe real-life situations in minute detail but stop when testing of the technique begins. This category contains articles that present the ethnography of an activity and show how it can be used for future design work. It can be argued that the lessons to be learnt from attempting to incorporate ethnography into design work lie not so much in the ethnography itself (or in its implications) but in what it can produce *within* the design process, how it combines with all the elements that enable situation A to become situation B. Yet it is this last question that is generally ignored. This is obvious in a number of articles published by the CSCW². Notable examples are Harper *et al.* (1989, 1996), Heath and Luff (1992) and Pycock and Bowers (1996). Let us look at the second article in more detail.

Heath and Luff (1992) give a detailed presentation of the activity of a London underground line control room. They first describe the jobs of the people who work there (the line controller and divisional information assistant) and the different tools they use (fixed line diagram, train timetable, telephone, public address system, etc.). They then present the way in which the controller and information assistant coordinate their everyday activities: (i) how they are each constantly aware of the other's activity while carrying out their own tasks e.g. by holding their headset to one ear only; (ii) how they try to make their activity visible to the other, e.g. by speaking loudly on the phone or talking out loud; and (iii) how they regularly ensure that the other person does not miss any important information, e.g. they indicate the arrival of new information by snapping their fingers if the other person is on the phone. According to the authors, these informal means of communication are used to remove the barrier between the tasks performed by the controller and those of the information assistant. They form a redundant system that is essential if the control room is to function correctly. As the authors point out, these observations have significant implications in terms of the design of future control rooms. They underline the importance of respecting (and even encouraging) informal communication between the persons present in the control room. Thus, the idea, for example, of replacing the line diagram shared by the controller and information assistant with individualised line diagrams cannot be recommended. It would make it difficult for each of them to continually evaluate how the other person interprets the information on the diagram. On the other hand, it would be interesting to develop a tool so that the controller could share any adjustments made to the timetable with all the personnel outside the control room at the same time. This would enable the redundancy observed inside the control room to be extended to the outside.

The article by Heath and Luff that we have only described very superficially is highly informative about what happens in an underground control room. It also

² Computer Supported Cooperative Work.

perfectly illustrates what an ethnographic description can *potentially* contribute to design work. It does not, however, say anything about what *actually* happens when an ethnographic study is used for a design project. How and with whom did the authors negotiate access to the field and how will their observations be used? To what extent did that restrict the study they produced? How was the study received by the various people in the field? Was it taken into account, and by whom and how, to redesign a control room? All these questions remain unanswered. The article, in fact, says practically nothing about what needs to be done by the people concerned – starting with the authors themselves – to adapt the ethnography to a design project.

3 – The third category is perhaps the most important from a quantitative viewpoint. It includes articles that report on a real-life test of the technique but in which only certain parts of the test are recounted. This is the case, in particular, of articles whose structure tends to follow the different steps of the technique they are describing. These steps provide guidelines for selecting certain aspects of what occurred in the field to the detriment of others. This generally has two effects: (1) the efforts made to apply the technique in a particular way are not apparent and (2) it seems that the only reason for mentioning the field elements is to illustrate the technique. This category includes authors such as Bødker and Gronbaek (1991), Bardram (2000), Brandt and Messeter (2004), Ehn and Kyng (1991), Holtzblatt and Beyer (1993), Kensig *et al.* (1998), Christensen *et al.* (1998), and Simonsen and Hertzum (2008). Let us take an example.

Bødker and Gronbaek (1991) present a cooperative prototyping experiment aimed at designing a patient case record system for municipal dental clinics in Denmark. Basically, the article is organised as follows: (1) the authors first give a general presentation of cooperative prototyping and explain why the prototypes must be tested directly by users and what must be done to make that possible; (2) they then describe the context of the experiment: the participants' profession (dental assistants), the IT systems used at the clinic, the dental assistants' requirements in terms of IT tools, and the software used to make the prototype; (3) the authors present the two prototype evaluation sessions: the preparatory work carried out before each session, the changes made during the sessions according to the users' remarks, and the changes made after the sessions; (4) they describe the positive aspects of the experiment: the fact that the participants remained focussed on ensuring that the tool corresponded to their working conditions, that it was possible to modify the prototype during the session, and that new functionality could be identified; (5) they were able to identify the obstacles to be avoided: prototypes that were too complicated for users, too many time-consuming changes to the prototype during the session, unrealistic expectations on the part of users in relation to the prototype, etc.

