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MEANING AND REPLICABILITY OF SOCIAL PSYCHOLOGICAL EXPERIMENTS: A PERSONAL ACCOUNT

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Abstract

In this paper I describe the gradual evolution of experimental social psychology from a so called "science of discovery", with invariant findings over time and space, to an art of theatrical reflection with self-made reproductions of social life to so engage in more thoughtful action. I do this by means of a narrative account of my sixty year long personal experience with the various changes in the discipline, from the many cases of non-replicability early in my career, to the splendid discovery by vigilant students at my department of the probably most impressive case of fraud in the history of the social sciences, to the persistent denial of a need for a new logic by orthodox positivists in the discipline, even after more than half of the carefully designed replications of important experiments in the discipline failed. I look at this evolution through the lens of gradual objectification in science as a whole, from the most distant objects, such as stars, to the closer and closer reality of our own existence, namely the production and processing of meaning, and the inevitable recursivity (historicity) and incompleteness (one cannot include one's own discourse in the discourse one has at that moment) of that endeavour. I also describe moments of institutional evolution, its political besides scientific interest, and encounters with the most influential protagonists in the development of the new logic. I also reflect a moment on the theoretical and methodological implications of that new logic, and end with showing how the theme of social engagement and the move toward cultivated action, rather than by freezing history in the so called truth of invariance, was actually the core idea in the work of Kurt Lewin, and has been used inadvertently in the subsequent use of experiments in the discipline, even by those who pretended to be the astronomers of the mind. But with the clear note that reflection on social life in the form of self-made reproductions of that life, is probably the most human thing we can do, and enables us to create culture, rather than blind evolution in merely physical or biological terms. So, making experimental social psychology a valuable tool of humanization as well, but only when properly used.

Keywords: experimental social psychology, problems of replicability, historical development, science versus art, primary social logic, thoughtful action

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The problem of replicability: my first encounter

It is nearly sixty years ago that I acted for the first time as experimenter in a social psychological experiment, and was immediately confronted with something what later would prove to be a major problem in the discipline, namely the inability to replicate a so called classic result, but without the discipline as a whole taking this very serious. It was a critical replication of the famous Festinger and Carlsmith (1959) experiment about the effect of lying on the liar's own thinking. In that experiment, first year psychology students at Stanford University participated one by one in an, according to the investigators, very boring psychological test, namely an hour long packing spools and turning screws a quarter forward, and were at the end suddenly asked by the experimenter to go tell to the next participant in the waiting room that it had been very pleasant. This was, thus the experimenter, to see how a positive expectation at the beginning would influence the performance on the test. Normally, he said, this role would be played by a paid assistant of his, but since this assistant had just called that he was unable to do the next session, he now asked the previous participant himself to play that role. To half of the students to which he asked this, he promised twenty dollars for their collaboration, and to the other half one dollar, but with only a few exceptions, they all immediately agreed to collaborate. At the end, and before going home, they were also asked to go to another room in the building to fill out a questionnaire about "life on campus", and in one of the questions they were asked how they felt about the test they had just performed. Those who had been paid one dollar for telling that it was enjoyable rated it, on a scale from -5 (very negative) to +5 (very positive), on average +1.35, or somewhat positive, whereas those who had been paid twenty dollars on average -0.05, or totally neutral. The control subjects, or those who had only performed the test, and had not been asked anything else, rated it on average -0.45, or basically neutral as well.

In the eyes of Festinger and Carlsmith this was good supportive evidence for the theory of cognitive dissonance which Festinger had published two years earlier, in 1957. In that theory, Festinger claims that people have a need for cognitive consistency, or cognitive consonance as he calls this, between the various cognitions which they have about themselves and about the surrounding world. When this cognitive consonance is somehow broken – for example, when they know that the test they had performed was very boring, but also know that they told to the next participant that it had been very enjoyable – they experience cognitive dissonance, and will try to reduce that dissonance to a tolerable level. One of the possible means to do this, thus Festinger, is to start to believe themselves that what they said was true, but when they know that they told this for a lot of money, then this cognitive dissonance is already reduced to some extent in that way, and they do not need to change their mind anymore in the direction of what they said to achieve that goal. For that reason, thus Festinger and Carlsmith, it are only the students who were paid one dollar for saying that it had been very enjoyable who changed their mind about the test in the positive direction, whereas those who had been paid twenty dollars not.

This is definitely a good story, which clearly explains to the reader what Festinger and Carlsmith meant by cognitive dissonance and dissonance reduction, but whether it is also a coherent explanation of what they did and found remains the question. To begin with, the original attitude of the students toward the test was not, as the story wants us to believe, very negative, but totally neutral, namely on average -0.45, which is less than 5% of the eleven point scale removed from the zero midpoint. If it had been the purpose to obtain a neutral score, then this would certainly have been seen as a splashing success. When I once told this to a very positivistic oriented colleague, he answered that students in Stanford probably do not want to tell very negative things about the research of their professor, and therefore rated an objectively very boring test in neutral terms. I was truly shocked by that response, because if one uses so called “objective measures” to assess how the attitudes of the students really are, and then replaces the obtained measures by imaginary magnitudes which better fit the story, then one may as well use the story as evidence, and leave the numbers aside. Second, and that is even more important, it is far from certain that the slightly positive result in the one dollar condition was a consequence of the positive content of what the students had told to the next participant, because there was no control condition in which they were asked to tell something different, say a negative content, to see if the result would be different.

It was primarily for this second reason that my then professor in social psychology in Leuven, Jozef Nuttin, decided in 1964 to perform a critical replication of the Festinger and Carlsmith experiment, and asked me – I was then in my third year psychology studies – to freely (that is unpaid) participate as experimenter in that replication. What I immediately accepted, because I felt quite honoured that, while I was still an ordinary student, I was already asked by my professor to assist him in his research. It is only later that I realized that in that way I was probably brought in a similar psychological condition as the students in Stanford who were suddenly asked by their experimenter to leave their status as ordinary participant and become collaborator in the manipulation of the next participant. By “critical” replication, Nuttin understood that he not only wanted to replicate the “positive” role playing condition, or the condition in which students were asked to tell to the next participant that it had been very enjoyable, but also wanted to add a “negative” role playing condition, or one in which students were asked to tell that it had been very boring. This latter condition was supposed to represent the truth instead of lie, but when we look at the basically neutral rating in the control condition, we may call it a lie too, but then in the negative instead of positive direction. But whatever the labeling, truth or lie, the results were exactly the same, namely on average -0.40, or basically neutral in as well the low paid positive as low paid negative condition. This not only contradicts the effect of content of the played role on the subsequent attitude of the subject, but the -0.40 in the low paid positive condition is obviously a non-replication of the +1.35 which Festinger and Carlsmith reported in that same condition, and on which they had based their dissonance interpretation of their experiment. Nuttin published these results in 1966 in the International Journal of Psychology, but was virtually never mentioned in subsequent textbooks and articles on the subject, so that until today, or more than sixty years after the original publication, students in the discipline still read and learn that Festinger and Carlsmith have shown that when people freely tell something about their experience of which they know it is not true, they may start to believe themselves that it was true anyway. And of course, that is what the experiment did, but not in terms of its design and data, but in terms of what authors of textbooks and articles wrote about it.

