Qual Quant (2011) 45:263–291
DOI 10.1007/s11135-009-9293-0

Causality in qualitative and quantitative research

Jacques Tacq

Published online: 5 January 2010

Abstract We are flooded with a wave of writings on causality in the social sciences during the last decades. The same holds for the relationship between quantitative and qualitative research in the social sciences. An enormous amount of texts appears on (causality in) qualitative research, mostly in a controversy with quantitative research. These writings induced us to develop the thesis of "unity in diversity", i.e., that there is no difference "in principle" between causality in qualitative and quantitative research, because both are supported by what I will call an "experimental logic". In developing this thesis a plea is being made for going back to the sources. A historical overview of theories of causality is presented, which develops into two prominent views: INUS-causation and causal realism. A historical framework is also outlined for the opposition between quantitative and qualitative research, in which French positivism and British empiricism are opposed to German neo-kantianism and neo-hegelianism. After having developed the thesis of "unity in diversity" for this historical framework, the same is being done for the recent literature: "mixed methods research", the book DSI of KKV, the reactions of David Collier and "QCA" of Charles Ragin. At the end the question of small-n research and the case n = 1 is examined.

Keywords Causality · Explanation versus understanding (quantitative research versus qualitative research, positivism versus hermeneutics) · INUS-causation · Causal realism · Experimental logic · Counterfactual conditional

1 Introductory observations

When looking at the literature of the last decades on "qualitative research methods", more particularly on causality in qualitative research, one cannot help thinking of that book of the Dutch historian Annie-Romein Verschoor with the wonderful title "Omzien in verwondering" ("Look Back in Wonder"). Especially in the world of political scientists, notably

J. Tacq (✉)
Catholic University of Brussels, Brussels, Belgium
e-mail: jacques.tacq@hubrussel.be

those who concern themselves with international comparative research, there appear many publications, one after the other, on "qualitative comparative analysis", "comparative case-studies", "causal inference", "counterfactual analysis", "process tracing", "research designs in qualitative analysis", "moving beyond qualitative and quantitative strategies" and many other writings. They are getting very nervous and that's how it goes rather often among political scientists. One of the titles even makes mention of "Causality in Crisis?" Apparently there is a book of three authors King, Keohane and Verba entitled "Designing Social Inquiry" (King et al. 1994) which is considered so important that countless reviews are devoted to it and one even confines oneself to the abbreviations KKV (the names) of DSI (the title) when reference to the book is being given. Charles Ragin too, with his book "The Comparative Method" and Collier as well, with his writings on "Selection Bias" score points on the political scientist citation-index. Next to that, the pleas for "Mixed Methods" in the social sciences, among others from Teddlie and Tashakkori, and Creswell, contain—within the framework of integration of qualitative and quantitative research—discussions of causal explanation and inference. Big words are being used. Just like in the recent—more general—revival of qualitative research methods one speaks occasionally of "The Interpretive Turn" and "The Fifth Moment" (see among others Denzin 1994), in the same way Tashakkori and Teddlie (2003) refer to mixed methods research as representing "The Third Methodological Movement".

In all these discussions—predominantly from political science—there are explicit or implicit references to debates from other disciplines and from the past. I give some examples. Already in *mathematics*, which will be considered as typically "quantitative" in the minds of many, we can make mention of a "qualitative mathematics" when we look at René Thom's Catastrophe Theory. Also in *logic*, which is in the classical sense an extensional logic in which the validity of arguments is determined independent of the content of propositions, we can see—when looking at Montague-grammatics—an intensional logic in which the intension, i.e., the content of the proposition, is also taken into account. Another example is *economic science*, which will most certainly be considered as quantitative by many, but which was designated by Werner Sombart—in a prominent book of 1930, "Die drei Nationalökonomien"—as "Eine Verstehende Ökonomie" (An Understanding Economy), because economic conduct has to be understood from the motives of the conducting individuals. The same holds for *historical science*, in which a quantitative approach will always be situated in a context of narrative historiography, counting within a context of recounting (narrating, telling). And the distinction between *classic and romantic science* refers to the Russian neuro-psychologist Alexander Lurija who wanted to develop a synthesis between the nomothetic and idiographic approach in the social sciences in his two famous case-studies, "The man with a bullet in his head", about a soldier who experienced serious memory problems being the result of a bullet in his brains, and "The Mind of a Mnemonist", about an ex-journalist with an inexhaustibly large memory who developed into a mnemonic artist and was investigated by Lurija during more than 30 years.

In all these debates there is a significant reference to Max Weber, who had already built a *bridge* between the quantitative and qualitative approach in the social sciences in nineteenth century. Indeed, *positivism* à la Comte and Mill is too one-sided. *Hermeneutics* à la Dilthey, Windelband and Rickert also has its one-sidedness. And sociologist Max Weber succeeded in bringing "Verstehen" (Understanding) of hermeneutics and "Erklären" (Explanation) of positivism together in what he called "Erklärendes Verstehen" (Explanatory Understanding). His view was brilliant and it is really very remarkable—we have to think again of Annie Romein-Verschoor—that there still exist one-sided sociological schools of thought, such as "Symbolic Interactionism" in Amsterdam, which still sails under the flag of "Verstehen", and "Explanatory Sociology" of Utrecht and Groningen, which still swears by "Erklären",

whereas Max Weber has offered us such a beautiful proposal of synthesis already more than a century ago. Consequently, it goes without saying that a further elaboration of Weber's synthesis deserves recommendation.

Not only with regard to sociological schools and lines of reasoning, but also with respect to the subject of *causality*, we encounter many obscurities and inadequacies. Collier, Teddlie, Ragin and the aforementioned authors KKV of the book DSI, all of them abundantly make use of causal terminology. But here too, the words they use are bigger than the debates they take part in. They discuss randomization, the independence of observations, the possibility of statistical generalization, specification error, the measurement level, sample size and other statistical subjects, also a discussion of thick and thin analysis, but a real fundamental discussion of causality is not to be found. Here too, a link with philosophical ideas would be more than welcome.

2 Formulation of the problem

With the above constatations in mind it becomes clear that two things have to precede a thesis on causality in qualitative and quantitative research: 1. an explanation of the concept of causality and 2. a discussion of the area of tension between qualitative and quantitative research. Accordingly I will first give an overview of the main debates on *causality*. After that I will do the same for the field of tension between *qualitative and quantitative research*. Thereafter I will develop the thesis that there is *no difference* "*in principle*" between causality in qualitative and quantitative research. For, even though these two types of research look like two totally different worlds, I will nevertheless—making use of a generalized concept of 'experimental logic'—try to point out that the underlying thought-pattern about causality is in fact the same. This is the formulation of the problem of this contribution. By way of conclusion and following on this thesis I will pursue in greater depth a number of contemporary debates which are held by the aforementioned authors from political science and in "mixed methods research", including the controversy about the question whether a large number of observations is required to perform causal research.

3 Causality in the social sciences

I will now first give an overview of the main theories of causality. This overview is an historical one and will result in a number of prominent views, such as INUS-causation of John Mackie and causal realism of Harré and Madden and in their footsteps Roy Bhaskar. These views will offer the opportunity to develop the thesis that there is no fundamental difference between causality in qualitative and quantitative research.

We may consider Aristotle to be the founding father of causal thinking. He was a philosopher who distinguished many types of causes, e.g., *causa materialis*, *causa formalis*, *causa efficiens* and *causal finalis* (Aristotle 1977). In modern science of the seventeenth century, his causa efficiens or labour cause became the guiding if not the only principle. Causation was then associated with "production". The labour cause was the active agent, the external power was like the knock with a hammer, and the effect was that which undergoes in a passive way, like the chestnut that bursts into pieces. This notion of production is present in Newtons external power which brings a resting body into movement. It is also present in social policymaking. For example, policymakers in France in the nineteenth century started a policy of raising the birth rate and of encouraging immigration to establish an excess of

births over deaths, so that the aging population structure could be reversed. It is also present in the stimulus-response scheme of the school of behaviorism. And it is even present in our language, because many verbs such as "poison", "cure", "calm", "humidify", "illuminate" contain the idea of production.

It was also in modern science that cause was clarified in terms of necessary and sufficient condition by Galilei, where "*necessary*" means "conditio sine qua non" or "If not X, then not Y" and "*sufficient*" means "If X, then always Y". (One should notice that "X necessitates Y" means "X is a sufficient condition of Y". Confusion is possible here!)

David Hume in the eighteenth century is really a turning point in the history of causality. Bertrand Russell called Hume the bankruptcy of eighteenth century rationalism, whence we could name Hume a rascal. In fact no author today can still write about this subject without first discussing Hume. An outstanding example is Karl Popper, who, in his book "Objective Knowledge", has a first chapter titled: "The Hume Problem" (Popper 1966). Hume was not against causality (see Hume 1739, 1748, 1786). He believed that the world was full of it. But he was skeptical about the possibility for science to get insight into the question of why a cause is followed by an effect. His starting-point is purely *empirical*. He argued that knowledge of causal relations was never brought about in an a priori fashion by means of pure deduction, but that it was totally based on experience. Adam could not deduce from seeing water that it could suffocate him. Hume also believed that causes were external to their effects. If a billiard ball collides with a second ball and the latter starts moving, then there is nothing present in the first ball which gives us the slightest idea about what is going to happen to the second one. As for the middle term in between cause and effect (i.e., the causal arrow) Hume stated that such concepts as production, energy, power, and so forth belonged to an obscure philosophy that served as a shelter for superstition and as a cloak for covering foolishness and errors. We see the fire and feel the heat, but we cannot even guess or imagine the connection between the two. We are not even directly conscious of the energy with which our will influences the organs of our bodies, such that it will always escape our diligent investigations. The question of why the will influences the tongue and the fingers, but not the heart and the liver, will always bring us embarrassment. And the idea that the will of a Supreme Being is responsible here, brings us far beyond the limits of our capabilities. Our perpendicular line is too short to plumb these yawning chasms. He concluded that when we saw both the cause and the effect, then there would be *constant conjunction*. After some time of getting used to this, *custom* arises. And then, via some mechanism of *psychological association*, we gradually start to develop a *belief*. And on the basis of this belief, we use causal terminology and make *predictions*. In short, the only thing that really exists is constant conjunction (regularity theory); the rest is psychology. Loosely speaking: correlation is the matter; the rest is chatter.

With the reactions on David Hume, one can fill a whole library. Among others, Immanuel Kant, who first admitted that Hume had awakened him out of his dogmatic half-sleep—because unlike British philosophers of common sense he had at least taken the effort to base his conclusions on thorough examination—refused to accept that only experience is the basis of causality (see Kant 1781). He believed in prior categories of understanding, which, together with experience, brought us the synthesis called objective knowledge. Causality was, in his view, one of these categories of understanding. Although the Kantian view is very interesting, because Kant succeeded, in a wonderful way, to bring together the empirical and rational side of our knowledge, yet this view of causality as an a priori category is not undisputed. Mackie (1974) gave the example of a piece of wood that is cut into two parts with an axe and argued that from a long enough distance we would first see the parts fall apart and then hear the sound of the axe due to the difference of speed of the light and the sound,

but up close we would first hear it and then see the parts fall apart due to the inertia of the bodies. And if a little train on the table bumps into a second train, whereby the second train starts moving, then this has nothing to do with our prior faculty of the mind, because there might be a hidden magnet under the table that brings about the second movement. So, there is not much of a priori causal knowledge here, but rather the act of hypothesizing knowledge in a tentative way, with experiment and observation needed to come to a conclusion.