This type of report provides an excellent user guide: it clearly explains the validity of the technique; it illustrates each of the steps by means of concrete, authentic examples; and it highlights both the strong points and the risks associated with the technique. A presentation of this type, however, tends to exclude some of the steps during which the technique was actually developed. By concentrating on the application phases of the technique (preparation phases and evaluation included), the report relegates the other aspects to the background. Practically nothing is known about the work carried out beforehand so that the experiment could take place. In this specific case, the authors simply indicate that they were giving an introductory IT class to dental assistants in a trade union training programme and that the experiment was conducted as part of the course (*Op. cit.*, 345). Many questions remain

unanswered. How did the authors go about negotiating the courses with the union? How were the participants chosen? How did the authors convince them to participate in the experiment? Did they try to extend the experiment by offering additional classes?

A common denominator of the three types of report outlined above is that they seem to ignore all or part of the field tests in which PD techniques are developed. In the first category, the author simply presents a fictive test of the technique. In the second, the report stops when the test has only just begun, while in the third, the author only selects tests that illustrate the different steps comprising the technique. These articles thus give the impression that the techniques and the fields in which they are applied are homogeneous. They do not show what was done by the participants to establish a link between the techniques and the heterogeneous elements in the field. In other words, a problem occurs whenever the technique is defined outside the field. The technique predefines the form in which the report is written, when the report should reflect the way in which the technique is shaped in the field. In other words, in these articles, the technique was defined by the authors alone. The field actors can always point out the limits of the technique, but their main role is to apply it. The way in which they helped to redefine the technique so that it could be applied, is not discussed³. In fact, these articles report on *ready made* techniques and not techniques *in the making*. The landmarks that enable the success of the projects to be evaluated remain invisible.

3 Questions that *homogenise* the field

We are obviously not the first to point out that research articles on PD techniques have certain limitations. Authors such as Beck (2002) and Greenbaum (1996) observe that these articles rarely address the political aspects of design situations, while others such as Rönkkö (2008) regret that they do little to determine the influence of socio-political factors on the application of PD techniques. Let us take a closer look at the arguments developed by these authors.

1 – Beck (2002) observes that most of the more recent studies eschew the political dimension of PD, i.e. they ignore the political context in which IT projects are developed. Yet Beck reminds us that dominance patterns were at the centre of a large number of projects that led to PD. She mentions, for example, collaboration between the Norwegian Computer Center and the Norwegian Iron and Metal Workers Union in the seventies. The aim was to train union representatives in computer concepts and language so that they would be able to defend the interests of workers more effectively when computer tools were introduced in the workplace. Beck also mentions the Utopia project, initiated in 1981, in which researchers closely collaborated with newspaper type setters to develop graphics software. The idea, this time, was to directly involve the workers in the design so that the final product would reflect their interests and not only those of the management⁴. This political dimension

³ There is something paradoxical about these articles. While they all promote participatory techniques aimed at involving all the actors concerned in a design process, they also seem to hesitate to fully involve the actors concerned when it comes to defining the participatory techniques themselves.

⁴ Here Beck is referring to Asaro's historical analysis (2000).

however seems to have virtually disappeared today. The author cites articles presented at the IRIS (Information Systems Research in Scandinavia) conference between 1995 and 1998. "Evidently, issues of interest to IRIS contributors include development of participatory design as a technique, e.g. looking at methods for carrying out PD in various settings, making PD systems more robust, discussing its relation to software engineering (...). However, framing research so as to make visible or to change power differences cannot be easily claimed as a key concern of a community." (*Op. cit.*, p.79-80). The decline of the political dimension of PD can no doubt be attributed to various factors⁵, but the main explanation, according to Beck, is that the notion of participation is quite simply inadequate. The participation of users in design work, she explains, does not guarantee that the interests of the dominated will be taken into account. PD therefore needs to be reconstructed, not on the basis of *user participation*, but on that of *questioning dominance patterns*: "Rather than participation, concern with power and dominance needs to be stated as the core of the research field of PD. Thus, analysis and development to be published as PD should be motivated in serving the dominated (and may or may not involve participatory design)." (*Op. cit.*, p.82-83).