Increasing lack of replicability, but no reaction

I have later, when I had already become professor in social psychology myself, also tried to replicate the original Festinger and Carlsmith result, but in vein, I did not succeed. What I did find though was that subjects whose experimental task consisted of helping the experimenter in dictating another subject's work, liked their experimental task a lot more than subjects who executed exactly the same task, physically speaking, but being dictated by another subject who assisted the experimenter in telling them what to do, or thus a pure social rank effect independent of the physical content of the task. And what I also noted was that the classic procedure of Festinger and Carlsmith, namely ask students to volunteer in the manipulation of the next participant, does not work at all in a context in which the students do not like to be in the institute in question and do not value research, because when I once tried to use that procedure in a special school for judicial children, who obviously do not wish to be in that school, let alone have paid thousands of dollars themselves to be allowed to study there (as the students in Stanford had done), nobody complied and they all bluntly refused. This "failed" experiment was obviously not published, because in some sense there was no experiment, but it is clear that this failure throws a different light on the so called "freedom of choice", of which the literature says that it is crucial to obtain a dissonance effect, because if only those who already wish to participate in research accept the experimenter's invitation to freely help him in the manipulation of the next participant, then it should be no surprise that those who get that chance, and can thereby demonstrate their solidarity with the researcher, do not wish to say very negative things about that research. It is not the place here to elaborate that idea in more detail, but I have explained it further in an article, published in 1999, in the European Journal of Organisational Psychology, under the title “How helping your boss can change the meaning of work”.

The only time that I “did” see a result which might count as supportive evidence for the idea that freely speaking positively or negatively about the test may influence the subject’s attitude toward the test in the direction of what they said – or the “saying is believing” hypothesis, as Elliot Aronson, a former Chief Editor of the Handbook of Experimental Social Psychology, called this – was when I read a manuscript that Girandola and Joule, two French colleagues, had sent for publication to a major journal in social psychology, and for which I was asked to act as one of the reviewers. In their manuscript, Girandola and Joule described, among other things, two different procedures for the Festinger and Carlsmith experiment, namely one in which, just like in the original experiment, they first let the students perform the test, and then asked them to freely tell to the next participant that it had been very pleasant (positive content) or very boring (negative content), and another procedure – let us call this the “imaginary performance” procedure – in which the students were only given a description of the test, and, without having it performed themselves, were asked to freely tell to the next participant that it was very pleasant (positive content) or very boring (negative content). Now, with the oridinal procedure the results were basically like Nuttin’s, namely +0.50 and +0.42, or basically neutral after as well the positive as negative role playing, but with the “imaginary performance” procedure, the results were exactly like the “saying is believing” hypothesis wants, namely +2.77, or quite positive after the positive role playing, and -2.55, or quite negative after the negative role playing. I then strongly advocated publication of these results, with the request to highlight this effect (because, remarkably, Girondola and Joule, who focused on something different, had not seen it themselves, and it was only after my personal rearrangement of their reported date that it became visible), but in vein, the manuscript was rejected entirely. However, somewhat later, and by pure coincidence, I noticed the publication of “part” of Girandola and Joules data in another less prestigious journal, but not enough to make the comparison as I just made above. I then decided to try to publish a paper myself, in which I reported not only my own repeated failure to replicate the original Festinger and Carlsmith result, but also mentioned the astonishing difference between real and imaginary performanc of the test as found by Girandola and Joule, with the suggestion that one of the reasons why authors of textbooks and articles continue to report “as if” Festinger and Carlsmith had found a “saying is believing” effect is that they themselves have never performed the test themselves (at least not as real subjects) and thus “imagine” that there is such an effect (even when there is not), because that seems to be the way that people who only “imagine” the test get influenced by what they say, or as I mentioned in the paper “the reality of a fiction that becomes the fiction of reality”. The paper was published in 2000 in The International Journal of Social Psychology, under the title “Festinger and Carlsmith, 1959, Fact and Fiction”, but just like Nuttin’s paper, was virtually never mentioned in textbooks and subsequent articles on this subject, so that students in the discipline continue to read and learn that Festinger and Carlsmith have shown that people who freely tell something of which they know that it is not true, may start to believe themselves that it is true anyway. And, again, that is indeed what the experiment has done, but not in terms of its own design and data, but in terms of what authors of textbooks and articles have written about it.

A scandal, and the opening of the curtain

So, it is clearly not only very recent that problems with replicability of experiments in social psychology have been mentioned in the literature, but it is only very recent that the discipline as a whole started to pay serious attention to it. This change in mentality did not originate, as one might think, from a fundamental revision of the underlying logic of the discipline (because that fundamental revision existed already for a long time, but was totally neglected and even treated as heresy by orthodox positivists in the discipline), but from a plain scandal. A well-known researcher in social psychology was found to have published and to have let several PhD students graduate on data which did not exist, but which he had invented himself. This was first surmised by a couple of young PhD students at my University, who wanted to know where the data on which they were supposed to graduate actually came from, and was later confirmed by a specially installed committee. It is truly shocking, in retrospect, that not only young PhD students, but also several well established colleagues have published for years together with this researcher in question, and thus have claimed superior expertise on matters of which in fact they knew nothing about, to only discover that what they told was fake after hearing a couple of young PhD students ask questions about the provenance of their data. This looks like critics of star restaurants who for years get plastic on their table, and only start to test the difference with real vegetables after hearing a couple of young assistants in the kitchen ask questions about where the chef buys his food. This is definitely no sign of good taste, let alone of superior expertise to tell ordinary people where they can eat best, except maybe for paraphernalia, such as silver work, or the pink of the waiter in telling what is on the plate, to not forget the check at the end of the meal. There was even a publication in Science, a top Journal though in science as a whole, and it is still an enigma for me how this paper was ever accepted in that Journal, because apart from the fact that it was fake, also the content itself was astonishingly trivial, namely that if trash is anonymously thrown in places where it is ostensibly forbidden, the chances that other people will do the same increase. I honestly know nobody in my circle who did not know that already, and had even used preventively to keep his own environment clean. There was also no colleague then who, after the official removal of that paper from Science, tried to quickly replicate that experiment for real, to so not miss a chance for an important discovery in the discipline, it was simply left like that.