The tradition of hypothesis testing, often used today in our scientific research, was initiated by *positivist philosophers* of the nineteenth century such as August Comte and John Stuart Mill and became standard procedure within positivist research in the twentieth century. Popper (1959) and Hempel (1965) also contributed to this general attitude by means of their deductive-nomological approach, which is well-known as the covering law theory (i.e., every concrete causal statement is covered by general laws that operate in the background and serve as general theories and hypotheses).

In the meantime, the debate on causality goes on. Most proposals contain a policy of encirclement, because causal production is not approached in a direct way but rather indirectly via some other criteria. One such criterion is "probability" used by a school of scientists who may be called the adherents of *A Probabilistic Theory of Causality*. This is the title of a book of Patrick Suppes, who initiated this school of thought in 1970. His opinion corresponds with the opinion of the majority of scientific researchers in the world—that is, X is a cause of Y if and only if X exists (the probability of X is greater than zero), X is temporally prior to Y (the moment in time t_X comes before t_Y), there is a statistical relationship between X and Y (the probability of Y given X is greater than the probability of Y by itself) and there is no spuriousness (the statistical relationship between X and Y does not disappear when controlling other potentially confounding factors).

These criteria for causality are in use in social research, especially in *statistical analysis*. The "Method of Path Coefficients" of Sewell Wright in genetics in the 1920s (Wright 1934), the "Simultaneous Equation Models" of Herman Wold in econometrics in the 1950s, the "Causal Models" of Simon and Blalock in sociology and other social sciences in the 1960s and 1970s (Blalock 1971), and the "Linear Structural Relations System" (LISREL) of Karl Jöreskog in the 1970s (Jöreskog 1973) and after are but a few of the many examples. Three main problems will always haunt this school of thought. The first is the notion of probability, which explains statistical relationship (i.e., correlation) but not causation. The second is the dependence on theory, which is expressed by the relations between the variables in the causal model, especially by the factors to control for spuriousness. The third problem is that temporal priority is used instead of causal priority, which means that research practice is building on the sophism "Post hoc, ergo propter hoc" (i.e., Thereafter, so therefore).

Another policy of encirclement is the use of Galilei's criteria "necessary" and "sufficient" condition, which is done by many modern authors, but in the most intelligent way by Mackie (1974). He reacts against the extreme standpoint of Bunge (1959), who tells us that we commit the sin of meaning-inflation if we liberalize the concept of causality so that it covers nearly everything. In Bunge's view there are many forms of determination in reality—probabilistic, dialectical, functional, structural, mechanical and other determinations—of which the causal determination is only one. The core of this causal determination is the criterion given by Aristotle's *causa efficiens* (i.e., necessary production). Next to productive, the causal relation is also conditional (If X, then Y), unique (one cause, one effect), asymmetrical (when X causes Y, then Y does not cause X) and invariable (no exceptions, no probabilistic causality). Bunge also gives other strict criteria, like linearity, additivity, continuity and the like. His notion of causality is very rigid. He reacts against functionalists, interactionists and dialecticians, who see everything in terms of interdependence and are, therefore, romanticists. In

their view, causality is a key to every door, a panacea. In Bunge's view, causal relations, if strictly defined, are only a small part of reality, but they exist. They are neither myth, nor panacea.

It goes without saying that Bunge's (1959) rigid definition of causality is a paradise of simplicity. Mackie (1974) reacts against this rigidity in developing a very liberal notion of causality that is close to everyday language and accepts probabilism, multicausality, teleology, functionalism and many other forms of determination. His approach is in terms of necessary and sufficient condition. A causal factor is, in his view, a necessary condition "in the circumstances", which means that silent premises and particulars should, as much as possible, be made explicit. For example, a fire breaks out in a house. Experts claim that this is due to a short-circuit. They do not give a necessary condition, because other things than a short-circuit, such as the falling of an oil-heater, could have caused the fire. They do not give a sufficient condition, because even if there was a short-circuit, other conditions such as inflammable materials or the absence of an efficient fire-extinguisher are necessary for a fire to start. Therefore, there is a complex set of conditions, positive and negative, that together with a short-circuit are sufficient but not necessary for the outbreak of a fire because other factors could have caused the fire. A short-circuit is a necessary part of the set of conditions, for without it, the inflammable materials and absence of an efficient fire-extinguisher could not cause a fire. A short-circuit is then an insufficient but necessary part of a complex set of conditions, of which the total is unnecessary but sufficient for the result. In short: a short circuit is an INUS condition for fire—that is, an *I*nsufficient but *N*ecessary part of a set, which is *U*nnecessary but *S*ufficient for the result. For Mackie the N of INUS is the most important. In his approach the emphasis is on "Necessary condition". This can be understood in terms of counterfactual conditional, which means that, if we would reason counter to the facts such as in "Suppose that no short-circuit had been taken place", the effect would then fail to appear. In linguistics this is known as "irrealis". Such reasoning can also be explained in terms of "possible world approach": in a possible world which would be equal to the actual world up to one point, i.e., that a short-circuit would not have taken place, the fire would have failed to appear.

Mackie adds to this that there will always be factors that cannot vary, but are fixed in the causal field. For example, being born is, in the strict logical sense, an INUS condition of dying, but it cannot be a candidate cause of dying because a statement on the causes of death refers to people who have lived. A less evident example is the syphilis example of Scriven (1959). *Treponema pallidum*, a bacteria, is the unique cause of syphilis. However, only a small percentage of people contaminated by the syphilis bacteria come into the third and last phase of *paralysis generalis*, a brain paralysis accompanied by motoric disorder and mental disturbances and causing death. Now, the first statement about the unique cause refers to a different causal field than the second statement about paralysis generalis. The first is the causal field of all persons who are susceptible to the bacteria, for example, all persons who have sexual intercourse. The second is the causal field of all persons who are already contaminated by the bacteria.

In research practice this notion of causal field is of crucial importance, because it also contains, next to self-evident fixed factors related to the research problem, factors that are fixed due to pragmatic considerations related to time and space (living in a country and doing research in that country) in addition to factors that are actually INUS conditions but based on causal a priori considerations and, due to the danger of heterogeneity, have been fixed (doing research in the causal field of the unemployed and leaving young workers out because youth unemployment is a different problem entirely). Capital sin number one in the social sciences is really the neglect of the causal field.

Shortly after John Mackie proposed the INUS-model in his book "The Cement of the Universe", approximately in the same period, another anti-Humean view on causality was also developed. This was of a totally different nature and was introduced by the book "Causal Powers, a Theory of Natural Necessity" of Harré and Madden (1975), in which, following John Locke, they propose another ontology which is totally different from the thing-event-thinking of David Hume. Particular things and events possess in their view powers, possibilities and potentialities from within and that's the whole point of causality. In a Humean world events A and B are external to each other and we search solely for the constant conjunction between the two. But in the ontology of Harré and Madden A possesses a causal power which brings about a persistent generative mechanism, which then produces B. They give the beautiful image of a man who takes forty winks in a chaise longue in the garden on a fine sunny afternoon with a moderate heat and not a breath of wind. There are no flies, no mosquitos, no wasps and no shouts of the neighbour's children. Suddenly the man jumps from his chair, runs rapidly to the garden house, takes the mowing-machine and starts mowing the grass. Nothing outside him has changed. His successive acts are the product of his decision, the source of which has to be found in a condition inside him. Represented in a simplified way one could say that Harré and Madden bring the causal arrow, which was according to David Hume situated in-between A and B, inside A. For, A possesses an internal power, which can bring about a causal mechanism and can hence produce B. "Can" produce B, for albeit that the production of the result B is latently present, it will in some cases not be realized in a manifest way. If that would be half of the cases, then Humeans miss out on half of causality in the world, for they only concern themselves with cases of manifest realization.

In the footsteps of Harré and Madden, Bhaskar (1978) has given—in his book "A Realist Theory of Science"—a systematic treatment of this theory of causal powers and natural necessities, of which some scientists would say that it represents a recent "Copernical revolution" in philosophy of science. Bhaskar makes a distinction between classical empirism à la Hume and transcendental idealism à la Kant and he defines his own position as transcendental realism. This causal realism is about generative mechanisms and structures of the world, which form the basis of "natural necessity", i.e., necessity in nature independent of human beings or human activity. In Bhaskar's view, unlike Mackie above, necessity does not refer to the counterfactual conditional, but rather to the transfactual conditional, with which he points out that the activity of generative mechanisms and structures represent a reality independent of the factual outcome, in other words that it is latently present but is not necessarily manifestly reflected in the facts, as was already explained above. Knowledge is in his view a social product, but the things of which we produce knowledge exist independent of us. Actually, there are two dimensions in philosophy of science, a transitive dimension, according to which previously built knowledge is utilized to generate new knowledge, and an intransitive dimension, in which the object is the real structure or mechanism that exists and acts independent of us. It is of course the intransitive dimension that stands in the centre of the discussion here. Causal realism is a view on science which is essentially oriented towards possibilities. Much attention is being given to tendencies, powers and potentialities. These tendencies can be realized, but it is also possible that they are not realized or that they are realized but not recognized or not discovered by human beings (Bhaskar 1979).

It goes without saying that David Hume would turn in his grave if he would read all this, for he stated that such concepts as production, energy, power, and so forth belonged to an obscure philosophy that served as a shelter for superstition and as a cloak for covering foolishness and errors. But we might possibly look at it in another way. For, we stated above that in a Humean world events A and B are external to each other, but that in the ontology of causal realism A possesses a causal power which brings about a persistent generative mechanism,

which then produces B. In Bunge's view A is no longer an event, but a causal process in itself. As we suggested above, the causal arrow is now being brought inside A, so to speak. But this is precisely what John Goldthorpe also says in an interesting book "On Sociology. Numbers, Narratives, and the Integration of Research and Theory" (2000, p. 158), namely that we should (when treating causality as a generative process) draw the logical conclusion to test statistical models *of these processes themselves*. In other words, the unit of observation and analysis is no longer the thing or the event, but the causal process, the mechanism. In this way we save—so to speak—positivism, but we give it flesh and blood. An interesting line of thought!

Many *authors of causal realism* have put the "causal mechanism" in the centre of their thinking. Salmon (1984) designates causal processes, causal interactions and causal laws as the mechanisms by which the world operates. Nancy Cartwright too highlights real causal mechanisms in her thinking. In her view things and events have causal capacities and, due to the properties they possess, they have the power to bring about other events or situations (Cartwright 1989). In the same way Jon Elster focuses upon the study of mechanisms in his book "*Nuts and Bolts for the Social Sciences*" (1989a) (the expression "nuts and bolts" means: how something works, how it is assembled) (see also Elster 1989b).