We believe that Beck dismisses the notion of *user participation* a little too quickly. Admittedly, the notion is somewhat vague. It may well be that some of the methods that require user participation lead to subtle censorship of certain groups rather than letting their voices be heard. But that is not necessarily the case of all the so-called participatory methods. Everything depends in fact on the way in which *participation* is put into practice. As a result, to be able to judge this notion with impartiality, it must be possible to compare the various ways in which it has been implemented, that is, not only how it has been converted into techniques and methods, but more especially how these techniques and methods have been applied in the field. In other words, for each PD project, the many different ways in which the idea of participation has been turned into the concrete application of a PD technique⁶ would need to be described. The problem is that very few reports of this type exist. That, however, does not seem to bother Beck. She has no doubts about where the difficulty lies: *dominance patterns are at the root of the problem*. But surely this notion is as vague as that of *user participation*. Surely it, too, runs the risk of being associated with various practices some of whose effects will be deemed negative and others positive⁷. And what exactly do we know about the ways in which this notion can be turned into a design process? In fact, it would seem that Beck is simply suggesting replacing one notion with another even though we have very little idea of what they may produce in the field. In doing so, the author also sidesteps part of the issue that she is raising. When addressing power relations in design, we are also considering the

⁵ Beck mentions the following factors : software is becoming more and more standardised and its design increasingly piecemeal; it is being used more and more for leisure and is no longer confined to the work place; the unions no longer necessarily take a critical stance in the corporate world (*Op. cit.*, p.80-81).

⁶ It is obviously very difficult to predict what might result. For our part, we would nevertheless be surprised if the notion of participation – or more exactly some of its interpretations – did not prove to offer several elements that might be worth placing at the centre of PD (see below our analysis of the research conducted by Blomberg *et al.*, 1996).

⁷ See Shapiro (2005, p.34-35) for a discussion of the problems posed by *calling dominance patterns into question* when seeking to apply this as a design principle.

issue of power relations in PD situations. But how can we explore power relations in these situations without describing the controversy that surrounds the notion of *participation* and without going into other aspects such as the various interpretations proposed, the obstacles encountered and the changes that need to be made before the notion can actually be applied? In other words, surely it is in the different ways in which the idea of *participation* is shaped in the field that power relations can be detected in PD projects. Yet that is exactly what Beck ignores when she simply replaces the notion of *user participation* with that of questioning *dominance patterns*.

2 – Let us consider a second criticism levied at Participatory Design. Rönkkö (2008) regrets that very little research shows how "socio-political factors" influence the application of PD methods. To help fill the gap, the authors describe a case in which the restructuring of a company solved the problem that a PD method was initially supposed to address. Their article can be summed up as follows. The story takes place in a company that is developing a user interface platform for mobile phones. The company has an Interaction Design (ID) team whose work is to design new interface prototypes and test them on users. The members of the ID team have a problem: the engineers in charge of encoding the platform rarely take the ID team's work into account. To solve the problem, the ID team decides to use a method based on user scenarios called *Personas*. The idea is to make user needs both more concrete and more precise and thus force the encoding engineers to take them into consideration. Unfortunately, the ID team soon realises that it is impossible to apply the method. Specifications of a technological and marketing order are regularly added during the projects making it impossible to implement an approach based on user requirements. The method is thus abandoned. Retrospectively, however, it seems that another factor also contributed to preventing the method from being used successfully. During the same period, massive corporate restructuring placed the ID team at the centre of the company's data flow, reversing the balance of power between the ID team and the encoding engineers. Restructuring therefore provided a solution to the exact problem that *Personas* was supposed to help solve.