It will be no surprise now that, after this débâcle, what really hit the press all-over the world, the confidence in experimental social psychology as a positive science went down very quickly, to culminate a few years ago in a so called "replication project", or a project in which various colleagues in different parts of the world tried to replicate well known experiments in the field as carefully as possible, to see if the originally published results would stand. What they did not, because in more than half of the cases the originally published results were not found again, and I do not mean in exact terms, like in physics, but only in terms of statistical significance in the originally published direction. In whatever branch of the positive science such a result would certainly have led to the immediate elimination of that branch, but not so in experimental social psychology, because instead of finally looking in more detail at the already long existing logic by which such a result is not only no surprise, but even more or less expected, the orthodox positivistic researchers simply went on in the same direction, only with even more measurement and more statistics. As if, if we could take the entire humanity as sample (what is obviously impossible, because there are already more illiterates on this planet than there are North Americans, let alone than first year students at Universities), we would then for once and for all know how people will behave and continue to behave on earth. Social Psychology in that logic would then become like astronomy of the mind, with deterministic object permanence and insensitivity to our act of observation. I know that this has not been said in precisely these words in the positivistic circles, but it is definitely an underlying form of thinking in a reaction which assumes that non-replicability and cultural evolution is a consequence of lack of measurement and lack of statistics, and not an intrinsic property of the nature of the discipline itself.

The gradual development of a new logic: from past to present

But then what is this fundamental change of logic by which the problems with replicability of experiments in social psychology are not only no surprise, but rather obvious? To explain this, I need to go back for a while to the historical origin of the discipline, because in this way we can see where we come from and where we have gone to. The first experiments of which, in retrospect, we can say that they were "social psychological" date back to the beginning of the previous century, when investigators in "general" experimental psychology – that is, the experimental study of the mental functions of the psychological apparatus, such as attention, discrimination, association, etc. – were sufficiently advanced to become sensitive to variations in their findings which did not seem to originate in the assumed general properties of the psychological apparatus itself, but in the social conditions in which the experiment was performed, such as the presence of other people as audience or as coactors (other subjects who participate in the same experiment at the same time) in the laboratory. To control these unexpected and unwanted social factors (because when one studies the general properties of an apparatus, one does not expect the presence of another similar apparatus to change these properties), some investigators started to manipulate these "social factors" on purpose, and tried to systematize their effect. But because there were no social terms available in general experimental psychology (because that is the study of an apparatus, and thus of something individual), the researchers who did this had to borrow their social terms from their already existing knowledge of social life, or even better, from their academic study of social life, sociology, because in that way their descriptions and analyses got an academic character as well. Thus, when Walter Möde, published his Berlin studies on coaction (he let subjects perform the task alone, or in groups of two, four, etc., and also varied the nature of the coaction) in a monograph in 1920, he called it "Experimentelle Massenpsychologie", and described his work explicitly in the introduction as "Eine neue Soziologie". The term "Masse" (German for "mass") was a well known term in the (mainly French) sociology of his days, in which it was often used to refer to the unordered (anomic) social substance from which ordered (nomic) social institutions are made, and to which they will probably return when not taken care of well enough, like the entropic loss of order in thermodynamic systems. Floyd Allport, on the other hand, who did more or less the same research in Harvard as Möde in Berlin (actually at the explicit advice of his German supervisor, Hugo Münsterberg, who was hired by William James to direct his psychological laboratory in Harvard), published his results in the same year 1920 as "The influence of the group on association and thought", or thus also with a very common social term, group, to describe what he did and found. One of the things he found was that co-acting subjects judge ambiguous stimuli less extremely than subjects alone, what he described as "implicit" conformity. A good decade later, Muzafer Sherif, first at Harvard but later at Columbia University, added "communication" to "coaction" (he let the co-acting subjects tell aloud to each other what they observed), and found even more conformity. He first published this as “The study of some social factors in perception” (1935), but a year later as “The psychology of social norms” (1936), or thus again with a very common social term, norms, to described and analyse what he found (after which the same experimental paradigm, in this case the study of the autokinetic effect, or the apparent movement of a small spot of light in a dark room, was used for years to describe and analyse the phenomenon in question). The same happened also in the study of other social phenomena, such as imitation, conformity, obedience, minority influence, innovation, altruism, aggression, cooperation, competition, etc. It have described this in more detail in an essay in 1973 (actually the text of my inaugural lecture for my appointment in Tilburg the year before), which I entitled “Ars Artefactorum”, or literally the Art of the Artefacts, and with as subtitle “Considerations on the growth of experimental social psychology” (1973).

In that same essay, but later more extensively elsewhere, I have also suggested that the way in which experimental social psychology emerged from general experimental psychology, is emblematic for the way in which also other branches of the objectifying sciences – that is, sciences which try to say how things are and will remain, in contrast to technologies, which try to say how things can be made and changed – emerged from their predecessor, namely as an attempt to objectify the inevitable conditions which play a role in the objectification of the previous branch, but which only manifest their influence when the knowledge in the previous branch is sufficiently advanced to get disturbed by them. For example, the first branch of objectifying science is probably astronomy, and that makes sense, because when the object of investigation is very far removed from the different observers, then the chances to change the object are minimal, but the chances to see the same are maximal, at least as long as the description is given in units of time and space, or numbers, because when we use narratives, such as say “mythology”, then there are obviously differences between different groups or communities. What does disturb the unity in the space-time description of stars, though, is the condition of light, not only the difference between day and night, or the turning of the earth, but also the speed of light, colour dispersion, aberration, etc. Thus, to preserve or reconstruct the unity in the space-time description of stars, light itself needed to be investigated. Then came optics, first of lenses outside the human body, say telescopes, and then inside the human body, say the self-adaptive lenses of the eyes, because, obviously, those with good lenses, in both parts, did see more and better than those without good lenses. Then came neurophysiology, first the chemical reactions upon light inside the eye, then the transmission of these reactions via neurons to the brain, and finally the transformation of the incoming signals in sensation and consciousness. And then came something for which the psychological apparatus of the individual subject is no longer sufficient, but obviously still "necessary", namely the meaning making interaction "between" subjects to transform the meaningless sensations and consciousness in "meaningful perception", because in the end we see "something", a star, or stone, or whatever, and we think and talk about it in words and sentences and stories. It is upon this last factor in the construction of meaningful perception, namely the meaning making interaction "between" subjects, that investigators in general experimental psychology hit in the beginning of the previous century, and turned it in a separate discipline, namely "experimental social psychology". But – and that is the point I want to make – the logic which they used in that endeavour was at first the same one as the one they took with them from "general" experimental psychology, namely one in which meaning is conceived as an "a priori" or "given" message of reality itself, and meaningful perception only as a personal registration of that message by means of the psychological apparatus. We call that the logic of individual realism. The "social" in that logic has nothing to do with the very construction of meaning, but only with the distribution or communication of it "after" it has been registered individually. But there is one point in that logic by which the social can become the source of meaning, although not in the individualistic and realistic sense of the word, namely when that individualistic and realistic registration of the given meaning of reality fails, and the consensus which normally follows upon the communication of truth fails too. At that moment, thus the logic of individual realism, people will start to construct consensus on purpose, and that is the background of the many forms of pressure toward consensus in human interaction, such as conformity, rejection and even killing of deviants, etc. We call that the logic of the "secondary" social constructionism. "Secondary", because it only occurs "after" the individual registration, and is basically a communicative byproduct of it. It is this "secondary" definition of social construction which has been adopted in the early decades of experimental social psychology, with Leon Festinger, who published an explicit theory of that kind halfway the previous century. With only one additional assumption, however, this secondary social definition can easily be generalized to a point where it starts to look like a "primary" social definition, whereas it is not, but, on the contrary, becomes the ultimate denial of it, but in disguise. Indeed, when we assume that in real life people can never completely and correctly register the "a priori" or "given" meaning of reality, either by shortcomings on the side of reality itself (i.e., weak or distorted information), or on the side of the individual subjects (i.e., emotions, poor intelligence, bounded rationality, etc.), then something which in principle is only a social compensation for the lack of individual truth, namely communication and consensus, becomes the only means for subjective truth which is left on earth. In earlier papers on this matter I have called this the “fallen angle” model of the secondary social constructionism, or the model in which people on earth are doomed to constant discussion and quarreling, because they lost their angelic capacity to see all truth at once. Real angles in this model, but these creature do not live on earth, are doomed to constance peace and agreement, because with their perfect individual minds, all exact replica of the single perfect mind, they can only see truth and, thus, the same.