So far for the most important fundamental debates about causality, which are relevant for research in the social sciences. Many subjects which rise in the practice of research are related to these important theories. For example, the controversy on the question whether a limited number of observations (or even óne observation, the case n = 1) is sufficient to conduct a causal investigation as against the question whether a statistical analysis of big numbers is necessary, comes in fact down to a debate between on the one hand David Hume and his regularity theory and Patrick Suppes with his probabilistic theory, who is essentially a disciple of Hume, and on the other hand John Mackie with his anti-Humean point of view, who runs counter to this. And in "process tracing", "historical analysis", "detailed thick analysis of cases" and other proposals of contemporary authors, the same frame of reference and also causal realism will be full-scale present in the background.

4 The area of tension between quantitative and qualitative research

As causality is nowadays often related to qualitative research, in a controversy with quantitative research, I will now first shortly summarise what is the long and the short of the field of tension between quantitative and qualitative research. I will hereafter also situate it within an historical framework. It will become apparent that the terms qualitative and quantitative and the discussions about their opposition have so many connotations that it will not always be simple to draw a clear line of demarcation. I will first give a general situation-sketch and I will thereafter restrict myself to those approaches which have causal ambitions.

In general we have a dualism, with *two groups of social scientists* who are opposite to each other, who have each their own world and who speak a different language (→ two language thesis). In one group we have the thing-event-language, with concepts like: thing, event, laws, cause, causal explanation. The other group speaks the person-action-language, with concepts like person, action, rule, reason (motive), mental explanation (= understanding).

This *list of concepts* can be enlarged immensely! The oppositions mostly refer to differences in methodology: quantifying as against qualitative research; exact measurement and generalization as opposed to being close to the data, to do no violence to the unique character of reality and its complexity, to let the persons involved formulate their own interpretations, to let concepts emerge during the research; further also a deductive as against an inductive

approach; statistical testing as opposed to exploratory research; "testing" versus "gauging"; objective against subjective approach; searching for laws that hold for all time-periods and contexts as against looking for insights into the "here and now context", therefore time-dependent and where the context is essential for the insight; and further the well-known distinctions such as emphasis on reliability opposed to emphasis on validity; correspondence between theoretical concept and operational variable in one group as against correspondence between reality and our concept of reality in the other group; "la cuisine scientifique" (the scientific kitchen) versus the gate of science; "context of justification" against "context of discovery"; and with a view to the research procedure: statistical analysis with large samples and enquêtes versus in-depth interviews and participant observation; strive for distance between investigator and investigated as opposed to strive for closeness between investigator and investigated, such as in "Verstehen", "taking the role of the other" and "inner perspective"; and more generally: "Kausal Erklären" (causal explanation) against "Deutend Verstehen" (interpretative understanding); causality versus teleology; causa efficiens (labour cause) versus causal finalis (final cause) (Aristotle); variable language against the intentional aspect of action, and "last but not least": reductionism against holism.

In addition to this methodological emphasis, the discussion has taken *many other forms*: a *political* one, in which (Anglo-Saxon) liberalism comes up against neo-marxism; an *axiological-ethical* contrast, in which reference is given to value-free research (critical distinction between values and facts) as against value-committed research (emancipatory); a contrast between different *philosophical positions*, which refers to the controversy between positivism and dialectic thinking, or between positivism and hermeneutics, or more in general between positivism and anti-positivism; and further also relating to different *scientific disciplines*, i.e., natural sciences and the humanities.

The difference lies *ultimately deeper*: it is in fact an epistemological contrast between *realism* (metaphysical realism) and *idealism* (transcendental philosophy in which the subject is seen as constituent for the object).

In this contribution the emphasis will be on the methodological angle, but it goes without saying that it will not always be possible to distance ourselves from the other emphases. The link with the issue of causality will especially become clear in the historical framework, of which I will now gave a brief sketch.

The historical frame of the contrast between explanation and understanding refers to two traditions, two philosophical positions, i.e., positivism and hermeneutics.

Positivism refers to names from nineteenth century, such as August Comte and also Emile Durkheim in France and John Stuart Mill in England, also Bentham and Spencer. In fact it goes back to philosopher David Hume in eighteenth century. In twentieth century it refers to logical positivism—or also logical empirism or in general neo-positivism—of the Vienna Circle, with names such as Moritz Schlick, Otto Neurath, Rudolf Carnap and many others, and in their wake Karl Popper and Carl Gustav Hempel.

Hermeneutics is neo-Kantian, idealistic and situated within philosophy of language. Names from nineteenth century are the neo-Hegelians Droysen and Dilthey and the neo-Kantians Windelband and Rickert from the school of Baden in Germany. Simmel and Weber can also be situated here, albeit that they take up particular positions. We already mentioned above the explanatory understanding of Weber. Names from twentieth century are the Italian B. Croce and the Englishman R. Collingwood (idealistic wing of hermeneutics) and the German H. Gadamer (interpretative hermeneutics).

When we take this historical frame as our angle, then it is predominantly a debate that has been started by the nineteenth century German school of neo-Kantians and

neo-Hegelians as a reaction against British Empiricism (Hume, Mill) and French Positivism (Comte, Durkheim).

However, many other perspectives can be given. I mention five of them. *Firstly*, in this whole debate between positivism and hermeneutics there is next to a debate on sociology and social sciences, also involved: a debate on *historical science* and philosophy of history. Think, for example, of the French Annales-school and her fight against event-history (van den Braembussche 1985). A *second* angle can be drawn from Ludwig Wittgenstein. His "Tractatus Logico-Philosophicus" and his "Philosophische Untersuchungen" (Philosophical Investigations) have given rise to school formation, "Tractatus" to logical positivism with the emphasis on observable natural events and the use of the aforementioned deductive-nomological scheme of thought with particular attention to regularities and general laws, and "Philosophische Untersuchungen" to a totally different position, in which a pluralism of "language games" is presented which gives room for the linguistic usage and in which the emphasis is now on human actions, meanings, intentions, grounds, goals, rules, norms, maxims and contexts (Wittgenstein 1981, 1999).

A *third* line of thought starts from American *pragmatism* with as representatives William James, Charles Peirce and John Dewey. They consider something true if it works. True is what is being made true. Unlike German idealism, there is no search for ultimate grounds. James replaces the emphasis and asks: "What is the cash-value of an idea?" What are the profits, the results? Following on pragmatism a line can be drawn to symbolic interactionism (Blumer, Kuhn) and also to many other American sociologists, like Mead ("I" and "me" as part of "self"), Cooley (looking glass self), Thomas and Znaniecki (The Polish peasant), Goffman (labeling theory), Glaser and Strauss (The Discovery of Grounded Theory) and many others, who all represent the qualitative view, in reaction against positivism.

A *fourth* angle runs from *phenomenological sociology*. Here reference is given to Alfred Schütz, who described social reality as composed of interpretations of interacting subjects. This phenomenological sociology has exerted a strong influence on Berger and Luckmann and especially on etnomethodology of Harold Garfinkel, also on Cicourel, Sacks, Zimmerman, Wieder and many others. It is a view which can also be placed against positivism.

A *fifth and last* line starts from logical positivism and is brought into action by Thomas Kuhn in his book "The Structure of Scientific Revolutions" (1962). Kuhn's ideas have initiated an enormous discussion of the question what science is. For, with his view Kuhn demands attention to the way in which science really operates. The static, logical-epistemological models of Popper and Hempel (deductive-nomological model of explanation) are certainly insufficient: they have to be completed with a socio-historical component. Not logic as such, but the use of it, the way science is used in the course of history becomes important. This is in line with pragmatism. This debate has been held world-wide and is still running. After Popper and Kuhn came Lakatos, Feyerabend, Sneed, Laudan and today still Van Fraassen, Hacking, Cartwright and many others. Connections can be made here, too, with Wittgenstein II, because words obtain their meaning from their use and context, just like becomes apparent in the notion of language games.

In all mentioned lines of thought, hermeneutics, Wittgenstein II, American pragmatism, phenomenological sociology and Thomas Kuhn's view, the matter at issue is a polemic against positivism. It is in this polemic that the problem of causality is involved. Consequently, we have to first ask ourselves what is really meant by positivism. The term refers to the main work of August Comte "Cours de philosophie positive", a work of six volumes at which he has worked during 12 years and in which is explained that each branch of knowledge passes through three stages, theological, metaphysical and positive (scientific) stage, respectively. These three stages are related to the spiritual development of mankind as a whole and

according to Comte also to the individual, for he writes: "Who does not remember to have been theologist in his childhood, metaphysician in his youth and physicist as adult?" Maybe Comte exaggerates here, but probably there is to be found more truth in the fact that it holds in his view also for the sciences themselves: all sciences were dominated first by theological concepts, then by metaphysical speculation, to come finally into the mature stage of positive science. The term "positivism" implies: rejection of metaphysics. The basic principle of positivism is: start from that which is given, which is factual, which is "positive" and reject all questions and elaborations that go beyond it as useless. And that which is given, factual, positive, that is simply the phenomena. It follows that positivism restricts science (and philosophy) to the domain of the phenomena. We must accept these phenomena as such and we must try to order them and come to scientific laws—laws of similarity and laws of order—and from these discovered scientific laws we must try to foresee future events and on the basis thereof intervene in the world. In other words, we must ≪Savoir pour prévoir≫ (to know in order to foresee), statement of Francis Bacon, to which Comte adds: ≪et prévoir pour pouvoir≫ (to foresee in order to control). Here we are in the middle of the problem of causality. It is perfectly clear that in Comte's view it makes no sense to ask for the 'essence' of something or for the 'deep' or 'true' causes. Positivism relies solely on facts and concrete experiences, on phenomena which can be observed by the senses. However, according to Mart-Jan De Jong it remains thé problem of positivism à la Comte what is meant by the expression "positive". Comte seems to refer to three things: (1) That which is real, is positive; that which is not real, is negative. (2) That which is meaningful and useful, is positive; the senseless and useless is negative. (3) That which is sure and can be determined exactly, is positive; that which is unsure and cannot be determined exactly, is negative (such as in positive law, which is the entirety of laws in force, as opposed to 'natural' law). Comte himself has pointed out that all three meanings apply to positivism. Therefore, he restricts himself to the real thing, to the social useful thing and to that which can be determined exactly, in contrast with the endless quarrels of earlier metaphysics.

The ideas of John Stuart Mill in England, nineteenth century, are philosophically totally in line with August Comte, also with Bentham and British empiricism. In his book "A System of Logic, ratiocinative and inductive", a work that has been standard textbook at most universities of the world and has become a classical work with high popularity, Mill discusses his well-known methods of experimental research for the natural sciences as well as the social sciences and he also explicates his philosophical starting points, which come down to positivism à la Comte, as we already indicated above (Mill 1872). The Kantian thesis that knowledge of our world is inferred from prior assumptions is mistaken, according to Mill. All statements, no matter how abstract or hypothetical, have their ultimate origin in experience. If our memory would possess sufficient capacity to stock up and order all observed particular facts, then we could in fact reason without general propositions. But as we do not possess this capacity, we make use of marks which label the many particular facts, so that we can assign the same label to a new fact. Once different marks are available, groups of facts are again labeled by means of marks of marks. Hence: the real inference is always from particular fact to particular fact, from many observed cases to a new case. But in making this inference we use marks as a guide. Therefore, we make a "train of reasonings", i.e., a whole series of inductive inferences through marks of marks. It follows that our statements are in origin inductive, not prior and deductive. The start is always: observations and experiments. This does not mean that there is no room for deduction. Each science aims at becoming more and more deductive, i.e., at acquiring knowledge of general laws. But this makes such an advanced science no less inductive. It can always be lead back to the inductive initial phase.