This is obviously only a brief summary of the situation. Yet it clearly shows the ambiguity produced by Rönkkö's article. We are told that restructuring of the company hampered the implementation of *Personas*. However, we are also told that the method was abandoned because of the constant addition of technological and marketing specifications. That being the case, what role did the restructuring actually play? How did it affect the decision to stop using *Personas*? The authors do not say. It is a little like describing how projectile A put projectile B off course without recording the exact moment of impact between the two projectiles. We begin to have doubts: was there really an *impact* – that of A on B – or did A and B just happen to *cross paths*? The article does not enable us to decide. Let us take the question a step further. How is it that Rönkkö's article does not manage to present the role played by corporate restructuring in the failure of *Personas*? We believe it is because the authors insist on treating the restructuring as a "socio-political *factor*". Let us explain. A factor is a statistical notion. It is useful when we know status A and status B and we *cannot directly observe* the mechanism that leads from A to B. The repetition of the event is then used to identify the factors that are statistically correlated with the change from A to B. This strategy, however, is counterproductive when the mechanism leading from A to B *can be observed*. It means that heterogeneous assemblies are treated as homogeneous sets, which prevents the point of contact between the different assemblies from being known. That is exactly what is

happening here. The authors first build set A, *Personas-as-a-way-of-reinforcing-the-ID-team's-influence*, then set B, *technological-and-marketing-specifications-as-the-reason-for-Persona's-failure*, and finally introduce the "socio-political factor", *restructuring-of-the-company-as-the-reason-for-the-ID-team-regaining-influence*. Since these three sets are perfectly separate entities, it is impossible to see how they can now be connected to one another. Paradoxically therefore, what the authors are trying to demonstrate – the influence of “socio-political” factors – is exactly what is stopping them from clearly showing how one thing can influence another.

The authors of the articles discussed above all share the opinion that there is something lacking in the PD literature⁸. This something, for Beck, corresponds to "dominance relations" while for Rönkkö, the missing link is “socio-political factors”. The problem is that by focussing on these issues, the authors disregard a large part of what is happening in the field. “Dominance relations” lead Beck to ignore the notion of *participation* and therefore the power struggle that underlies its definition. “Socio-political factors” lead Rönkkö to treat the field as a series of homogeneous sets, preventing him from showing that certain events are interconnected. These issues, in fact, tend to artificially homogenise what is happening in the field. They exclude any attempt to see how the people involved try to connect up the heterogeneous elements in the field. The articles that focus on these issues appear to be the exact reverse of those devoted to the techniques themselves. While the first present facets ignored by the second, they all leave a large part of the field outside the framework of their report. As a result, we are still no further advanced in our quest for landmarks that would enable the success of a PD project to be evaluated.

4 Reporting on projects *in the making*

There are nevertheless a few articles that show PD projects *in the making* such as Blomberg (1996) and Carroll (2000). Below is a brief summary of the paper in which Blomberg (1996) relates a project conducted with a law firm.

When the authors begin their account, some of the project components are already in place. The authors already have a precise idea of the techniques they are going to apply. They want to use ethnographic resources such as video recordings during the development phase and set up cooperative prototyping sessions with potential users. The structure of the project has already been determined. It will consist of a cooperative approach⁹ between the authors, who are the members of a research unit at Xerox, developers from another Xerox unit and potential users in the law firm. The authors know some of the characteristics of the products they are going to develop. The products combine electronic document search systems and paper document character recognition systems. As a result, the focus will be on the law firm’s documentary practices. The firm’s two main activities - business law and litigation –

⁸ Rönkkö et. al (2008) sum up this feeling well: “We have to complement method

centered research with approaches that open up for sensibility to precisely what is hidden when focusing on methods.” (Op. cit., p.78).

⁹ The idea of cooperation, explain the authors, is that the law firm does not remunerate the researchers for their work and that the researchers do not promise to provide a working system at the end of the project.

cover very different documentary practices, prompting the authors to explore them separately.

The documentary practices of lawyers in the field of business law are guided by an important principle: “avoid drafting anything from scratch”, that is, if a document exists that can serve as a template, it should be used to create the new document. For this purpose, most lawyers have set up a collection of “boilerplate documents” they can keep re-using. However, the authors quickly discovered that some lawyers devote more time than others to building up their collection and that these collections are then used as resources by the other lawyers in the firm. The authors soon made the acquaintance of M who has an alphabetical collection of several thousands of documents. M immediately showed interest in an electronic version of his collection that would offer advanced search options and shared access. Collaboration then began between the lawyer and the researchers. M agreed to let the authors leave a camera in his office so that he could be filmed when using his collection either when alone or with other lawyers. In the meantime, the authors copied part of M’s collection and began to develop the first prototype. After a few adjustments, the prototype was installed in M’s office and filming of M testing the new tool began. At the same time, the authors endeavoured to use the videos produced by M or themselves to enable developers who could not be present on the site to participate in the design work.

