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WSTĘP/EDITOR'S INTRODUCTION

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Experimental Social Science

Abstract

My editorial introduction to *Selected Topics in Experimental Social Science* – the collection of papers which is the bulk of this issue of *Studia Sociologica* – grew out of the address I delivered at the opening session of the International Symposium on Experimental Research in the Social Sciences, Cracow, June 12–13, 2015. I extended my introductory presentation so much that it turned into a full-size article combining meta-theoretical reflections on theory and experiment in empirical sciences with information on laboratory experiments which were done by the Chair of Research on Group Processes from 1989 until the untimely death of Professor Jacek Szmatka (1950–2001), the founder of that research centre which no longer exists at the Jagiellonian University.

Key words: experiment, methodology of empirical sciences, three generations of sociological theories, network interaction system

Observation and experiment

In his worldwide used handbook, *The Practice of Social Research,* Earl Babbie placed the chapter on experimental method in Part Three which deals with 'modes of observation.' The chapter begins with the statement: 'At base, experiments involve (1) taking action and (2) observing the consequences of that action.' (Babbie 2014, p. 221). 'An experiment differs from other types of scientific investigation in that rather than searching for naturally occurring situations, the experimenter *creates* the conditions necessary for observation.' (Aronson et al. 1990, p. 11). In fact, while any *empirical science* rests on the *observation* of *regularities, experimental science* combines observation with a planned *intervention* in the *natural* course of events. Sometimes such an intervention is possible, sometimes it is not. One could drop balls, as did Galileo, from the Leaning Tower of Pisa, but even today no one is able to remove one planet from the Solar System to observe the System's behaviour after such an intervention.

In *social science* the range of actions you can take to observe their consequences is fairly wide, even though it is additionally limited by ethical concerns. But what do we learn more from experimenting than from observation alone, or why do we need to do experiments? (Webster, Sell 2014). Let us illustrate the problem with probably the oldest social science experiment described by Herodotus in 5th century B.C. and commented by Antoni Sułek (1989). The starting point for the experimenter, the pharaoh Psammetichus of Egypt, was the fact familiar to everyone: almost all people learn their first (ethnic) language from their biological parents. Observation certainly confirms this *statistical regularity.* Observation would probably suffice to provide enough evidence supporting the *general law* which states that *every* new-born child is able to learn *any* human language through communication with *any* speaker of this language. We also know from observation that communication between a child and the person who uses his or her ethnic language to address the child during daily contacts is in its early phase highly asymmetric with respect to speaking-to-listening proportion and initiating sequences of utterances. Therefore, it comes as no surprise to expect that the less active interaction partner will acquire the language of the more active one. Nevertheless, a question arises to which an answer cannot be arrived at without breaking the natural order. The question is: What will happen if the caretaker does *all* what must be done to meet vital needs of a new member of the human race (including establishing social bonds with him or her on the *preverbal* level) with the only *difference* that consists in refraining from speaking to the baby but waiting instead for it to start verbal communication.

Psammetichus *assumed* that any new-born would start speaking *a* human language at some stage of its normal development. With this (theoretical?) assumption he could seek an answer to the question of *which* language the child would speak, which kind of *exploratory research* is stronger than a mere attempt to learn *what* will happen when the natural process is blocked. Actually, the pharaoh's ambition was to carry out a true experiment aimed at *testing a hypothesis*. His hypothesis, which was not derived from any *general theory*, was very *specific* as it claimed that *everyone* would speak Egyptian provided that the ability to speak that language was not overridden by forced reception of a stream of words in another language. Sułek (1989, p. 650) praised Psammetichus for acknowledging – which is by no means a rule for the rulers – the negative result of his test. The first utterance of the *experimental subject* was recognized by the *experimenter's confederate* as the name of bread in the Phrygian language.