Now that we have made clear, by means of the views of Comte and Mill, what positivism, and with it the quantitative school, really means, we can oppose it against qualitative research. As we already mentioned, the historical frame of the opposition between the quantitative and qualitative approach in the social sciences refers mainly to the reaction against positivism of neo-Hegelian Dilthey and neo-Kantians Windelband and Rickert of nineteenth century in Germany.

Wilhelm Windelband made a classification of nomothetic and idiographic sciences. The nomothetic approach of the natural sciences is oriented toward general laws. The idiographic approach of the social sciences aims at knowledge of the individual, the unique, the particular of once-only events (idio is ancient Greek for "oneself"). The meaning of idiographic lies in the expression of the sense that lies in the single event and that is related to our feeling of value, such as in the intuitive understanding of a piece of art.

The most important student of Windelband and also his successor in Heidelberg was Heinrich Rickert. Together they represented the South-West-German Baden School of neo-Kantianism. Rickert observed that Windelband distinguishes natural sciences and the humanities on the basis of their difference in method and not in terms of ontology. Rickert agrees with this epistemological point of view and goes even further. He refuses to make the distinction between natural science and the humanities, for it is not in the subject but in the method that lies the basis of the distinction. Two ways of looking, not two pieces of reality (German: "Natur", "Geist") are the matter. For, we cannot make a portrayal of reality (German "Abbildung"), we always make a transformation of reality (German "Umbildung") by means of points of view, angles. It is interesting to notice here the enormous influence of Immanuel Kant. Well, according to Rickert there are two ways of looking at reality, generalizing, that is with the focus on the general, and individualizing, with the focus on the particular. The natural sciences as well as the cultural and historical sciences can apply both approaches, but in practice it appears that natural science applies predominantly the generalizing method and that cultural and historical sciences apply mostly the individualistic method. For example, the historian is interested in the particular case as such, in Napoleon, in the Renaissance, in the French Revolution. Here Rickert emphasizes that the choice of such a subject is a moment of selection, which finds place on the basis of certain values. We make a choice from the infinitely many phenomena and this choice is made on the basis of values. So, there is value-commitment, value-involvement. Such a value-commitment will also be shown in the way the scientist looks at these subjects, for every scientist is a child of his time in which certain values apply. He will study the context, he will make a puzzle, from part to whole and back from whole to part, and, in doing so, he will reckon with the values which hold in that context. Rickert emphasizes that this does not mean that the historian expresses a value-judgement on the selected facts, such as "Napoleon is son-of-a-bitch" or "The French Revolution was good or bad". The expression of a value-judgement holds in the sphere of belief, in the sphere of acceptance and rejection, in the political arena in which the "Battle of Gods" (German: "Streit der Götter") is fought. So, it does not mean value-judgement. But is means value-relatedness, i.e., that events are related to values as a theoretical activity.

This view of Heinrich Rickert will be almost completely taken over by Max Weber, albeit with another terminology, which comes rather from neo-Hegelian Wilhelm Dilthey. Indeed, it was Dilthey who made the distinction between Erklären (explanation) and Verstehen (understanding). From him comes the statement: "Die Natur erklären wir, das Seelen-leben verstehen wir". (We explain nature, we understand spiritual life.) The "understanding", which applies predominantly to the humanities, comes according to Dilthey down to "Erleben" (be keenly sensitive, feel intensely), which means to live through innerly. For historical science this is only possible as "Nacherleben" (where Nach stands for afterwards). According to

Dilthey one does not act externally, by observing external facts, but rather internally, by understanding from within, by interpreting and comprehending. In this connection he writes about positivism the following crushing statement: "The fact one was not prepared to believe everything, that was the tremendous power of positivism; the fact that it crippled the spiritual world in order to force it into the straitjacket of the external world, that was its limitation". Originally Dilthey wanted to found the internal working-method of the humanities on psychology, later he relies more on hermeneutics. This means the principles of understanding, interpreting and comprehending, original as protestant textual criticism: one studies texts, one looks at all elements of the text in relation to the whole. In the same way one is going to study cultures and eras by understanding all elements of a culture in relation to the whole and, in so doing, bringing forward the unique (the whole of meanings), which is totally different from developing causal laws. For example, Dilthey refers to a scientist who produces a work. This event is part of the truths which all together constitute science. It is also an economic process because of the production and sale of copies. It has also a juridical side, because a contract is signed. And it can be part of a bureaucratically organized occupational practice of the scientist. So, if we want to "understand" (Verstehen) the work of the scientist, then we have to keep informed about the state of affairs in science, about the economic situation of the book market, about the juridically arranged demands of the publisher, about the bureaucratically organized occupational practice of a university, and also: the social, religious and political backgrounds of the scientist. All this is called the first context by Dilthey: the context of interaction–relationships. The second context is the biography of the scientist, his identity. For a good understanding of his work we have to know his intentions and motivations, his development, etcetera.

For both contexts the interpreter will be confronted with the circle from part to whole: one understands the text only when placing it in the context, and vice versa: for an understanding of the context one is reliant on the reading of separate texts. In order to break through this circle we use (distinction of Schleiermacher): next to the comparative method, in which the text is compared with other texts in order to better understand, also the divinatoric method, which proceeds in an intuitive fashion, not a descriptive one, and consists of "Sich-hineinversetzen" (to transpose oneself in the entirety of studied life manifestations) and "Nacherleben" (which refers not only to empathizing or getting the feeling of something, but which is a reconstruction of the process).

We will see hereafter that a wrong impression is created here. There is the suggestion of a different concept of causality as compared to the quantitative approach, but a closer look will make clear that the underlying logic is basically the same. This will be discussed later.

With the treatment of French positivism and British empiricism and the reaction of German neo-Kantians and neo-Hegelians we have brought together the most important elements of the field of tension between quantitative and qualitative research and the historical frame it fits in. The later discussions about this in the twentieth century are maybe not a mirror image, but still a continuation of nineteenth century Erklären-Verstehen-controversy. But nevertheless there is one exception that is so original that it deserves special mention, i.e., the Polish sociologist and philospher Florian Znaniecki, well-known by his research together with the American sociologist W.I. Thomas on Polish immigrants in the United States (1918), who explicated the principles of his qualitative method, called "analytic induction", in a later publication "The Method of Sociology" (1934). This is a method in which research units are examined one by one and in which theoretical insights are adjusted to each observation. This process of continuous reformulation of the research hypotheses completes when new observations do no longer offer new insights, i.e., when theoretical saturation takes place. Glaser and Strauss (1976) used the expression "theoretical sampling" for this procedure.

Znaniecki joins battle with the statistical method (he uses other words, such as enumerative induction). It is bugging him that the statistician first generalizes. For, the statistician studies a great number of cases and looks for characteristics that are common. And he thinks that these features can be abstracted in the conceptual sense because of their generality. But in fact the process has to run the other way round. Enumerative induction abstracts by generalizing. Analytic induction on the other hand *generalizes by abstracting*. Starting from concrete cases, those characteristics are abstracted that are essential and only thereafter one generalizes, presuming that in so far as essential, they must be similar in many cases and hence possess a larger degree of generality. In his view a hierarchy of characteristics has to be established in terms of gradation of importance, so that structural dependencies between characteristics can be mapped.

In Znaniecki's view the principle of structural dependence is not the same as the *principle of causality*. According to him more is needed for causality. Two requirements must be kept in mind when analysing facts of causation in the social world. First, nothing happening within a social system calls for causal explanation which does not constitute a change of the system as a whole. Secondly, nothing can change the system as a whole which does not irremediably conflict with the original significance of its values, hence, there has to be a conflict with the prevailing values in the axiological sense. Counter-examples of the first requirement are a quarrel between the members of a group or the disobedience of a child towards its parents. There is no causality here, as for each of these actions they are either originally implied in the very structure and composition of the system or there exists a counter-action, which avoids the consequences for the system as a whole. There is also no causality in the case of latent tendencies, because then the second requirement is not fulfilled. For example, a parent may wish to give his child certain educational advantages, but is unable to afford it, because he is too poor. Or suppose that certain groups in society cannot function in a normal way because of political repression. Obstacles of this kind are only technical obstacles, which hinder the actual realization of a system, but do not affect the structure of the system. The latter occurs only when there are axiological obstacles, i.e., when the essential values of the system are conflicting. This is for instance the case when immigrant children come into contact with children of the community and start despising the cultural standards of their parents–immigrants and accept instead the standards of the new milieu.

Maybe Znaniecki is too strict here with regard to causality, but nevertheless his analytic induction is an original addition to the debate about quantitative and qualitative research. It is really a pity that contemporary debates on this have to a large extent been decreased to solely the opposition between statistical analysis of big numbers and case-studies of small numbers, or, in the words of Ragin: "the variable-oriented approach" and "the case-oriented method". Before examining this further I will now first develop the thesis that there is no fundamental difference between causality in qualitative and quantitative research, because both are founded on what I will call an "experimental logic".

5 Causality in qualitative and quantitative research: the very same experimental logic

A first—already mentioned—general remark which we have to make when we bring up the subject of causality, is that causal production is seldom characterized in a direct way, but that most characterizations imply an indirect approach, a sort of strategy of encirclement, via other criteria. It looks as if we want to get on the causal train, but, as we cannot take part in the train journey, we have to content ourselves with observations at the different

stations and platforms and from that try to come to valid statements about the rail journey. In the fundamental debates one such strategy of encirclement was the characterization via "probability", another via "necessary and sufficient condition". However, in contemporary debates it also happens—we look back in wonder once more—that the discussion is just about something totally different from what it stands for. For example, in the many debates about causality there is a constant discussion about the measurement level of the variables. There is a tendency to link causality to the quantitative measurement level according to which variables are measured as a ratio scale or interval scale. In this way ordinal and categorical variables are erroneously excluded in causal models. There are also authors who are inclined to restrict themselves to dichotomous variables, because they discuss causality from a bivalent logic. Both viewpoints are unrealistic. The distinction between measurement levels is purely statistical–technical in nature and has no direct bearing on the problem of causality as such. In a similar way there are a lot of discussions about the identification status of a system of mathematical equations in a causal model and about the different estimations procedures used to solve such a system, discussions which have got nothing to do with causality but are, alas, considered as such.

Something comparable happens occasionally in the world of qualitative research, in a polemic with the nowadays so abused positivism. The term "qualitative" is sometimes used as in "qualitative measurement level", synonymous with "nominal" or "categorical". Having in mind the fundamental debates of Dilthey, Windelband, Rickert and Weber, it is of course absurd to reduce the discussion to that level. But even if one does not do that, it still remains very often a discussion which is restricted to a contradistinction between research methods, e.g., between the case study approach and statistical analysis, which is rather meager when compared with the big debates on positivism and hermeneutics.