We will now take a look at the documentary practices on the litigation side. These are based on a legal process known as *document production* which consists in taking documents from clients' files and turning them over to the opposite side. During the first part of the process, the documents are sorted according to pertinence. Information on each pertinent document is then entered into a data base by a special litigation support service. It was the work conducted by this department that drew the authors’ attention. They then tried to develop a tool that would enable the department to render part of the data entry operation automatic. However, the researchers soon realised that there was a latent conflict concerning the way in which the work of the litigation support staff was defined. For some of the lawyers, there was a clear distinction between strictly routine work – that of the support staff – and strictly intellectual work - that of the lawyers. The support staff, some of whom contested this distinction, were soon joined by the researchers whose observations demonstrate the intellectual dimensions of the work carried out by the document analysts¹⁰. The authors then started to work on a prototype that would take these aspects into account. However, they were soon to learn that the senior management of the firm was seriously considering outsourcing its litigation support to the Philippines. Believing that the decision might be made without full knowledge of the work actually being carried out by the document analysts, the authors suggested that they present their work to the management. At the same time, the litigation support staff increased their productivity and lowered the cost of their services, thus securing their continued presence in the firm. Blomberg’s account stops there.

Our first observations about this report consist of a series of negatives. The authors do not focus exclusively on presenting PD techniques before they are applied, nor on a minute description of the working methods of potential users, nor on the

¹⁰ The authors explain, for example, that entering the date of a document often means choosing the most useful date for the lawyers from among several possibilities (when the document was written, signed or faxed).

different steps of the techniques during their application. Neither do they focus exclusively on “dominance relations” or the impact of “socio-political” events. This report has no pre-established form or programme. It simply recounts how the field components were configured and reconfigured throughout the project. This type of report is important because it offers no shortcuts and sets out to give a step-by-step account of the project. The intermediate steps can therefore be observed and compared and their pathways evaluated. The landmarks of a participatory project are now in sight.

It is interesting to observe the similarities that exist in terms of project construction between the case of the boilerplate document collection and that of the litigation support staff. The first thing to note is that, each time, the technological project leads to the creation of *emerging groups*, that is, groups which do not have a stable identity because their identity partly depends on what is being designed. Thus the project to scan the document collections of the firm’s business lawyers will necessarily have repercussions on M’s identity in the firm. Depending on the design decisions that will be made, M’s particular role with respect to the other lawyers will either be recognised or denied. The project to automate part of the work carried out by the document analysts will also have repercussions on their identity. Depending on the design decisions that will be made, the intellectual dimensions of their work will be denied or, on the contrary, recognised. It can thus be seen that the authors endeavour to provide the emerging groups with all the resources they need to have their particular identity recognised. M was provided with a camera so that he could film himself using his collection, with developers to discuss the implications of the recordings, and with a prototype. Likewise, the authors furnished the litigation support staff with precise descriptions of the intellectual dimensions of their work and a prototype based on those dimensions, and offered to present their work to the senior management of the firm when it was about to make a decision in ignorance of these dimensions. It can thus be seen that the aim of the project each time is to enable emerging groups to become fully fledged participants. The success of the project will then depend on the answers that can be given to questions such as the following: Is the emerging group able to explain exactly what makes it special in relation to others? Is it able to convey this properly to the other parties concerned? Is it in a position to negotiate with them as to the direction to be given to the design work?