The primary social constructionism as alternative to the secondary one

In the logic of the “primary” social constructionism, however, which emerged only later (mainly in the context of the theory of language), there is no “a priori” or “given” meaning in reality itself, and all meaning is defined as the referential product of the socially coordinated or signifying interaction “between” people. Truth then in that logic cannot be the correct individual registration of an a priori or given meaning in reality itself, but must be the socially valid reproduction, in thinking or in action, of the social coordinations in a community of practice which have led to meaning, and which were sustained as such in that community. But of course – and that probably explains the pervasive tendency toward the logic of individual realism in psychology, also in the traditional so called “social” psychology – once meaning is constructed socially, it can be internalized in one’s mind, and later be reproduced in “recognition”, or literally in “cognizing again” the meaning of what is perceived. At that moment it obviously looks “as if” the meaning of what is perceived was already present in reality before, and that meaningful cognition is indeed an individual registration of the a priori or given meaning in reality itself. Once we are there, it becomes exceedingly difficult to escape from that perspective, because from then on every social construction of meaning by means of communication and consensus becomes immediately a secondary social compensation for the lack of individual truth, and thus fiction instead of realism. It is not surprising therefore that what is currently called social constructionism is often blamed for being anti-scientific, or even worse, for being without anti-moral, because, as is said “anything goes”. But that this kind of blame is actually based on a secondary interpretation of social construction, and not on a primary one, becomes immediately clear when one realizes that the primary definition of social construction does not say at all that anything goes, but on the contrary, that only goes what is mutually exchangeable as social coordination in a community of practice, or in other words, is “moral” by definition. That meaning and truth is a referential product of the coordinated interaction in a community of practice becomes most clear when people from different communities of practice meet and try to do things together, because then they may clash, and will try to resolve these clashes in an orthodox way, what means either by forced "inclusion" (i.e., force the other people to "do like us", which, when they are children, may be called "education", but when they are adults, may be called "integration") or by forced "exclusion", not only physically (i.e., send them away or even kill them), but also mentally (i.e., let their body stay in our community, but not their mind, such as laugh at them, call them crazy, etc.). But when the conflicting communities are about equally powerful, and cannot or do not want to break their interaction (but for the latter we need "binding forces", such as interdependence or external pressure), then conflicts of coordination can lead to new coordinations, or new meanings which did not exist on either side before. In this way culture evolves – that is, partly by an orthodox conservation of the past, and partly by the resolution of conflicts of coordination in a constructive new way. If we had only the former, then, in the end, nothing would be left, because at each transition from one generation to the other something of the past would be lost. And if we hand only the latter, then at each transition from one generation to the other, we would have to start from scratch again. It is the combination which works, conservation and innovation together. In an earlier paper on this matter, together with Perret-Clermont, Nicolet, and Grossen, I have called this the combination of the "running down" and the "running up" model of evolution, analogous to similar models in physics.

Institutional changes and some prominent figures in the development of the primary social constructionism

There are many colleagues in the discipline who have contributed in one way or the other to the development of the "primary" social constructionism in social psychology, but remarkeably, two of them were former presidents of the European Association of Experimental Social Psychology, EAESP, which was created halfway the sixties (by the way, for which the decision to do this was made in an office in Leuven only a few meters away from mine, where I was then working on my thesis in social psychology. Reason for which I sometimes think of myself as a child of that Association, and have done my best to serve it as well as possible, but not without remaining vigilant to the developments in logic which seemed necessary for the development of the discipline). I call this "remarkeable", because the approach to social phenomena in that Association, as the name says, was explicitly "experimental", and that kind of approach, as I just explained above, was deeply rooted in the logic of individual realism in "general" experimental psychology, and as translated in a logic of "secondary" social constructionism by people like Festinger. Festinger was even explicitly involved in the start-up of the Association, as representative of the American

Council of Social Research, which, together with other American Institutions, such as Ford Foundation and even the NAVY, invested important financial and intellectual resources in the foundation of the Association. This all happened in the context of the postwar concern of politicians to promote international colaboration among social scientists– with, among other things, the creation of UNESCO – under the motto that we had learned how to split an atom, but not how to prevent or end a human conflict without horrendous violence. That this resulted in the promotion of a rather American model of doing social research was not the outcome of any proven superiority of that model in achieving that goal, but more of the obvious fear in the West – it was the period of the Cold War – that social inquiry in Europe might bend toward a more Communist model of inquiry, and that model, as we know, was far from the quit reproduction of these phenomena in the form of psychological experiments, but more a matter of the actual change of society, if not with peaceful means, then with revolutionary ones (and many colleagues in social psychology, also in the West, were actually thinking like that). This is somewhat described in a brief essay by Carl Grauman in the 1995 Annals of EAESP, but also in a scholarly paper (2013) by one of my former PhD students, Sandra Schruijer, who, after she graduated and had become professor in the discipline herself, decided to also study history, and made her master thesis in history precisely on that topic (and who, by the way, was also the person who, after she participated in the 1986 Summer School of EAESP in Bologna, where I was one of the teachers myself, took action to also let PhD students in social psychology become Affiliate members, what is now taken for granted).