The reverse also happens, i.e., a broadening of the discussion to such an extent that it causes an inflation of meaning which looses content. An example is the introduction of the "Handbook of Qualitative Research", written by Norman Denzin and Yvonna Lincoln. What they understand by qualitative research is almost everything: paradigms, epistemologies, interpretative frames and perspectives (hermeneutics, semiotics, phenomenology, etnomethodology, symbolic interactionism, cultural studies, constructivism, postpositivism, postmodernism, feminism, critical theory, Marxism, multi-paradigm-orientation), nature of the empirical materials (cases, personal experiences, biographies, stationary images, life histories, narratives, ethnographic prose, fictions, parables, conversations, interactions, visual texts), methods and data research strategies (interviewing, in-depth-interviewing, observation, participant observation, visual methods, investigation of personal or historical documents, archive work, clinical research, psycho-analysis, self-reflection, introspection, deconstruction, multimethod orientation, triangulation; they even mention here the statistical method, survey research and computer-assisted methods!) and orientations on different disciplines (anthropology, sociology, cultural studies, historical science, communication sciences, pedagogy, interdisciplinary orientation; they even mention physics here!).

They call a qualitative researcher a "Jack of all trades", a "bricoleur" (somebody who is underhand and acts deviously), an all-rounder who is able to turn his hands to anything, a do-it-yourselfer. With such a broadly-based and widely-ranging view of qualitative research one goes from one extreme to the other. One then no longer knows what one is talking about. What it then comes down to is to find a midway in-between narrowing-down and broadening of the meaning. That's why I gave above a rough sketch of the historical frame, starting from French positivism and British empiricism and afterwards elaborating the subsequent reaction of German neo-Kantianism and neo-Hegelianism. This frame, in which the contrast was predominantly seen as a controversy between Explanation and Understanding, is in my

view the best way to indicate which discussion is at stake, without being reduced to an all too restricted discussion of statistical analysis and without giving it a broad, all-embracing and self-undermining meaning.

Let us now deal with the question whether there is a difference between causality in qualitative and quantitative research. From the afore-sketched historical overview we deduce especially the view of our spiritual father Aristotle and his follower Bunge that causality comes down to "necessary production". Production is a term which is comparable to human "action" but is disposed of antropomorphism. "Necessary" was already defined by Galilei as "conditio sine qua non", but in that time it still figured in a very restricted frame of thought with óne cause and óne effect. With his INUS-model John Mackie has extended this frame of thought to a disjunction of conjunctions in a causal field, albeit that he stuck to "necessary condition" as the central characteristic of causality, which he defined as a "counterfactual conditional" in a possible world approach.

This idea of counterfactual conditional in INUS-causation really represents causal connection, as opposed to logical connection. For, if I look at a pure logical connection—for example from "I am the parent of" follows logically "I am older than"—then the switch-over to the negations will bring about a change of order (contraposition), for, from "I am not older than" follows logically "I am not the parent of", but from "I am not the parent of" it does not follow logically "I am not older than", for, the girl next-door of two years of age is not my daughter, but I am much older than her. In other words, in logic the expression "p $\Rightarrow$ q" has the same meaning as "not q $\Rightarrow$ not p", in which the order of p and q has been changed. But in the "counterfactual conditional", which represents the causal connection, things are really different. With Mackie's example in mind that the short-circuit is an INUS-condition of fire (p $\rightarrow$ q) the switch-over to the negations results in the expression "without short-circuit no fire" (not p $\rightarrow$ not q), in which the contraposition is not applied.[1]

It turns out that this "counterfactual conditional" perfectly coincides with experimental logic. For, in a controlled experiment two groups which are equal (or do not differ beyond random) are compared, an experimental group in which stimulus p is introduced, which brings about q, and a control group, in which stimulus p is not introduced, after which q fails to come. This is the same logic as in the "counterfactual". And that is why I shall use the expression "experimental logic", which fits in with Mackie's INUS-analysis and with John Stuart Mill's method of difference and which was considered by Mackie—paraphrasing Hume—to be the cement of the causal relation.

This experimental logic is also present in the research design of 'before and after observation', but there the control is more difficult, because in-between the two observations there is a time lag during which no other relevant factors than p are allowed to show a change. The very same logic is present in complex survey-investigations with multivariate analyses, in which one causal factor within a whole set of factors receives the attention. Such a causal factor is then a variable which varies, in the simple case of a dichotomy it varies from yes to no or from present to absent, just like above from p tot not p. And in the statistical analysis it is looked for whether the effect variable, when controlled for other variables in the model, also varies from present to absent, just like above from q to not q. For variables with more than two categories this analysis is simply extended to what John Stuart Mill called "concomitant variation" and which we indicate nowadays as statistical association or correlation.

This very logic is also present in case-studies. For it is not because cases are investigated that there are no causal relations between the "characteristics". And from the moment that

[1] Another formulation could be (p & c1 & … & cn) $\rightarrow$ q, in which p and all ci are necessary for the result q. This formulation is more in line with INUS, but it does not change the main argument given here.

more than one case is involved in the investigation, these "characteristics" will vary over cases and hence be "variables". It turns out that the investigation of only one single case—for which Cook and Campbell scornfully used the expression "one shot case-study"—is no science. For, even one single case is always compared with something, if necessary the whole population and then that case forms the one group, which is then a singleton, and all cases minus that one form the second group. Consequently, there is always comparison, even if there is—as it were—only one case. Anyway, case-studies are seldom restricted to two groups. For example, suppose one investigates some phenomenon in companies—such as role patterns of men and women—then the results will be very different in food industry, chemical industry, transport companies, agriculture, post offices, railway companies, dock industry, road-building or mining. Even in the case of case-studies researchers will select one or more cases for each or some of these branches of industry. Whence the expression "multiple case-studies" is in use. But such a selection of branches of industry in which cases are selected will soon resemble the construction of strata in a stratified random sample design, which is applied in surveys. And when cases within and between these sectors are compared in order to infer causal statements from that comparison, then the very same logic of the "counterfactual conditional" is used, which I referred to as "experimental logic".

Let me now explain in more detail and also broaden this point of view that many forms of investigation—quantitative or qualitative—deal with causality from one and the same basic logic. To do this, I refer to the book "Matière et forme" of Apostel (1974), who elaborated the anti-Humean and anti-Kantian view of Mario Bunge and tried to clarify systematically the notion of "production". Let it be noted in passing that Apostel tries to develop in this work a realistic epistemology, which has much ground in common with the works of Harré and Madden, and Bhaskar, but that he could not have read and assimilated Bhaskar, whose book "A Realist Theory of Science" was published one year later. But that is mentioned here only casually. In contrast with what Hume thought, Apostel believes it possible to give a meaning to the notion of production.

Hume declared that, based on observations, we can only discover constant conjunction between phenomena. Apostel accepts this as a starting-point en agrees with Hume that we cannot observe the causal arrow. But he defends the thesis that, when combining different observations in a judicious way, we can increase or decrease the likelihood of causal statements, even if we know that such statements refer not only to the actual world, but also to a possible world (albeit that this possible world is really possible and not only logically possible). In developing this thesis a number of basic options are taken. Firstly, events are conceived as spatio-temporally extensive, but are investigated within a partial history. Secondly, this partial history is divided into elements: processes, objects, situations, events, etcetera. Thirdly, attention is given to one or some of these elements. If we restrict ourselves to two elements and request a cause, given an effect (other cases are also considered), then this causal factor is conceived as a privileged element of a context. What the procedure then comes down to is to investigate this (complex) context and assign rules for the selection of a privileged element that 'produces' the result. For that purpose a basic scheme R–O is designated in a formal-abstract way: a relationship R and an order O are defined. This basic scheme lays the foundation for the analysis of productive causality. As the notion of production is an abstraction which departs from human action, it follows that the latter is an example of realization of the R–O scheme. For, human action is characterized by three elements which are interrelated: agent, instrument and material. Next to a relationship, an order is also present: the person taking action makes use of instruments to manufacture the material, whence the order is 'agent → instrument → material'. But this comparison with human action is only an analogy. To free the notion of production of antropomorphism the term 'actomorph' is introduced. That

a causal relationship is actomorph means that it is morphologically comparable with human action. An extensive logical analysis of the R–O scheme, for which I refer to the work itself (pp. 165–200), leads to the following conclusions. Hume is wrong, for, albeit that we cannot observe production, even so we can still observe the actual realizations of the R–O form. This is done—I translate freely—by feeling one's way over the environment of the causal factor and bringing about a hierarchy in it. Such an environment can be spatiotemporal in nature. We then look for other factors, which are situated closer to or more remote from the effect in time and/or space and we try to weigh up these factors against the privileged factor. Such an environment can be understood in terms of necessary and sufficient conditions. We then try to find other factors which are also necessary condition of the effect and which form together with the privileged factor a sufficient condition set, just like in the INUS-analysis of Mackie, which is conceived by Apostel as a special case of R–O realization and which is further extended to more complex combinations of necessary and sufficient conditions. It is also possible to set up a qualitative research in which one can verify to what extent different elements of the environment of the causal factor have characteristics in common with the effect, after which an order can be established. If desired, such an analysis can be quantified or formulated in probabilistic terms. Forms of hybridization between the aforementioned R–O realizations are also provided for. The context of the privileged factor contains further also non-realized possibilities and conditionalities. The latter point deserves particular attention. A simple example, which Apostel (p. 67) borrows from Stalnaker, is as follows. Let us assume I wonder how my employer will react on my possible attempt to obtain a raise of salary. Thus, I want to evaluate the following not yet realized conditional statement: 'If I tried this, then I would obtain that'. How can I, starting from real observations, obtain data about what is possible? Stalnaker mentions the following means: a. I could ask myself how my employer has reacted on other attempts that were undertaken by other persons or by myself on other moments; b. I could ask myself how my employer reacts now on comparable, non-identical questions; c. I could ask myself which are the consequences of my request or of a possible refusal or acceptance; d. I could ask myself which are, in the real world, the presuppositions of my request and of a refusal, etcetera. The degree of confirmation of a conditional statement will be larger to the extent that a larger number of such analogies are realized in the actual world.