The story on the pharaoh-experimenter illustrates the advantage of experiment over passive observation. We learned from that ancient study and its subsequent *replications* by other rulers (Sułek 1989, p. 647) that the assumption of innate ability to speak a *concrete* human language was wrong, which of course does not invalidate a general *paradigm* that tells us to look for *innate* sources or determinants of social behaviour.[1]

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[1] Hamlin, Wynn, and Bloom (2007) have shown that preverbal infants can recognize and distinguish between three abstract types of *social actions* possible for an actor *A* who sees an actor *B* trying to achieve a goal (*A* can *help B*, or *hinder B*, or *stay neutral*). Moreover, the authors infer from certain nonverbal responses that 'infants prefer an individual who helps another to one who hinders another, prefer a helping individual to a neutral individual, and prefer a neutral individual to a hindering individual.' They claim that the evidence they

Definitions of experiment point to other important characteristics of this type of investigation. Every time I teach 'Methods of social research' to sociology students I quote at the very beginning of my lecture on experimental method a definition which comes from the book by Antoni Sułek (1979, p. 15). In English translation his definition reads as follows (italics mine).

> An experiment is a *repeatable* procedure that consists in a *planned change* of some factors in a situation under investigation and simultaneous *control* of other factors, a procedure that is performed in order to learn from *observation* the answer to the question of what are the *consequences* of that change.

The term *control* is often referred to the *power* over the *whole* setting or situation under study, including the ability to change values of some *variables* (*factors*) which work in the situation. The definition I quoted makes a distinction between *experimental manipulation* ('planned change') and proper *experimental control* ('control of other factors'). *Manipulation* means the ability to endow *units of analysis* (usually individuals or groups) with values of *independent variables.* The experimenter's power consists in that it is up to him which value from a specified range is assigned to any unit of analysis. Values are created by performing certain operations (*experimental treatments*) on the units or placing every unit in one of few *experimental conditions.*

In a narrower sense, *experimental control* reduces to eliminating possible effects on the dependent variable of variables other than the independent variables. This purpose can be achieved in a few ways[2] of which *randomization*, or *random assignment* of units to conditions, has the widest applicability; in addition, it enables controlling variables unknown to the experimenter.

The third component of Sułek's definition is hidden behind the words 'observation' and 'consequences.' It is the *measurement of the dependent variable*, an operation that is done *after* manipulation to see the effect of 'planned change' of values of the independent variable/s on the dependent variable. All three components were explicitly named by Jerzy Brzeziński (1996, p. 286) in his definition of 'experimental model of testing hypotheses on the dependence between the dependent variable/s and independent variable/s.' In his handbook of research methods in psychology, the 'experimental model' appears as one of three 'models of *testing hypotheses*'; the other two are *multiple regression* model and *ex post facto* model; the meaning of the latter is similar to that of *correlational study* (Aronson et al. 1990, p. 28–31), which

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gathered supports the hypothesis that some elementary moral evaluations are innate rather than learned.

[2] Some variables can be controlled by disabling their action in an *artificial* environment. For example, the use of a computer network in an interaction setting instead of face-to-face contact eliminates many variables characterizing communication partners. You can even hide from them their gender if they are forced to communicate with each other by means of a special code instead of a natural language (many ethnic languages allow their users to recognize the gender of one's communication partner from the grammatical forms he or she uses). Other ways of experimental control are: *holding variables constant* (Aronson et al. 1990, p. 18–20) and *matching* (p. 148–150).

is in fact a kind of observation as the values of *all* independent variables are only *registered* by the researcher, whereas in any experiment at least one independent variable must be *manipulated*.

Testing hypotheses – as a more ambitious epistemic goal than a mere *description* of some regularity – is a no less important property of experimental research than the investigator's ability to *construct* a completely or partially artificial, relatively isolated setting, and to trigger, control, and measure certain processes that occur therein. Under such a broad understanding (more general than the one implicit in the aforementioned definitions) of experimental method, the diversity of *experimental designs* stems from various ways in which testable hypotheses are being formulated in the *empirical sciences*.