The two former presidents of EAESP which I have in mind as important contributors to the development of a primary social logic in the discipline are Serge Moscovici and Willem Doise. Serge Moscovici, who was the very first president of EAESP, always defended the notion that meaning is not an “a priori” or “given” meaning in reality itself, but is constantly created and modified by people in society. However, instead of elaborating that idea in much detail logical terms, he tried to illustrate it mainly in concrete terms of how people, in different communities of practice, think and talk about systems of thought, from science to religion, and called it the study of “social representations”. He also illustrated that logic in concrete studies of how active minorities may change the ways of thinking of dominant majorities, namely by repeating their own deviant vieuws consistently (but not rigidly), so that people in the dominant majority start to coordinate their way of looking at reality in with that of the deviant minority, but in a way which cannot be seen as imitation of the deviant minority, but as an invention of their own. Moscovici called this type of social influence “conversion”, in contrast to “compliance”.

The other president of EAESP, Willem Doise (president from 1978 till 1981), also did a lot of research in social representations, mainly in the field of “Human Rights”, but illustrated his primary social logic primarily in his research on the social development of intelligence in children, often referred to as “socio-genetic constructivism”. In the classic theory of Piaget on cognitive development, the new concepts which children develop are defined as internalized “operations” of the child on the world of things, or in other words as bacially “individual”. Doise, however, who worked at the time at the same department in Geneva as the one where Piaget worked, changed this in “cooperations”, or in socially exchangeable coordinations among children in their collective operations on the world of things. He illustrated that mainly in experiments in which children were asked to resolve problems of coordination in classic Piagetian tasks, such as, for example, the conservation of liquids in different glasses, collectively, and considered the solution only as “given” when the cooperating children finally agreed on the solution. This kind of work has later be elabored in great detail by one of Doise’s PhD students, namely Anne-Nelly Perret-Clermont, who, after she graduated in Geneva, was appointed professor in the psychology of Education in Neuchâtel, or the place where Piaget had studied himself and had become professor before moving to Geneva. With all of them, Moscovici, Doise, and Perret-Clermont (and with Perret-Clermont even to these days), I have had close personal contact (Doise even spent a sabat year at my department), and more than by reading their work, I have learned about the essence of their thinking in personal conversations, not only at work, but also at home.

But the undoubtedly most influential protagonist of the primary social constructionism in social psychology, and actually in the social sciences as a whole, is not a European, but an American, namely Kenneth Gergen. As most colleagues in social psychology in those days, Gergen was originally trained in the positivistic tradition of the discipline, and changed his mind about the needed logic in the discipline only afterwards. After his PhD with Edward Jones at Duke, and his appointment at Harvard, where he even became head of the committee for education in Social Relations, he finally moved to Swarthmore in Pennsylvania, as successor to Solomon Ash, or the author of the well known experiments on conformity with lines. From there, but also from any other places in the world where he want on sabbatical, or was invited for Lectures (including at my own University, where he was awarded an honourable degree in 1987), he gradually developed the logic of the primary social constructionism, at first as fierce attack on the prevailing logic of individual realism in the discipline, but over time more and more as a constructive agenda for research and practice on its own. I can obviously not summarize the essence of that monumental work in a few lines in this paper, and therefore I like to refer the reader to the probably most comprehensive and scholarly representation of that work, namely his book “Realities and Relationships. Soundings in Social Constructinism”, published with Harvard University Press in 1994 (but there are many other books and articles as well, several of which are published together with his wife, Mary Gergen, who was also professor in social psychology, and who has also contributed in many ways to the creation and propagation of the logic of the primary social constructionism in the discipline). Together with many awards and recognitions, including from APA, Gergen was also listed as one of the fifty most influential living psychologists in the world, in all parts of psychology together. It is therefore rather surprising, to say the least, that a few years ago a retired Dutch colleague in a local radio programma called him a controversial side figure for whom nothing is “objective”, and offered no other argument for that contention than the fact that he himself was member of the Dutch Royal Academy of Science, KNAW. Now, I will obviously not deny that this is a very respectable institution, and that membership in it is very respectable as well, but that it was also an argument in a decade long debate on the appropriate logic for a discipline, was so far unknown to me, and I honestly like to keep it like that. When something needs to be said about the scholarly work of a colleague, then a Habermas-like debate with a fair representation of that colleague’s work and a clear explanation of one’s own arguments seems to be more appropriate. In any case, that is what Gergen himself has always done, and what I have also tried to do when, together with Wolfgang Stroeve, the Chief Editor of the European Review of Social Psychology, I have published a Special Issue of the European Journal of Social Psychology on “Controversies in the social explanation of psychological behaviour”, in which various experts in the entire spectrum of the discipline, from the most positivistic end, to the most social constructionist one, including Gergen, could explain their thoughts and were discussed by various experts in the field. It is not that I do not know or respect the work of my Dutch colleague in question, on the contrary, because I was member of his appointment committee to full professor, review many of his manuscripts for publication and applications for grants, including for KNAW, was several times member of dissertation committees at his University, including for some of his own, and was even tangentially involved in the recommendation for his membership in KNAW in 2003, and granted in 2005. Precisely because of all of that I know very well that his condescendant remarks about Gergen on the radio had little or anything to do with a profound reading and understanding of his work, but more with a trivial personal matter which is not for discussion in this paper.

The inherent necessity of a new logic, and some implications for methodology

That sooner or later a new primary social logic of meaning and truth, different from the secondary one which was inherited from general experiment psychology, would be developed in social psychology, could already be inferred directly from the image of the evolution in the objectifying sciences which I briefly sketched above, namely one in which the successive branches in the evolution of science are defined as attempts to objectify the conditions which are inherent in the creation of knowledge in the previous branch, but which only start to manifest their influence when the knowledge in the previous branch is sufficiently developped to become disturbed by them, and to start to look at them on purpose. We can, as I actually did already in my first essay on this matter in 1973, represent that evolution as a series of inclusive sets, or as successive peels of an onion if you wish, with the star in the middle, then light, then optics, then the neurophysiological properties of the psychological apparatus, and finally the the meaning making interaction between people as most external peel. To objectify the center, we need all peels around, but when we objectify a peel, we then actually put that peel in the middle, and need all peels again, including the one we study, to objectify what we look at. That immediately makes clear that the further we go to the surface of the onion as object of investigation, the more our research becomes recursive and incomplete. Recursive, because to study meaning is by definition a contribution to it, and thus cultural and historical. And incomplete, because, as Gödel made clear already nearly a century ago, the definition of something as object of investigation needs a defining set, and since the making of meaning is the most external one, we cannot make it an object of itself, at least not without ending in paradoxes, or statements which are only true when they are false, and false when they are true. This growing recursion in moving from the center to the surface of the set of objectification was already noticed in physics, in the study of light. At first, light was interpreted in the same way as if it were a star, that is with deterministic object-permanence and total independence of the act of observation. However, it soon became clear that with light that is impossible, because to see something very small, such as a say a photon, one needs a wavelength of the observing light which is smaller than the size of what one looks at, and since wavelength is inversely related to energy, the looking “at” light “with” light actually resorts to giving it a relatively huge bump, what (together with a some other properties of light, but too long and complex to explain here) makes the accurate and simultaneous determination of position and momentum of what we look at impossible. This is one of the most fundamental theorems in quantum physics, of which all students nowadays now that its underlying logic is basically different from that in classical physics, or the physics of the relatively large.