We see that exploring possibilities in the neigbourhood of the causal factor (or in the neighbourhood of the effect or of the middle path) can help us in making production plausible. The INUS-analysis of John Mackie is an example thereof. To show that X is an INUS-condition of Y in causal field F one has to make an analysis of the environment of X, i.e., of the elements which form together with X a sufficient condition set, and of non-realized other sets which were realized at other moments and in other situations. Randomization in the standard experiment, and even the introduction of a control group, are actually also a modest start of investigation of the context. For, randomization is a strategy which has the purpose to eliminate other factors. Even if one does not thereby examine the context explicitly (such as in a multivariate analysis), it nevertheless means that the possible operation of so-called external variables is implicitly taken into account. Providing for a control group is in fact also grafted on an argument in terms of possible worlds. It means that the N of INUS, which refers to the logic of Mill's method of difference, is taken seriously: if X is a necessary condition of Y, then in a possible world which differs from the actual world in only one point, i.e., that X is absent, also Y will be absent. In the control group this possible world is as it were actualized. As we have seen above in the discussion of Apostel's view one can also in a qualitative research investigate to what extent various elements of the environment of the causal factor have characteristics in common with the effect, after which an order can be established. We have discussed several examples thereof. For instance, in the theory of

Wilhelm Dilthey, who reasons from hermeneutics, texts were divided into elements and were investigated in connection with the whole. In his example of the understanding of the work of a scholar he proposed to investigate two contexts, a first context of interaction-dependencies and a second context of the biography of the scholar, so that one could afterwards reconstruct the whole process by means of "Sich-hinein-versetzen" (comparable to empathy) and "Nacherleben" (relive afterwards). Analytic induction of Florian Znaniecki, too, is a clear example of Apostel's R–O realization. He starts from concrete cases and abstracts those characteristics that are essential (and hence possess a larger degree of generality) and after that establishes a hierarchy of characteristics in terms of gradation of importance, so that structural dependencies between characteristics can be mapped. These examples do not yet express well that the context of the privileged factor contains also non-realized possibilities and conditionalities, such as indicated in the example of my attempt to obtain a raise of salary, example which Apostel borrowed from Stalnaker. Another very beautiful example thereof is the research of Deutsch and collins (1951) of white people in New York, in which it was investigated whether going to live in integrated projects (together with black people) instead of segregated projects (X) is cause or effect of diminished race prejudice (Y). A number of strategies from the many which we encounter in this investigation are as follows. 1. If X is the causal factor, then a longer exposure to its action should bring about a more forceful action of Y: interviewees who lived longer in an integrated project showed less race prejudice than those who had taken up their residence only recently. 2. Questions that are retrospective in nature: from answers to the question "What did you think of black people before you came to live here?" it appeared that the attitudes in integrated and segregated projects were initially equal. 3. Investigation of the possibility for the effect variable to function as causal variable: for example, for those who ended up in integrated projects in spite of race prejudice one might expect that they would move after some time; in fact only few if any moves took place; another example is that refusals to accept a rented house in one of the projects were only in a few cases related to racial problems. 4. Investigation of individuals for whom racial prejudice (Y) was not yet filled in when the causal variable X appeared: for little children who can (supposedly) not yet have a clear meaning with respect to racial prejudice when moving into the house, it turned out that those who lived in integrated projects showed less race prejudice than children from segregated projects. 5. If the other causal direction would hold, according to which inhabitants of integrated projects had given preference for living there, then one might expect an unprejudiced attitude towards other coloured people; in contrast with that they showed an equal degree of discrimination towards Puertoricans as the inhabitants of segregated projects; moreover, inhabitants of both kinds of projects differed more in their attitude towards black people with whom they lived together than in their judgement of black-people-in-general. In this research example we see how the different strategies (extension of the causal variable from 'going to live there' to 'duration of residence', retrospection, to consider the effect variable as possible causal variable, research with children and to consider other effect variables) are an attempt to investigate also non-realized and otherwise- and elsewhere-realized possibilities and conditionalities in the context of the privileged factor (X), thereby gaining strength in the statement of causality and its direction. It is entirely clear that the "experimental logic", such as formulated above, is here also the guiding principle, even in this—predominantly qualitative—investigation. I hereby hope to have convinced the reader that there is no difference "in principle" between causality in qualitative and quantitative research, because both are supported by the same basic logic.

6 A number of contemporary debates

After this plea for unity in diversity we will now examine a number of topics which were recently under discussion. However, for the subject that occupies our mind here, there is no sense in entering at length into "mixed methods research". For, causality is not really brought up there. A distinction is made between QUAN and QUAL traditions. The QUAN tradition stands for postpositivistic approach, reduction to variables and hypotheses, use of measurement and observation, emphasis on testing of theories, use of experiments and surveys and the gathering of data which leads to statistical analysis. The QUAL tradition, on the other hand, is mainly founded on constructivistic perspectives because of the attention for multiple opinions which are socially and historically constructed, with a view to developing a theory or a pattern, which is oriented towards political issues and makes use of narrations, ethnographies and case studies and of an inductive approach. Mixed methods research is a new way to perform research in which information of both traditions are brought together in óne investigation. Many designs are provided for and it is agreed upon that a + sign will mean that both kinds of data are gathered simultaneously (QUAN + QUAL), with capital letters for the orientations which receives highest priority (quan + QUAL for a predominantly qualitative study) and with a little arrow when the procedure is not simultaneous but sequential in nature (QUAL → quan for a study which starts with qualitative data and subsequently adds quantitative data, albeit that the latter are less important). With these agreements, proposed by Teddlie and Tashakkori and others, one can already compose a great many designs. Creswell still added other things to this list, such as an explanatory or exploratory perspective, a nested or non-nested approach and the like. It is clear that a discussion of causality is barely at stake here. Neither really is a form of integration, because the QUAN and QUAL traditions still remain a bit separate.

Things are different in the case of the aforementioned KKV, Collier and Ragin. They really include the causal body of thought in their works. Their examples come mainly from the political sciences. Their writings are methodological in nature. For example, the book "Designing Social Inquiry" of G. King, R. Keohane and S. Verba (KKV) is a textbook of methodology of political science for students of the second year of a bachelor's degree. I can be very short about KKV. Their definition of causality is just the one which was named above "counterfactual conditional". They place it within an experimental logic and they show that in qualitative research, even for a "single unit", the same definition holds. They also deal with causal mechanisms and name as examples thereof the nowadays much debated "process tracing", "historical analysis" and "detailed case-studies", but they take the view that their definition of causality as "counterfactual conditional" is logically prior to the identification of causal mechanisms. It follows that their view is a special case of Leo Apostel and John Makcie (both not mentioned by them), with a little Bhaskar-sauce on top. It is a pity and causing confusion that they use deviating terminology for themes of discussion that are world-wide known. They use the terms "endogeneity", "conditional independence", "random causal effect" and also "homogeneity" for something totally different from what is usual in the literature. But apart from all this their textbook is very suitable for students of political science, with the experimental logic as the basic line of thought, with many examples from the field of political science and with a view which transcends quantitative and qualitative research, albeit that they do not explain what qualitative research really means, and with our aforementioned reserve for terminology and a number of statistical–technical discussions.

Their most important opponent is David Collier, whose writings take their book as a starting-point and go over their view with a fine-tooth comb (Brady and Collier 2004). He actually agrees with their definition of causality in terms of "counterfactuals" and founded in

experimental logic, but he thinks that causality is thereby only discussed as abstract concept. He is of the opinion that this abstract definition links up with the practice of experimental research, but he is sceptical about the practical implications in non-experimental observational research. The discussion of all the details thereof would lead us too far here. One topic of discussion is the difference between random selection (such as in random sampling in a survey) and random assignment (such as in randomization in an experiment) and the meaning of both in small-n studies. Another topic is Bayesian statistical analysis, which can be important for small-n studies and which is neglected by KKV. Other topics are sample size and (absence of) multicollinearity, which are in the view of KKV a sine qua non for causal research and which represent in the view of Collier an all too narrow—albeit useful—idea (Collier and Mahoney 1996). A debate is also held about what KKV name "data mining" and which is known in the literature as "multiple comparisons"—here too again deviating terminology. Then again, in the answer of Collier on this topic it becomes clear that he overlooks the existence of "testing of contrasts" and also of "screening" and "stepwise procedures" in multivariate analysis, which serve as a handle in the case of multiple comparisons. A controversy is also carried on about the aforementioned "conditional independence". While participating in the discussion Collier comes to four crucial criteria to make the distinction between quantitative and qualitative research, i.e., measurement level, sample size, use of statistical testing and thick versus thin analysis. We can be very short about measurement level and statistical testing. For, we have already indicated before that these have nothing to do with the discussion. The measurement level of the variables (or in research without variable-language the "characteristics") can be ratio, interval, ordinal or categorical, in quantitative as well as in qualitative research. It would be absurd to pay attention to this any further. The use of statistical tests is a second criterion mentioned by Collier. It goes of course without saying that large surveys with big numbers will show more "statistical" strength than small-n studies, but that is a discussion about the "power" of a test and therefore a totally different discussion. In fact one always performs a test, even in the case of small numbers, because—as I explained before—one will, even in a small case study, always compare; and to compare is to test, because it is to "check", to "verify". Consequently, I also do not wish to pay further attention to this second criterion. With the discussion about the third criterion, big or small numbers, I will close hereafter. Now there only remains the one about thick and thin analysis. Collier claims that in using these concepts he refers to the discussion of Coppedge about thick and thin concepts and asks us not to be confused with the distinction of Geertz between thick description, which is directed at the meaning of human action for the actors involved, and thin description, which is not directed at this meaning. With thick analysis he means that the analysis is based on a detailed knowledge of cases. Many scholars consider thick analysis as the most important characteristic of the qualitative tradition. Quantitative researchers on the other hand rely on thin analysis, because their knowledge of each individual case is far less complete, which is why they can work with larger numbers and are in a position to perform statistical tests. My master Ivo Molenaar expressed it always as follows: the statistician is allowed to throw away his pieces of scrap paper, because the arithmetic means and variances are sufficient to perform his statistical analysis; he no longer needs the details of all individual units separately. In the view of Collier the distinction between thick and thin analysis is very closely linked-up with the distinction of Ragin between case-oriented and variable-oriented research, which we will examine further shortly. Collier does not really explain well what he understands by thick analysis. Dilthey's context-investigation, Apostel's R–O scheme and Mackie's INUS-model, in which the environment of a privileged factor is explored, can possibly offer a good frame of reference, of which Collier's proposal is a special case and from which he could benefit. Besides, the idea that we aspire to a more detailed knowledge of a small number of cases,

given that this is impossible for large numbers, is known for a long time and reveals itself for instance in two-phase sampling. This is a form of sampling consisting of two phases, a first one in which a very small questionnaire is drawn up, which is subjected to a big random sample of individuals, and a second phase in which a small random sample is drawn from the big sample, followed by a long-lasting and detailed in-depth interview with this small number of individuals. Thin and thick analysis are then combined in óne investigation, so that in accordance with the mixed methods research a QUAN → QUAL-design emerges.

Another topic which receives special attention by Collier is the causal mechanism. In the footsteps of KKV he names "process tracing", "historical analysis" and "detailed case-studies" as examples and discusses the possibility that such mechanisms, also called causal processes, are observed. To that end he gives all kinds of examples, of the Bush-Gore neck-and-neck race in US president elections, of presidents in Latin America who show after being elected a reversal of policy in the direction of neoliberalism and of the causal impact of the nuclear taboo on American decision-making. We come away as wise as we went from these examples, because it is not made clear how such an observation of a causal process takes place in research practice. And this is really such a pity, for keeping in mind the analyses of Mackie and Apostel on the one hand and of Harré and Madden and Bhaskar on the other hand, a wonderful opportunity is provided here to bring together two large traditions—one of thinking in terms of causal logic (counterfactuals) and one of thinking in terms of causal realism (generative mechanisms)—in the actual practice of political research. In my own PhD-thesis, long ago, before I could have read the works on causal realism, I have given a modest initial impetus to such a practice. I will pursue this question in greater depth when discussing the last theme of this contribution, the theme of small and large numbers in social science investigation. But first I will say a word about Charles Ragin.