A hypothesis may have the form of a *prediction* that a definite *phenomenon* will occur whenever certain conditions are met.[3] Another simple experimental design consists in measuring the values of the same variable Y (e.g., body temperature or blood pressure) twice for the same set of units of analysis (e.g., a group of students) where the first measurement is taken *before* and the second *after* performing an operation on these units (e.g. having the students to read an exciting story). To rule out alternative explanations of the *change* (predicted by the hypothesis to be tested) in the average level of Y, a *control group* is often needed besides the *experimental group*. In the control group, Y is also measured twice but between two measurements no action is taken that could change the state of the experimental system. The two groups are formed at the very beginning of the experiment by dividing a *random sample* taken from a *population* (the one for which the hypothesis to be tested is expected to hold true) into two subsets by means of a chance mechanism (say, flipping a coin that guarantees that every unit is equally likely to become a member of either group). The *classic design* thus obtained can in some cases be simplified by skipping the first measurement in both groups. The difference between 'action' and 'no action' can be interpreted in turn as a difference in the values of a 2-valued variable X, which then becomes an *independent variable* in relation to the *dependent variable* Y.

Many experimental designs involve a comparison of *mean values* of the dependent variable across conditions. A *comparison of two means* is also at the core of the first social science experiment which was run in the 1880s by Maximilien Ringelmann, French agricultural engineer.[4] In the decade which saw inventing the first automobile, he embarked on an examination of pulling efficiency of horse teams and discovered that the mean force of a team (the mean is obtained by dividing the overall force of a team by the number of its members), was always lower than the mean computed from the values obtained separately for each team member.

[3] For example, when a group of persons are given an opportunity to report aloud their assessments of a stimulus which they all are exposed to, then there arises – from individual judgements via the interaction process – a group norm which subsequently affects individual perceptions (see the description of Sherif's experiment in Cartwright, Zander 1960, p. 23–25).

[4] His report was published in 1913 to the effect that the birth of experimental *social* psychology is usually traced back to Triplett's study (1898) which gave rise to the *together and apart* paradigm, the dominant paradigm in early experimental social science. See Brown 2000, Chapter 5.

Interestingly, Ringelmann employed students instead of horses. He told them to pull a rope tied to a dynamometer, first *apart* and next *together* in teams of varying size. He could *communicate* with his experimental *subjects* because they, like him, were *human beings.* Without their cooperation he would have been unable to carry out his research.

The necessity of communication between the experimenter and the people whose behaviour is to be studied may also entail some undesirable consequences that do not arise in *experimental natural science* where experimentation consists in the measurement of a number of variables in strictly controlled laboratory conditions. Research methods, such as testing hypotheses on *cause-effect relationships* between variables and designing experiments so as to compare groups with respect to the average level of dependent variable, prevail in the social and behavioural sciences. They are also commonly used in other empirical sciences along with more advanced ways of producing scientific knowledge. Physicists do not compare group means. They check if the readings from measurement instruments agree with the theoretically predicted values, which are *calculated* from the formulas expressing functional relationships between variables chosen to describe the current state of a physical system, such as a falling ball.

Such a 'hard' approach to *theory building* and *theory testing* has been recommended for use in the social sciences by Willer and Walker (2007). Drawing on an earlier book by Willer (1987), they distinguish *theory-driven* experiments from *empirically driven* experiments. According to them, these two types of experiments essentially differ on the level of the very logic of scientific investigation. Shane Thye (2014, p. 74–76) denies the radical nature of the opposition, noticing that both types of experiments face similar problems to cope with such as threats to *internal validity* from *confounding factors*. Although I share his view, it is not my intention to belittle the importance of the distinction made by Willer and Walker. It reflects the dissimilarity in a few respects of two ways of sociological theorizing described by Szmatka and Sozański (1994). I return to this topic later in this paper after delineating in the next section a broader meta-theoretical context in which the purport of experiment as a method devised for testing hypotheses in any *basic empirical science* can be properly understood.

Basic characteristics of the basic sciences[5]

Every basic science, no matter whether formal or empirical, natural or social, 'is oriented to the production and evaluation of knowledge claims' where the term knowledge claim is referred to any statement which 'can be accepted or rejected on the basis of some criterion of truth.' (Cohen 1989, p. 52–53). Methodology of the basic sciences formulates epistemic criteria for evaluating solutions to scientific

[5] In this and the following sections I use re-edited excerpts from Chapter 1 ('Structural Mathematical Sociology') of my still unfinished book (*The Mathematics of Exchange Networks*). The full text of Chapter 1 is available on my personal website (http://www.cyfronet.krakow.pl/~ussozans/chap1.pdf).

problems. While the form of the problems considered tractable in any particular science may also be subject to meta-theoretical analysis, the range of these problems and their substantive content is always determined by one or more paradigms, a *paradigm* being defined roughly as a set of guidelines, accepted by the academic community, as to what and how can be studied in a given discipline or subdiscipline.