We now need to understand the specific characteristics of emerging groups and their practical consequences. First, emerging groups must be distinguished from *potential users*. For example, in the case of digitisation of the boilerplate document collections, the potential users are all the business lawyers in the firm. However, their identity is not as important as that of M whose collection is like a small library. As a result, if the authors had merely conducted the prototyping sessions with a random selection of the firm’s potential users, the design would have led to an outcome that denies M’s particular identity. It is also important to clearly differentiate between emerging groups and *dominated groups*. If the authors had wanted their research to serve dominated groups, they would no doubt have worked with the litigation support staff, since they appear to be “dominated” by the lawyers, *independently* of any technological project. But would the authors have worked with M? Probably not. However, if the boilerplate document collections are digitised, it is obviously his particular identity which will be affected. Thus, being a *potential user* (or simply an *impacted person*) or a *dominated group* is not the same as having one’s identity intimately related to the outcome of a technological project. It is probably these

identities which are actually at the core of PD projects. They are the yardstick by which the success of the project can be determined.

It goes without saying that precaution must accompany this conclusion. To lend it more weight, Blomberg's account would need to be compared with that of other design projects. But our aim here is not to identify ready-made landmarks in order to evaluate PD projects. It is simply to show that it is possible to identify these landmarks. To do so, projects need to be described *in the making*, that is, it must be shown how the participants go about connecting up the heterogeneous elements in a project.

5 Landmarks for evaluating participatory settings

It should be noted that STS (Science and Technical Studies) has also attempted to define landmarks aimed at evaluating participatory settings. A pertinent example is the publication by Callon *et al.* entitled "Acting in an uncertain world" (2009). What is particularly interesting to us about the authors' work is that it is based on a series of articles describing science *in the making*. Let us therefore sum up the main steps of Callon's research.

1 – Callon *et al.* (p.75-104) first present a series of descriptions that show how science is developed in the laboratory. The descriptions cited are those of Latour (1984 and 1989), Callon (1986 and 1989), Rabeharisoa *et al.* (1999), Cambrosio *et al.* (1995), Houdart (2000) and Köhler (1994). By comparing these different descriptions, Callon *et al.* show that scientific research is based on three main "translations":

- *Translation 1* consists in reducing the "big world" to the "little world" of the laboratory. This means working not directly on "full scale" phenomena, but on traces left by these phenomena, so as to obtain smaller, simpler objects that can be more easily observed, cut up, combined and tested.
- *Translation 2* consists in setting up a research collective. This means bringing together both human resources (researchers, technicians) and non-human resources (machines, models, samples) and increasing the number of observations, calculations and objections until all the actors involved end up speaking with one voice.
- *Translation 3* consists in reconfiguring the big world to accommodate the objects developed in the laboratory. This means demonstrating the pertinence of laboratory work by setting up small "intermediate laboratories" that can reproduce and adapt to the big world what has been produced in the confines of the laboratory.

The authors explain that these three translations enable the specific characteristics of scientific research to be described without losing sight of the way in which it is connected to the rest of the world. These translations first emphasise the confines in which this type of research is conducted. To establish solid facts and produce tools that work, scientists need to be able to strictly control each of the elements of their research. The more sophisticated and the more precise the experiments are, the more they need to be protected from the outside world. This concern with control and therefore confinement can be seen at every stage of the process, i.e. when the scientists have to downscale the objects on which they are going to work, when they

select their colleagues and work tools, and when they set out to “laboratise” the world. The political dimension of scientific research also comes to light. Translations 1, 2 and 3 form an arena in which various possibilities can be explored and decided upon. Choosing to keep certain aspects of the real world rather than others means choosing to ask one question rather than another; choosing to work with a particular research team or to buy a particular computer means exploring one answer rather than another; choosing to set up an “intermediate laboratory” at one location rather than another means choosing to change the world in one direction rather than in the other. The problem, according to the authors, is that this political transformation of the world takes place in the absence of any real discussion: it is reserved for experts only – lay persons are excluded.

2 – Callon *et al.* (p.105-150) then present descriptions of cases in which lay persons demonstrate their capacity to collaborate with experts, that is, to participate in translations usually reserved for specialists. Examples are to be found in the works of Brown (1992), Clarke (1998), Barthe (2000), Rabeharisoa *et al.* (1999), Epstein (1995) and Wynne (1996). Here we will only mention three of them.