In a prominent work "The Comparative Method. Moving Beyond Qualitative and Quantitative Strategies" of 1987 Ragin has presented a proposal to perform causal research by means of Boolean analysis. He makes use of binary data with ones and zeros (Extension for polytomies is possible). An example is the analysis of the causes of peasant revolts with four causal factors: A = persistence of peasant traditionalism (1 = yes, 0 = no), B = commercialization of agriculture (1 = yes, 0 = no), C = the existence of a substantial class of middle peasants (1 = yes, 0 = no), D = the residential preferences of the landed elite (1 = absentee, 0 = resident). Truth tables with zeros and ones are constructed to indicate the different combinations of four binary causal factors and one binary effect factor (R = peasant revolt: 1 = present, 0 = absent). Frequencies of occurrence are added in the truth table for preparation of statistical analysis, but frequency criteria are in Ragin's view not the most important, because the focus is rather on types of situations. Boolean addition and multiplication are applied and are equivalent to disjunction (the logical operator OR) and conjunction (the logical operator AND) from logic, respectively. Uppercase letters indicate the presence of a condition and lowercase letters indicate its absence. In this way one obtains a Boolean equation, for example R = ac + aD + BD + Abd, which expresses that there is either a combination of low level of peasant traditionalism (a) and few middle peasants (c); or a combination of low level of peasant traditionalism (a) and absentee landlords (D); or a combination of commercialized agriculture (B) and absentee landlords (D); or a combination of peasant traditionalism (A), little commercialization of agriculture (b) and resident landed elites (d). A reduction is applied in this equation when certain combinations are logically impossible. Furthermore all kinds of procedures of Boolean minimization are applied until no further stepwise reduction of Boolean expressions is possible, which comes down to reducing the equation to its logically minimal kernel. Thereafter Morgan's laws are applied: the occurrence of revolts R is transformed into the remaining absent of revolts r and the equation

is recoded accordingly. An analogy with the logic of "necessary and sufficient conditions" can be made. Factorizing is also possible. The Boolean algorithm has been implemented in a microcomputer package called QCA (Qualitative Comparative Analysis) which carries through the reductions.

Ragin has not restricted his Boolean analysis to a pure algorithm. His proposal is embedded in a whole philosophy of science, in which the big debates are not avoided. From him comes the nowadays much quoted distinction between "variable-oriented approach" and "case-oriented approach", which has much ground in common with all aforementioned distinctions (which are not mentioned by him), such as explanatory and interpretative approach, nomothetic and idiographic point of view, generalizing and individualizing method, QUAN and QUAL and others. The interesting thing in his view is that he requests from us to take care of causal thinking, for the Boolean operation with reductions forces us to put matters concerning content in the center of causal research, rather than methodology or statistical analysis. And at the same time, being a qualitatively oriented researcher, he still keeps an open attitude towards statistical analysis, for in his view QCA can be perfectly combined with discriminant analysis, loglinear analysis, logit and probit models and logistic regression. But one is not obliged to perform a statistical analysis of big numbers. An approach with small numbers, in which the focus is more on the detailed analysis of cases—one can compare here with the thick analysis of Collier—is also possible. In a separate chapter, with examples, he also devotes himself to strategies in which the variable-oriented and case-oriented approach are combined, even synthesized, albeit that he does not explain clearly what the latter would really mean. Anyway, QCA is an original and promising approach which is still developing. Furthermore, one can always find information at the "international resource site" http://www.compasss.org. Let me now by way of conclusion pass on to the promised discussion of causality in small-n studies.

7 Causality in small-n studies and the case n = 1

Many scholars—today and even already decades ago—have asked the question whether a limited number of observations (or even óne observation, the case n = 1) is sufficient to conduct a causal investigation, or whether a statistical analysis of large numbers is necessary. One of them is Ragin (1987). He realizes that a small number of cases is not sufficient for the application of a technique of statistical analysis. For, the possibilities of systematic control and of generalization are then enormously reduced, so that the qualitative study with small numbers is inferior as compared to the statistical method. But he resists. Even apart from the fact that the number of available units is sometimes very limited (such as in international comparative research of countries) and that there are sometimes more explaining variables than cases, so that a huge problem of degrees of freedom emerges, he contends that the case-oriented approach has a strength which is absent in the variable-oriented approach. And this strength is what John Stuart Mill has named the chemical combination of causes, the combined and holistic character of explanations, causal complexity, multiple conjunctural causation. He uses these different expressions to indicate that various causal factors in combination and in interaction together constitute an explanation of a social phenomenon and that it is best to map these different combinations. A case-oriented qualitative study pays unbelievably much more attention for that than a quantitatively oriented statistical study and Ragin's proposal of Boolean approach is an example of such a qualitative approach. This Boolean approach even offers the opportunity to choose the midway in-between complexity and generalizability. Indeed, researchers who apply this method can diagnose causal

complexity and at the same time deal with large numbers. However, it is a pity that Ragin indicates nowhere what has to be understood by small n or large n.

But Collier on the other hand does give us an indication. He puts the cutting-point somewhere between a number of 10 and 20. In an attempt to place the discussion somewhat broader he refers to examples of qualitative studies with quite large n and also to studies which rely heavily on statistical testing but actually work with small n (of 11 and 15). How or where he did get hold of the number 10 or 20 is not clear. One could refer to the bottom of a t-table (for Student's t-distribution) and the z-table (for the normal distribution) and place the minimal n there where t-scores come pretty well in the neigbourhood of z-scores. But what is "pretty well in the neigbourhood"? It goes without saying that the determination of the sample size is a complex matter. It depends on many things, among others the number of variables included and the research design. It is thereby important to keep in mind what my colleagues of Groningen have called the *NCB*-strategy (in Dutch language KVP): when performing data-a*N*alysis (in Dutch language dataverwerKing) to first think about data-a*C*quirement (dataverwerVing) and even maybe about data-a*B*andonment (dataverwerPing), because if one wants all cells in a multidimensional contingency table to be filled, then one will be forced to keep the number of variables and the number of categories per variable reasonably small. In my book "The Methodological Atelier. Recommendations and Observations for the Social Sciences" (2001, pp. 72–80). I have discussed three strategies for determining the sample size, keeping the standard error small, making the power as large as possible and taking care of the cell frequencies.

But all this has no real bearing on causality, but rather on elementary rules of science. For—in spite of Ragin—each empirical scientific investigation requires a minimum number of observations in order to come to conclusions in a valid and reliable way. When the numbers are too small one will be "statistically" punished, because the confidence intervals of the calculated quantities will be so large that the possibility of statistical generalization will be endangered. Authors KKV of book DSI are imbued with this idea and consider a minimum number of observations as a conditio sine qua non for causal research. The case $n = 1$ is for them fundamentally forbidden. By that they react against Eckstein (1975) who breaks a lance for a singular "crucial" case. For example, when a very unlikely observation is at stake, a case which can hardly be reconciled with theoretical expectations, but which still stands the test, then one would have a stronger case in making conclusions. According to authors KKV such a case $n = 1$ does not stand up to scrutiny, because there is always more than just one variable in research and a fortiori more than one observation will be necessary, and also because of the matter of measurement errors and because our statements in scientific research are not deterministic but probabilistic in nature. Even so, authors KKV still try to consider fully the possibility of small numbers, or even of $n = 1$. Indeed, a single observation actually will make sense when it is part of a research programme, so that it can be combined with other singular observations of other researchers. In their view even the case $n = 0$ can make sense. An example is the investigation of a nuclear war between two nuclear superpowers, which has never occurred. In reflecting upon threats with nuclear war and in considering the frequency and seriousness of threats between countries with and without nuclear weapons, one can still carry out observations and test certain implications of the theory. In this way it is always possible, in the case of small n (or $n = 1$), to introduce extensions or changes in the formulation of the problem which can lead to meaningful research. A self-evident example is of course the making of observations with new units, at other places and at other moments in time, which means that n becomes larger. But it is also possible to observe new things with the same units (or unit). An example is that certain theories about the extinction of dinosaurs—a unique pre-historical event—have implications for the chemical composition

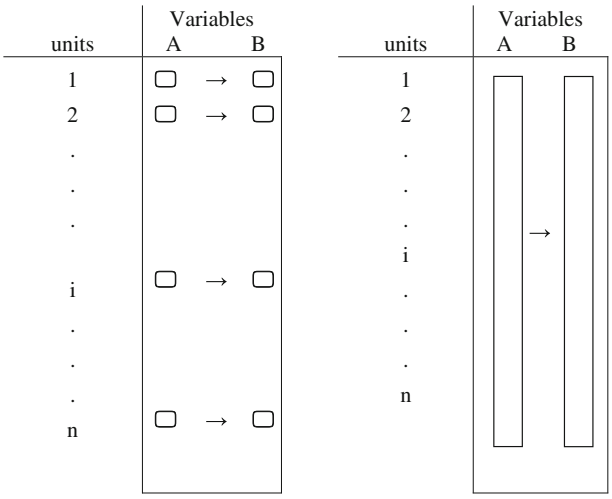
of rocks, which can be observed. Another example is the investigation of the effect of price fluctuations on social unrest, in which on can ask oneself what the implications are for the behaviour of companies or of agricultural cooperatives or of individuals, after which one can make new observations within the same investigation with the same n. Another example would be that one makes in one country extra observations with subunits, provinces, districts or municipalities.

So far for the discussion among contemporary authors. The daring thesis that a modest number of observations is sufficient to acquire evidence for a causal connection, was already brought to the fore in 1974 by John Makcie. He gives the simple example of a little piece of litmus paper which is plunged in a liquid and becomes red. He admits that in practice we will not restrict ourselves to one observation. We will repeat the test, for it might be possible that in the first attempt other relevant changes had occurred which we had not noticed. When making repeated attempts the presence of other relevant changes will be less likely. This repetition refers to the sufficient condition component, which we see in S of INUS and which we have already encountered in Hume's regularity theory. But Mackie explains clearly that not the repetition as such gives support to the conclusion that the plunging in liquid is the cause of becoming red. What is really at stake is the sequence 'plunging → becoming red' which is in itself prima facie causal in nature, in each individual case. The experimental observation or the before-after-observation thus reveal a causal sequence and not a sequence just like that. We do not discover that litmus paper is plunged in liquid *and then* becomes red, but we do discover that the plunging of litmus paper *makes* it become red. It should be noticed, though, that Mackie does not mean here that we can directly observe the middle term (the causal arrow →). His conclusion that the plunging in liquid *makes* the litmus paper become red must not be understood as a direct impression of causal sequence. We derive from the observation that the plunging is a good "candidate"-cause, but it is solely an INUS-condition, for, all kinds of assumptions and background-conditions remain unspecified.

But, whether a direct observation or a tentative formulation of a hypothesis tested by observations is at stake, in any case we notice here that Mackie already offers a modest initial impetus to causal realism, which has expanded enormously after him and in which the causal mechanism is at the centre. We have also seen such an impetus in the work of Leo Apostel, in the same year 1974. In the very same period we have also seen this in the *catastrophy theory* of René Thom, a mathematical theory in which abrupt changes of a dependent variable are induced by an infinitesimally small change of an independent variable. Such changes, named catastrophies, are conceived by the theory as a transition from one force of attraction to the other as the result of a small change in a background variable or of a small stochastic fluctuation (stochastic noise). Such catastrophic jumps occur when a conflict of forces of attractions is arising. One can for instance think of the British ferryboat Herald of Free Enterprise which capsized in March 1987 off the Belgian coast. Parijs (1978, pp. 195–220) discusses application possibilities for the social sciences. An obvious example is the theory of historical materialism, an asymmetric causal theory according to which production forces (labour force and means of production) enter into contradiction with the existing production relations (social relations which determine in which way labour force and means of production are combined), so that the latter are replaced by other production relations which correspond to the new state of development. This economic infrastructure of production relations for its part determines the entirety of superstructures: juridical, political, religious, family-oriented and others. The adaptation of production relations to the level of development of production forces runs by leaps. Now, these discontinuities are diachronic, for they are changes in time and they refer to one separate unit, one particular system, a

territorium, a nation, an economy. In other words, catastrophy theory is an historical, not a sociological theory. It makes singular causal statements, for instance for óne particular state, not general causal statements for a large group of states. And for óne separate state the causal mechanism is indicated.