This new logic in the founding of the discipline, has obviously also profound implications for methodology, or for the logic of the methods which we can reasonably use in the performance of the discipline. It already starts with the famous dictum in science that “to know is to measure”, because that obviously only applies to the science of matter, and not to the study of meaning. Meaning has no length, no speed, no weight, nor anything that can be seen and measured in units of time and space, or numbers. Meaning is the referential product of the coordinated interaction between people, that is internalized in concepts, and needs to be “understood”, not “measured”. An attempt to measure meaning is as absurd as try to interview stars, one cannot do that. But what we can do, of course, with regard to meaning, is “imitate” the semiotics of measurement in the science of matter, like when we ask people to tell us in numbers how much they agree with what we say, but when we do not speak the same language, and have not understood alsready what is said, we cannot even start the procedure, let alone consider the numerical answer a more “objective”, or a more “as it really is” index of what is meant that just speak with each other and try to understand the conversation. Another thing we can also do numerically with regard to meaning, and often do, is describe the distribution, like when we count the number of people who say this and the number of people who say that, but that is not a measure of what each person says, only a description of the distribution. In fact, we also do that in the construction and application of tests, namely give a numerical account of the population and try to locate individuals in them. And when we do that, we obviously need statistics, and when we do it, we better do it right. It may well be for that reason that methodology in psychology is often equated with statistics, and not with the ontology and epistemology of what we do. There is huge misunderstanding therefore in the social sciences about the meaning of “quantitative research”, because it basically refers to distributions, and not at all to the character of any of the examplars in the distribution. The counting of heads is not the same as understand the meaning of what they say. In the science of matter, the numbers to count distribution are commensurable with those which are used to measure exemplars, but not so in the study of meaning. In physics, for example, we can easily say that the average weight of all molecules in the collection is X, and the weight of the molecule in the corner X as well. In the world of meaning, that makes no sense, there is not such a thing as the average meaning of all sentences in this paper, simply because there is no metric for the meaning of a single sentence on its own. Apart from whether we talk about matter or about meaning, we often make mistakes in both domains when going from parameters on a population to those within individual examplars, like when we use correlations on a group to propose interventions on individual people. This is not warranted, at least not on logical grounds. To make this clear at once, just take the following example. Imagine three individuals, A, B, C, who are tested three times on two variables, X and Y. Imagine that the scores of A on as well X as Y are respectively 1, 2, 3, those of B respectively 2, 3, 4, and those of C respectively 3, 4, 5. The correlation between X and Y in that case is obviously very positive, as well computed on the group (between individuals) as when computed on the repeated measures (within individuals). Now reverse the scores on X, and make them respectively 3, 2, 1 for A, respectively 4, 3, 2, for B, and respectively 5, 3, 2, for C, and leave the ones on Y unchanged. The correlation between X and Y in this case is still very positive when computed on the group, but becomes very negative when computed in the repeated measures. Thus, andy attempt to make inferences about the associations within individuals on a basis on the correlation between individuals is unwarranted, as is often done in the use of structural equation models to invent scenarios of intervention, no matter how sophisticated the mediator or moderator terms may be. This is totally different from phase state models in physics, which do not only describe the connection between variables at a given moment in time, but which also contain "laws of evolution" to go from one moment to the other in time. One also wonders sometimes to what extent the spatial models which are use in psychology to describe correlations, such as factor analysis, are actually understood by those who use these models. Why, for example, do we assume that correlations in the world of meaning live in ordinary vector space with a common zero point, and not, for example, in an affine complex vector space with curved dimensions. Is there any convincing argument to prefer one above the other? In physics, which uses models of space, this is far from a trivial question, but one which actually defines the space in which one can do coherent observations and make coherent forms of analysis. In quantum physics, for example, it is impossible to work with ordinary vector space, and only complex vector can do the job. In the world of meaning on the other hand, it does not matter, because once we made a choice, we can continue to work with that choice, because space itself does not answer. Another thing we can do numerically with meaning is, again, not measure meaning in itself, but the physical and biological conditions which go with the production and processing of meaning, such as, for example, the speed of nerve conduction while paying attention to something, or the amount of oxygen saturation of blood in various areas of the brain while thinking, etc. In fact, that is how "general" experimental psychology started, namely when Wilhelm Wundt, as assistant of Herman Helmholz in Heidelberg, used the newly developed measurement techniques in neurophysiology to make the philosophically assumed functions of the mind, such as attention, discrimination, association, etc. visible through such type of measurements, and called it for the first time in history "Psychologie wie Wissenshaft", or literally "Psychology as Natural Science". There is a huge resurge nowadays of that kind of psychology with the advent of new techniques to measure changes in activity of different areas of the brain while performing mental tasks, usually referred to as "neuropsychology", but just like before, these measures do not measure the content of meaning, but only the physical and biological changes which go with the production and processing of it.