What are the distinguishing features of *science* as a special kind of *knowledge*? The answer is that scientific knowledge already is or should be developed so as to be:

> (1) *intersubjectively communicable*; (2) *methodically* produced and validated; 3) *systematized*; (4) *consistent*; (5) *logically provable* or *empirically testable*; (6) as *certain* as possible; (7) rich in *information*; (8) *universal*; (9) *general*; (10) *precise* and *accurate*; (11) *parsimonious* and *simple*; (12) *abstract*; (13) *conditional;* (14) *cumulative*.

Most of these characteristics are also included in Markovsky's (1997) list of the criteria for evaluating scientific *theories*. *Intersubjective communicability* is achieved in each discipline through *codifying* its language, or establishing clear, workable criteria upon which meaningful statements can be distinguished from those recognized as meaningless. Codification of *scientific discourse* inevitably leads to supplanting natural language by artificial *formal languages* in which complex expressions are built from simpler ones by applying to them certain explicitly stated *rules* so that meaningful statements are recognized from their syntactic structure. Formalization of the *syntax* (relations within a system of signs) is a necessary step preceding the codification of two other aspects (distinguished by Morris in his *Foundations of the Theory of Signs*, 1938) of any language (more generally, any *semiotic system*), *semantics* (relations between language expressions and the objects in the 'world' to which they refer) and *pragmatics* (relations between a language and its users).

Scientific knowledge should be produced *methodically*, even if it ultimately grows out of unplanned discoveries of new facts or new conceptual representations of known facts. *Methods* are prescriptions on how to perform various activities at every stage of the research process, primarily at its last and most important stage when knowledge claims are validated upon 'some criteria of truth.' In the *formal sciences*, a knowledge claim is *accepted* if and only if it can be deduced from already accepted claims by means of logical *rules of inference*. The *deductive method* is also used in *empirical sciences* along with *empirical testing* (in particular, *experimental method*), a way of validating knowledge claims which is peculiar to these sciences. By requiring scientific knowledge to be produced methodically, we also mean that the *evidence* needed to test a hypothesis must be collected with the use of intersubjectively controllable *data generation procedures*.

Science also differs from common-sense knowledge in the degree of *systematization*. This requirement pertains both to terms and propositions, two basic components of any knowledge. *Terms* are names of things, properties, relations, functions, and other constructs studied in a given field. *Propositions* (sentences), as formed with the use of terms, constitute the higher level of the language. What is even more important, they are conceived of as statements which can be *true* or *false* in a given *domain* in which the terms occurring in them are *semantically interpreted*.

Collections of terms and propositions should be structured so as to form *terminologies* and *theories*.

Contradictory hypotheses may coexist in science, yet among the propositions that are accepted in a given discipline there should never be two sentences such that one of them is the negation of the other. *Consistency*, defined by this requirement, is the most fundamental condition any jointly accepted collection of knowledge claims must satisfy. In particular, every scientific theory should be consistent.

We require scientific knowledge to be *intersubjectively provable* or *testable*, but we have to acknowledge the fact that all proofs and tests are *relative*. In the *formal sciences*, a hypothesis is accepted as a *theorem* if there exists its demonstration based on explicit specific axioms whose consistency is usually justified by invoking a more fundamental theory. The *empirical sciences* use the *deductive method* too – as a way to derive consequences from already accepted theoretical propositions and as part of testing procedures.

To *test* an empirical theory, one must first identify a number of situations that meet the theory's *scope conditions* and admit of gathering *evidence* indispensable for validating theoretical predictions. The scope conditions (see Cohen 1989, p. 83; Foschi 1997) determine the range of systems to which the theory applies; they can also specify special system states or some additional circumstances in which theoretically predicted events should occur. Since empirical systems that meet all scope conditions are seldom found in nature, one cannot do without constructing fully or partially artificial systems. Created by the researcher, they are easier to study than natural systems but are no less real than the latter.