Bearing the concrete research practice in mind, this has the following consequence. When A is the cause and B the effect, then one will not—such as in statistical causal models relying on analysis of regression—observe A and B first and thereafter indicate the causal arrow, but one will focus on the causal mechanism A → B. When more than óne unit is investigated, for example several nations, then the statement will not be "A and B are with a probability of x% causally related", but rather "A is the cause of B in x% of the cases". The difference between both research strategies (see Tacq 1984) can be graphically represented as follows:



A is cause of B in x% of the invidual cases (n causal case-studies)

A is with a probability of x% cause of B (n observations of characteristics A and B)

We see here that a totally different research strategy is chosen when the causal relationship is conceived as individual connection, which represents a causal mechanism. Instead of making n observations of the characteristics A and B and postulating a causal order, one now makes n causal case-studies, so that the judgement about the causal order is already made in the observation phase. Something similar was done by Braam (1973) in his study of the influence of companies on government. In a sample of shipbuilding yards and other companies which were related to water transport he tried to analyse to what extent attempts of influence of small and big companies and of individual companies and coalitions had an effect. For each company and for each coalition separately, for which a problem and an attempt of influence was observed, he verified whether a favourable decision of the government had followed. It is clear that his strategy consisted of reconstructing actual processes of individual decision chains. As Braam himself mentioned, such a procedure can sometimes make the observation phase very labour-intensive. But in my view that is the price one has to pay for carrying out sound causal research.

8 Summarizing conclusion

In this contribution the thesis has been developed that there is no principal difference between causality in qualitative and quantitative research, because both are being supported by the same fundamental logic, which was indicated as "experimental logic".

We started with an overview of the most important theories of causality by means of a walk through history, beginning with Aristotle, who is the spiritual father of causality. This historical survey developed into two prominent views, i.e., on the one hand INUS-causation of John Mackie and on the other hand causal realism of Harré and Madden and in their footsteps Roy Bhaskar.

After that we explained the contradistinction between quantitative and qualitative research. As it is not always simple to draw a clear line of demarcation, we first gave a historical sketch of the received views, first of French positivism and British empiricism, with August Comte and John Stuart Mill, respectively, as their most important representatives, and next of German nineteenth century neo-kantianism (Windelband and Rickert) and neo-hegelianism (Dilthey), which resulted ultimately in the point of view of Max Weber. Separate attention was also given to the Polish sociologist Florian Znaniecki because of the original character of his 'analytic induction' as a method of causal investigation.

With these movements of ideas from history in mind we developed the thesis that reflections of causality in quantitative and qualitative research are based upon the same "experimental logic". This was elaborated in various ways. First, the "counterfactual conditional" of John Mackie's INUS-model was compared with John Stuart Mill's "method of difference", which underlies the experimental design.

The very same logic was also retrieved in case-studies, which are classified as qualitative, as well as in stratified random sampling from large-scale quantitative surveys. On the basis of the work of Leo Apostel and his analysis of the notion of "production" we have put things in a broader philosophical framework. Apostel starts from the basis scheme R–O (Relationship–Order) and interprets a causal factor as a privileged element of a context. Scientific research of this context helps us to make a reasonable case for "production". John Mackie's INUS-analysis is a special case. Other examples of modest beginning of such examination of the context are randomization and the introduction of a control group in an experimental design. In qualitative research too, Wilhelm Dilthey's hermeneutics and Florian Znaniecki's analytic induction are examples of Apostel's R–O realization. It is important to recognize that the context of the privileged factor contains also non-realized and otherwise- and elsewhere-realized possibilities and conditionalities, an idea that can also be found in causal realism of Harré and Madden, and Bhaskar. This was clearly illustrated in the research example on racial prejudice in New York.

After having elaborated the thesis that there is no difference "in principle" between causality in qualitative and quantitative research, for the movements of ideas from the historical framework, we also entered into the recent literature, predominantly from political science: 'mixed methods research', the book DSI of the authors KKV, the reactions of David Collier and finally Boolean analysis of Charles Ragin. Here too, we confirmed the same thesis of unity in diversity, because all new proposals continue to build on the aforementioned themes, albeit in another terminology.

To conclude we discussed a subject over which there is a big to-do in the literature: small n and the case $n = 1$. Two extremes were placed opposite one another, on the one hand the large-scale survey in which scores of characteristics are placed in a data matrix and in which one is in the dark with regard to causality, and on the other hand the

labour-intensive causal case-study, case per case. The latter is difficult to execute and requires a huge labour-intensive observation, albeit that it is really preferable and is in line with the most prominent theories of causality, i.e., INUS-causation, in which the individual sequence stands in the centre, and causal realism, in which the main emphasis is on the causal mechanism.

References

Apostel, L.: Matière et forme. Introduction à une épistémologie réaliste. Communication and Cognition, Gent (1974)

Aristotle: Metaphysica. Het Wereldvenster, Baarn (1977)

Bhaskar, R.: A Realist Theory of Science. Harvester Wheatsheaf, New York (1978)

Bhaskar, R.: The Possibility of Naturalism. A Philosophical Critique of the Contemporary Human Sciences. The Harvester Press, Brighton (1979)

Blalock, H.M.: Causal Models in the Social Sciences. MacMillan, London (1971)

Braam, G.P.A.: Invloed van bedrijven op de overheid, een empirische studie over de verdeling van maatschappelijke invloed. Boom, Meppel (1973)

Brady, H.E., Collier, D.: Rethinking Social Inquiry: Diverse Tools, Shared Standards. Rowman & Littlefield, Berkeley (2004)

Bunge, M.: Causality. The Place of the Causal Principle in Modern Science. Harvard University Press, Cambridge (1959)

Cartwright, N.: Nature's Capacities and their Measurement. Oxford University Press, Oxford (1989)

Collier, D., Mahoney, J.: Insights and pitfalls: selection bias in qualitative research. World Polit. **49**(1), 56–91 (1996)

Comte, A.: Cours de philosophie positive. Librairie Larousse, Paris (1830–1842)

Creswell, J.W.: Research Design. Qualitative, Quantitative, and Mixed Methods Approaches. Sage, Thousand Oaks (2003)

Denzin, N.K., Lincoln, Y.S.: Handbook of Qualitative Research. Sage, Thousand Oaks (1994)

Deutsch, M., Collins, M.E.: Interracial Housing. University of Minnesota Press, Minneapolis (1951)

Eckstein, H.: Case study and theory in political science. In: Greenstein, F.I., N.W., Polsby (eds.) Handbook of Political Science. Addison-Wesley, Reading, MA (1975)

Elster, J.: Nuts and Bolts for the Social Sciences. Cambridge University Press, Cambridge (1989a)

Elster, J.: The Cement of Society. A Study of Social Order. Cambridge University Press, Cambridge (1989b)

Glaser, B.G., Strauss, A.L.: De ontwikkeling van gefundeerde theorie (The Discovery of Grounded Theory). Samson, Alphen a/d Rijn (1976)

Goldthorpe, J.H.: On Sociology. Narratives and the Integration of Research and Theory. Oxford University Press, Oxford (2000)

Harré, R., Madden, E.H.: Causal Powers. A Theory of Natural Necessity. Basil Blackwell, Oxford (1975)

Hempel, C.G.: Aspects of Scientific Explanation and other Essays in the Philosophy of Science. Collier-MacMillan, London (1965)

Hume, D.: A Treatise of Human Nature. Clarendon Press, Oxford (1739)

Hume, D.: Het menselijk inzicht (Enquiries Concerning Human Understanding), vert. J. Kuin. Boom, Meppel 1978 (1748)

Hume, D.: Philosophical Essays Concerning Human Understanding (1786) (London, A. Millar, 1748) revised edition (London, M. Cooper, 1751); republished as An Enquiry Concerning Human Understanding in Essays and Treatises on Several Subjects (London, A. Millar, Edinburgh, A. Kincaid & A. Donaldson, 1758); republished in 1786

Jöreskog, K.G.: A general method for estimating a linear structural equation system. In: Goldberger, A.S., Duncan, O.D. Structural Equation Models in the Social Sciences, pp. 85–112. Seminar Press, London (1973)

Kant, I.: Kritik der reinen Vernunft. Felix Meiner, Hamburg (1781)

King, G., Keohane, R.O., Verba, S.: Designing Social Inquiry. Scientific Inference in Qualitative Research. Princeton University Press, Princeton (1994)

Kuhn, Th.: The Structure of Scientific Revolutions. University of Chicago Press, Chicago (1962)

Mackie, J.: The Cement of the Universe. A Study of Causation. Oxford University Press, Oxford (1974)

Mill, J.S.: A System of Logic, Ratiocinative and Inductive. Routledge & Kegan Paul, London (1872)

Popper, K.: The Logic of Scientific Discovery. Hutchinson, London (1959)

Popper, K.: Objective Knowledge. A Realist View of Logic, Physics and History. Clarendon Press, Oxford (1966)
Ragin, C.C.: The Comparative Method. Moving Beyond Qualitative and Quantitative Strategies. University of California Press, Berkeley (1987)
Salmon, W.C.: Scientific Explanation and the Causal Structure of the World. Princeton University Press, Princeton (1984)
Scriven, M.: Explanation and prediction in evolutionary theory. Science **130**, 477–482 (1959)
Suppes, P.: A Probabilistic Theory of Causality. North Holland Publishing Company, Amsterdam (1970)
Tacq, J.J.A: Causaliteit in Sociologisch Onderzoek. Een Beoordeling van Causale Analysetechnieken in het Licht van Wijsgerige Opvattingen over Causaliteit. Van Loghum Slaterus, Deventer (1984)
Tacq, J.J.A.: Het methodologisch atelier. Adviezen en beschouwingen voor de sociale wetenschappen. Acco, Leuven (2001)
Tashakkori, A., Teddlie, C.: Handbook of Mixed Methods in Social & Behavioral Research. Sage, Thousand Oaks (2003)
Thomas, W.I., Znaniecki, F.: The Polish Peasant in Europe and America. Gorham Press, Boston (1918–1920)
van den Braembussche, A.: Theorie van de maatschappijgeschiedenis. Ambo, Baarn (1985)
van Parijs, Ph.: Théorie des catastrophes et materialisme historique. Revue française de sociologie **19**, 195–220 (1978)
Wittgenstein, L.: Tractatus Logico-Philosophicus, edited by D.F Pears. Routledge, London (1981)
Wittgenstein, L.: Philosophical Investigations (tr. G.E.M. Anscombe). Prentice Hall, New York (1999)
Wright, S.: The method of path coefficients. Ann. Math. Stat. **5**, 161–215 (1934)
Znaniecki, F.: The Method of Sociology. Octagon, New York (1934), reprinted in 1962