The recognition of experimental social psychology as Art of Theatrical Reflection instead of as Science of Discovery

But once we realize that the study of meaning is not the same as the science of matter, as well in the logical as in the methodological sense of the word, then question immediately arises what "experiments" in the discipline really are, and what we can eventually do with them in our cultivated dealings with society. In my earlier writings about that question, I have used the term "Art of Theatrical Reflection", in contrast to "Science of Discovery". Let me explain. In the science of matter, experiments are typically special arrangements to look further or deeper in parts of Nature which existed all along, but were too small or too far away to be seen and measured with the naked eye, such as the telescopic exploration of distant stars, or the microscopic study of small particles on earth. We call that "Science of Discovery", or literally of taking the cover of invisibility away from what existed already long before, but had not yet reached our senses to become part of our empirical world in a measurable way. In social psychology, on the other hand, experiments are typically special arrangements to reproduce very visible and usually well known social phenomena in the theatrical form of psychological experiments in a lab, with subjects as the actors and psychological tasks as roles. For example, the very visible and well known phenomenon of conformity in groups was reproduced by Solomon Ash as an experiment in perception, in which naïve students conformed with the utterly erroneous responses of other "stooge" participants in the same experiment. The equally visible and well known phenomenon of obedience to legitimate authority in hierarchical systems was reproduced by Stanley Milgram in the form of an experiment on learning, with naïve participants who complied with the experimenter's request to deliver painful electric shocks to another "stooge" participant who made errors, etc., etc. That is definitely not discovery, but transformation, or making something what we actually know already visible again, but in a different form. In fact, it does what our ancestors also did when they painted their hunt on the wall of their cave, and "reflected" on it with their peers, and probably also educated their children, in the language of this self-made mirror on the wall of their cave. Today, we no longer "reflect" on our past, nor "imagine" our future in the language of self-made mirrors on the wall of our cave, but in that of paintings in museums, or novels, or puppet-theatre, or films, drama, opera, schoolboards, power-points, you-tube, dance, multi-media performances, etc., and, yes, also in that of self-made reproductions of social life in the form of psychological experiments.

Experiments in the world of meaning are totally different from that in the Science of matter, because they only become commonly "experiential", and in that sense "experimental", when they carry meaning, or become "semiotic". This self-made art of theatrical reflection, however, easily creates the "illusion of discovery", because the expressions which are put on stage have not been seen before, not because they existed already long before in that form, but were too far or too small to be seen with the naked eye, but because they were never put on stage in that particular form before. And they also give a sense of objectivity, because when we look at them together from the same distant theatre, we easily see the same. And last but not least, they also give us a language of exclusive expertise, because to count as an expert in the discipline, one has to know, just like critics of ordinary theatre, who wrote the piece, for what purpose, with what kind of narrative, etc. But any claim that this language of exclusive expertise is the result of the exclusive discovery of something that existed already long before, but was never seen or heard in that particular form before, would be utterly absurd, and would even deny the constant use of that new language in our current society, namely think and talk, and even teach about social phenomena in that new language. It may well be for that reason that the so called "application" of experimental social psychology to societal concerns is typically not rewarded with patents, as if usually the case in applications of experiments in the science of matter, but with copy-right, or literally the "right" to think and talk and teach about social life in the language of officially published experiment in peer reviewed journals. If there is one branch in the social disciplines which might deserve the title of science of discovery, then it is definitely not experimental social psychology, but rather cultural anthropology, because experts in that field "do" visit places where we usually do not go ourselves, and tell us later what they have seen and experienced there. In the past, we had to rely only on their verbal reports, but nowadays they can bring all kinds of recordings with them by which we can see as well, and sometimes even better "what is there" than if we had gone there ourselves. The latter is certainly the case nowadays with the plethora of documentaries on ordinary TV about the life of animals in virtually all places on earth, from the highes mountains to the deepest crests in oceans. That also makes us reflect on our own human life, but fortunately not only in terms of similarity, but also in terms of difference, because although ants and primates may do things sometimes which resemble what we also do, they have not played Bach yet in the Concertgebouw of Amsterdam, nor have sent a conspecific to the moon and brought him back alive. That is what we do, and much more.

Theatrical Reflection as part of thoughtful action: from only looking back, to the imagining and making of the future

But when we accept the status of theatrical reflection of experiments in the discipline, then the next question, of course, is what exactly we can do with them in our cultivated dealings with society. My answer to that question is rather obvious, namely reflect, but then not merely in the passive sense of mirroring the past and imagining the future, but also in the active sense of helping to create the future, and use these self made artefacts as tool of more thoughtful ways of doing that. We are the only species, as far as I know, which externalizes past experience to reflect on it in that type of “shared experiential”, and in that sense “experimental” form. It is amazing, however, in retrospect, how much this idea of experiments in the discipline as tool of more thoughtful action, was already present in the life and work of one of the most important protagonists of the discipline, often called the father of the discipline in post-war USA, namely Kurt Lewin. Already before his departure to the USA in 1933, Lewin had performed several projects of emancipation for women and workers in Berlin, and continued to do so after his arrival in the US, with the Harwood project as probably the best known one. But to demonstrate the value of his interventions, Lewin also used comparison groups, or groups in which his interventions were not used, to so see the difference. By interventions in Lewin’s case, however, we should not think of the classic standardized manipulations in RCT type experiments, but rather as suggestions in which the participants themselves could also co-decide on how to plan and how to execute the project, or something what later has become known as “Participatory Action Research”, which is the true legacy of Kurt Lewin in social and organizational psychology. In fact, Lewin himself has done very few RCT type experiments himself, certainly for somebody who is often quoted as the father of the discipline in postwar USA. That was more something what some of his students did, such as Festinger, Thibaut, Deutch, Kraus, etc., while other ones continued to specialize themselves in the guiding of groups in action research, know as “group dynamics”. I have known many of these students of Lewin’s in person, especially John Thibaut, with whom I spent a whole summer at his department in Chapel Hill, and talked a lot about the life and work of Kurt Lewin. It was very clear in these conversations that Lewin did not think of experiments as science of discovery, but more as art of theatrical reflection, and used them mainly as tool of demonstration, together with other tools, such as, for example, film, of wich Lewin made several himself, and was even aided in that endeavour by probably one of the most dramatic directors of all time, namely Sergej Eisenstein. This is well described in a couple of scholarly papers (1992, 1993) by one of my very first students in social psychology in Tilburg, namely Mel van Elteren, who studied the life and work, plus the surrounding social context of Kurt Lewin in great detail.

This demonstrational or persuasive function of experiments in the discipline has continued to be used afterwards, but more and more veiled in the semiotic imitation of the science of discovery, to so gain status and credibility. In some cases, however, the persuasive function was not veiled at all, but appeared loud and clear in the casuistic approach to an issue. The probably best known example of such a casuistic approach is the famous “Stanford Prison Experiment”, or SPE, performed by Zimbardo in 1971. After his move from New York to Stanford, actually to replace Leon Festinger who moved from Stanford to New York (and had actually left experimental social psychology by that time, because he felt it was too slow to get something done in society, and solved that problem by going back to general experimental social psychology instead of engaging in participatory action research), Zimbardo asked students to role play the role of guards and prisoners in the cellars of the University, but had to end this Navy funded study prematurely, because some of the students started to display behaviour which was also observed in the real prison of Abu Graib years later (and to the judicial trial of which Zimbardo was called in as expert witness for his experience in Stanford). Zimbardo, who was later elected as president of APA (American Psychological Association) has been severly criticized for that so called “experiment”, but actually “demonstration”, because, as they said, he actually “directed” his students in the direction of what he wanted, to which one can immediately reply that, even if that were the case, it also illustrates how little power one needs, in this case that of Stanford professor, to let people do things of which in other circumstances they would say will never do. I have known Zimbardo personally very well, as I was his research assistant in the 1967 EAESP Summer school in Leuven, in which he was one of the teachers, and even was the experimenter in the so called “deindividuation” experiment in that School which Zimbardo published a year after under the spectacular title of “Deindividuating the Belgian Army” (because participants in the experiment were privates from a nearby military basis who were asked, in individuated or deindividuated circumstances, to punish a fellow private for making errors, or a procedure which resembled very much the one which Stanley Milgram had also used before him in the study of blind obedience). I later learned that Zimbardo, who graduated from Yale, had gone to the same highschool in Brooklyn as the one where also Milgram had gone to, and that there was a strong emphasis in that school, with even a semi-professional theatre on campus. I have to admit that I liked the demonstrational style of Zimbardo's, not only in his case studies (of which he did several more), but also in his classic RCT type experiments, because it made his argumentation very clear and persuasive.