Callon *et al.* first mention the case of the families of children with muscular dystrophy (Rabeharisoa 1999). They explain how these families gradually succeeded in informing people about the particular problems surrounding their children. Some of these families first made the decision to no longer hide their children, but to bring them out into the open and publicly pose the problem of their difference. They then formed associations and began to draw up a list of patients, comparing them and classifying them according to their similarities and dissimilarities. They also tried to reconstruct the development of the disease by making films and photograph albums. Based on these early documents, researchers were able to set up more precise and more rigorous experiments that finally led to identifying the genetic disorder responsible for the disease. This case study, explain Callon *et al.*, illustrates the capacity of “lay persons” to participate in the basic collection of documentation and the posing of new scientific questions (translation 1).

The authors then go back to the work carried out by Epstein (1995) on associations of AIDS patients. They explain how these associations had to plunge into the complex question of clinical trials in order to be heard by the medical institutions; how they observed, for example, that certain minorities such as Afro-Americans and women were under-represented in the double blind trials; and how they then questioned the capacity of these studies to take into account the particular characteristics of these populations. The authors also relate how these associations came to participate in the discussions of the expert community on how to conduct drug trials. Some of the experts consider that it unrealistic to carry out studies on subjects who have not received any prior treatment and that the studies have to be conducted under conditions that are as close to reality as possible. For others, on the contrary, the studies can only be valid if they are carried out on subjects who are “not polluted” by past treatment. The patients’ associations are also divided on this question: some worry about the segregation that necessarily results from excessively strict application of the protocol while others advocate even stricter application, convinced that it is the best way of ensuring that effective treatment will be found. Callon observes that this case illustrates the capacity of “lay persons” to be an active part of the research community and participate in its configuration (translation 2).

The authors then discuss the research conducted by Wynne B. (1996) on sheep

breeders in the Cumbrian hills. A short time after the Chernobyl nuclear cloud passed over England, the British authorities prohibited the sale of lamb and mutton for three weeks. Their decision was based on the opinion of experts who believed that the radioactive cesium would disappear from the environment after twenty days. Surprisingly, after three weeks, the contamination had not disappeared from the Cumbrian hills. After additional studies, it was obvious that the experts had made a mistake. Their expertise was based on observations made on alkaline soil and did not apply to the limestone hills of Cumbria. The sheep breeders also suspected that the persistent contamination was not due to the Chernobyl cloud, but to a fire in a neighbouring reprocessing plant that took place several years earlier. Due to the insistence of the local populations, additional studies were conducted. During the studies, however, the scientists refused to take into account the sheep breeders' knowledge of the complexity of their environment. This became obvious when the experts started to measure the effect on the sheep of spraying their pasture land with bentonite. The sheep breeders immediately pointed out that nothing could be learnt from the experiment, because their sheep were not used to being in enclosures. The simple fact of conducting the experiment would therefore disturb their metabolism. According to Callon, this case illustrates the capacity of lay persons to help to adapt knowledge gained in the laboratory to the complex reality of the big world (translation 3).

3 – These descriptions enabled Callon (p.136-150, 163-173) to determine the common denominators of the various participatory experiments:

- They first showed that the separation between “experts” and “lay persons” is brought into question each time. It seems in fact that collaboration develops between two types of researchers: (i) *indoor researchers* who focus on establishing extremely precise facts controlled in the laboratory and (ii) *outdoor researchers* who try to establish the complex relations that exist among the actors of the big world.
- Callon also shows that classical representation procedures are brought into question during these experiments as well. These procedures are based on the votes of isolated individuals and/or on the consultation of already existing groups. Yet those who seek recognition in these participatory experiments do not belong to either category. They are *emerging groups*, that is, groups whose identity is gradually constructed or re-constructed as the scientific projects develop.

The authors (p. 174-190, 215-223) then try to determine the criteria that can be used to evaluate settings intended to provide a framework for the experiments. These criteria are aimed at measuring the *participatory intensity* of the settings¹¹. This intensity can be evaluated according to two principles. *Principle 1* is used to determine how early in the process the outdoor researchers join the indoor researchers, expressed in degrees:

¹¹ The authors also mention (i) *openness criteria*: the degree of *diversity* and *independence* of the groups consulted and the degree of representativeness of spokespersons ; (ii) *quality criteria*: degree of *seriousness* and *continuity* of the spokespersons; and (iii) *implementation criteria*: *equality of conditions of access to discussions*, *traceability* of discussions, *clarity* of rules regulating the discussions (see Callon *et al.*, 2001, p.215-223).

- *Degree 1:* The outdoor researchers help to reconfigure the big world once the laboratory work is finished. They help the indoor researchers to adapt the reality in which they live to the objects designed in the laboratory.
- *Degree 2:* The outdoor researchers participate in organisation of the research collective. They ensure, in conjunction with the specialists, that the collective includes all the necessary competences and participate in discussions in which knowledge of the subject is strengthened.
- *Degree 3:* The outdoor researchers help to identify and formulate the problems that are going to be investigated. They help to assemble the premises on which the preliminary hypotheses will be based.

Principle 2 is used to measure the degree of involvement of the emerging groups in the composition of the research collective:

- *Degree 1:* The emerging group claims to have a distinctive identity. It asks the following questions: who does the group consist of? what are its projects, its expectations and its interests? how does it define and describe itself? It then does its best to have its particular identity acknowledged.
- *Degree 2:* The emerging group starts to listen to the other groups concerned (whether they are other emerging groups or already established groups) and to join in the discussion. It ensures that each person is listened to and heard.
- *Degree 3:* The emerging group negotiates with the other groups concerned so that a collective identity can be developed. Each group concerned agrees to its identity being negotiable so that the different identities can be adjusted so that all of them will be taken into account.

In the wake of this development, the question obviously arises as to whether the evaluation grid proposed by Callon can be applied to Participatory Design projects. We are not in a position to answer that question at this stage. But our aim here is not to identify the landmarks to be used to evaluate the success of PD projects; it is simply to understand how we can go about doing so. The research conducted by Callon seems to be highly enlightening in this respect. The strength of the model proposed by the authors lies in the fact that they continually strive to follow the progress of projects *in the making*, that is, to show how the people involved try to connect up heterogeneous elements in the field. This type of description is essential. It is what enables us to understand that indoor research is the result of a particular configuration which can then be reconfigured to make it more accessible to lay persons. It is also what enables us to identify the landmarks that can be used to evaluate the transition from the separation of experts and lay persons to collaboration between indoor researchers and outdoor researchers.

6 Conclusion:

We have argued in this paper that it is essential to describe PD projects *in the making* if we want to identify the landmarks for evaluating the success of these projects. We have developed two arguments. The first is that *the lack of precise landmarks in the literature on PD is due to the fact that projects in the making are rarely described*. Most articles devoted to PD techniques report very little on the testing of these techniques in the field. These articles generally belong to one of three categories: those that describe fictive testing, those which stop their description when the testing begins and those that only describe tests that illustrate the different steps of the technique. A certain number of articles do, however, try to highlight issues not addressed in the articles devoted to the techniques themselves. These include “dominance relations” in design situations and how “socio-political factors” affect the application of PD methods. Focussing on these issues, however, produces the same result as the articles devoted to PD techniques. A large part of what happens in the field is simply ignored. The landmarks that enable the success of the projects to be evaluated therefore remain invisible. The second argument is that *the most precise landmarks come from the description of projects in the making*. This is illustrated by the article by Blomberg *et al.* (1996). By giving a step-by-step account of the progress of a project conducted in a law firm, Blomberg *et al.* show the crucial role played by *emerging groups* in PD projects. These are groups whose identity is called into question by the technological project and develops as the project progresses. The fact that these groups become fully fledged participants in the project thus appears to be a specific landmark that can be used to evaluate the success of a PD project. Another example of the advantage of describing projects *in the making* is to be found in the work of Callon *et al.* (2009) in the field of STS (Science and Technical Studies). These authors present numerous articles that meticulously describe how scientists transform the world and how “outdoor researchers” sometimes force scientists to collaborate with them. These descriptions enable the authors to determine criteria to precisely evaluate the settings designed to provide a framework for the experiments. These criteria evaluate (i) how early on in the project the outdoor researchers start working with the laboratory researchers and (ii) to what extent emerging groups are included in the composition of the research collective.

We are obviously not suggesting that PD researchers should stop inventing new techniques or setting up PD projects. We are not simply suggesting that the PD community should switch to STS. What we are saying is that, in order to evaluate its practices, PD needs to describe them with the same meticulous care with which STS describes those of scientists and the people who collaborate with them.

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