A final plea for experimental social psychology as tool of thoughtful action

So, there is basically nothing wrong with using experiments in the discipline as Art of Theatrical Reflection, because as I just said above, the use of artefacts as mirror of the past and as imagination of the future, to so engage in thoughtful action, is probably one of the most human things we can do, and which distinguishes us from other animals who only follow the path of their biological evolution, and who do not create culture (at least not to an extent that we can see). To then sell that art as science of discovery, as if it were an astronomy of the mind, is only a means to gain credibility and status, but does not achieve that goal at all, but, on the contrary, locks us further in self-made chambers of cleverness from which we tell on high heels what other people actually know already long before, but in a different language. The division of language which results from that self-made seclusion also leads to a remarkable psychological phenomenon of its own, namely the so called "unconscious", or the pointing at a discrepancy between how ordinary actors explain their own behaviour and how the so called external experts do this, what implies that, in practice, there are as many forms of the unconscious as there are communities of external expertise. In the community of psychoanalysis, for example, the other people who attribute their own behaviour to learned habits are said to be unconscious of their infantile impulses, whereas in the community of behaviourism, the other people who attribute what they do to infantile impulses are said to be unconscious of their learned habits. And in ancient Greece, those who attribute what they do to either of these two forces are said to be unconscious of the hidden manipulations of sneaky Gods. The best way then to get rid of the unconscious is to either ignore the experts (if that is possible at all) or to educate people until they become experts themselves. It may well be for that reason that experiments on the unconscious are usually done with first year students in psychology, and not with last year ones, because by then "they know", at least we hope they do.

Another implication of the use of experiments in the discipline as Art of Theatrical reflection is that the so called "laymen" in the discipline can no longer be defined as the ignorant outsiders who are only entitled to deliver their data, but have no right to understand themselves what they do or say. In fact, they then become the knowledgeable insiders who lend us their language of society to invent scenarios of reproduction and to interpret what we have done. To then pretend that we are the ones who enlighten their lives by our so called discoveries is like borrow money, and when we give it back in a different currency, pretend that we are the ones who lend it, and even deserve to get paid for it. A better way then to do research would be to work with them directly, and try to learn as much from them as we expect them to learn from us, as in fact Kurt Lewin did with his famous participatory action research, and what I have also tried to do when, after years of experimentation with young students in a lab, I have started to do engaged research with experienced practitioners who were willing and able to use their own reflective practice as basis of learning, and tried to make this transferable in the form of a scholarly dissertation. I have been much aided in that endeavour by colleagues from the Taos Institute, or an Institute that was founded and directed by Kenneth Gergen, of which I spoke above, and that was particularly dedicated to that type of inquiry. This has been one of the most enriching periods of my career, not only in terms of learning, but also in terms of getting things done.

But all that, I like to mention loud and clear, is not without still enjoying an experiment from time to time, especially when it is clear and elegant, but not to exclude outsiders, but on the contrary, to include them in a shared experiential, and in that sense "experimental" form of reflection. The problem with the discipline is not that it exists, but that it is often sold as science of discovery, with the semiotic imitation of that science as tool of persuasion, but to actually tell stories which people know already, but with in a different language. This can even go so far that the numbers which are used in that endeavour are not mentioned in their authentic form, but as narrative moulding in the story, as happened for example, with Elliot Aronson, former Chief Editor of the Handbook of Experimental Social Psychology, who wrote about the Festinger and Carlsmith experiment, I quote: "The results were clear-cut: Those students who were paid twenty dollars for lying – that is, for saying the spool packing and screw turning had been enjoyable – actually rated the activity as dull. This is not surprising – it *was* dull" (e.g., Aronson, 1995, p. 202). The "actual" ratings, as I mentioned in the beginning of this paper, were respectively -0.05 and -0.45 on a scale from -5 (very negative) to +5 (very positive), or as close to the zero midpoint of the scale as possibly can be. I not think that this would count as a fair description of actual measurements in even the most science of matter, but, indeed, as a narrative moulding of numbers in the author's desired story. This tendency is rather common in the discipline, and even happened to my own highly esteemed supervisor, the late Jozef Nuttin, despite his nearly phanatic emphasis on precision and control in research. In his later research on self-persuasion by verbal role playing, following upon his critical replication of the Festinger and Carlsmith experiment, he found that students who despised their conventional exam system at their University –

an average evaluation of 2.7 on a scale from 1 (very against) till 17 (very pro) – rated the system on average 7.6 and 9.2 after helping their unconventional female professor with role playing a very positive attitude toward the system on respectively the National Radio and National TV. In his theoritical, say "verbal" description this finding, Nuttin called it an "acognitive verbal assimilation" of the rating to the very positive words which were used in the role playing, but, of course, that is impossible, because the word "neutral" or "don't know" (which is the verbal equivalent of an intermediate rating), does not resemble the word "very positive" at all, in any case not more than it resembles the word "very negative". Or to say it in a different way, it is not because we know already, on other grounds, that orange is a mixture of yellow and red, that a shift from yellow to orange on a scale from yellow to red, can be called an "acognitive verbal assimilation" of the rating to the word "red". A more plausible description then of the shift would be that the students, after their confrontation with a very unconventional professor who nevertheless asked them to publicly role play a very positive attitude toward the conventional system, did not know anymore what to say, and chose the middle of the road, because "with such a professor, one never knows", and that is what the students said "don't know", or "neutral". But whatever the explanation, the experiment made us think and talk about what happened in a more reflective way, and so made us imagine what might happen again, and prepare for it in a more reflective way. In that sense, or in the sense of tool of shared experiential reflection, I thing that experiments in the discipline, together with other forms of art and other disciplines in Humanities, deserve to be used in the scholarly education of anybody who prepares for more thoughtful and responsible action in society. That is my vote.

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