For any *empirical system* that meets a theory's scope conditions, one must state some more or less specific *hypotheses* concerning its predicted 'behaviour.' Hypotheses should be derived then from the theory, supplemented, if necessary, with auxiliary assumptions which may point out operational counterparts of *theoretical variables.* To test theory-based predictions and thus the theory itself, one must *observe* and register actual behaviour of the system under study; observation usually amounts to measuring values of some variables. If the observed behaviour of the system agrees with the predicted behaviour *within the margin of error*, then the theory is said to have been corroborated by the evidence generated to test it.

If observation of a 'natural' course of events cannot provide sufficiently rich and unambiguous evidence, one has to create an artificial setting in order to give nature an opportunity to speak in a more extensive or more articulate way. In either case, the researcher must devise a procedure to *generate evidence* interpretable in the context of his or her theory, or a procedure for translating the cues emitted by the external world into meaningful *data*. In an ideal world, such a procedure would be dictated by the theory alone. In the real world, it should be designed so as to minimize 'error' or 'noise' occurring also in experimental systems as they are made from the material taken from the real world and are never completely protected against the influence of the external environment.

Given an adequate research design and reliable measurement techniques, the outcome of a test should depend on whether the theory undergoing verification correctly depicts regularities operating within a well defined category of things or events. An empirical theory must be supported by the evidence in a number of tests to get incorporated into the body of established knowledge in a given discipline. Theoretical propositions which have been accepted and have few other desirable properties (universality and generality being considered most important) are called *laws*. Once accepted, an empirical law can be applied outside the setting in which its predictive power has been confirmed. Although our confidence in a law grows with each successful application thereof, the *certainty* characterizing mathematical knowledge can never be attained in empirical sciences. While a mathematical theorem, once correctly demonstrated, will be accepted forever, laws in empirical sciences are vulnerable to refutation. However, an empirical law need not be automatically discredited if negative results of further tests raise doubts about its validity. If some observations depart from the predictions deduced from a well established theory, the first suspicion is that the theory has been incorrectly applied. Such an explanation is possible because scientific knowledge is necessarily *conditional* (Cohen 1980), that is, any scientific knowledge claim is applicable only if definite scope conditions are met.

The core laws of an empirical theory that are protected from hasty falsification are called *principles.* Their epistemic status is the most contentious issue in the philosophy of science. While for 'realists' principles render objective *regularities*, for 'conventionalists' – also called 'instrumentalists' – they are but *tools* invented to enable a selective, concise and coherent account of the data. Willer and Walker (2007, p. 59) ask 'What, then, does theoretic science assert about the regularity of the world?' and answer 'It claims that whether the world is regular cannot be judged independently of the theories through which the world is understood' (p. 59). Such an answer shows authors' sympathy for the instrumentalist meta-theoretical stance, however it is expressed less radically than in earlier statements (to be quoted later in this paper) by Willer himself (1987).

As Imre Lakatos noticed (1970), an empirical theory does not drop out of the corpus of accepted scientific knowledge because of being simply falsified. Once approved, a theory is abandoned only if it can be replaced by a new theory which accounts for all the facts explained by the old theory as well as for some facts that the latter cannot explain. It is the strongest meaning of the postulate that scientific knowledge should grow *cumulatively*.

Every *investigation*, scientific or judicial, theoretically or practically oriented, is aimed at reducing cognitive *uncertainty*, first of all, in any situation where hypothetical answers to a question are known, but one is not sure which of them is true. 'In a somewhat aphoristic form, science is an information-seeking process' (Szaniawski 1976, p. 297). In the light of formal *information theory*, richness of *information* and *certainty*, items (6) and (7) on our list of the goals pursued by science, turn out to be conceptually intertwined. However, their understanding must remain intuitive until an *intersubjective* practical method for measuring epistemic probability becomes available. In general, the pragmatic aspect of the language of science admits of limited codification, which opens the door for sociological interpretations of methodological rules as mere norms or conventions approved by academic communities.

Universality and *generality* are two qualities that distinguish *laws* from other accepted scientific propositions. The broader the scope of a theory, the more general the theory is regardless of the nature, abstract or historical, of entities it deals with. In logic, the term *general statement* is referred to any proposition stating that *all* things have a property *v*. The derivation of a particular conclusion from a general statement is probably the most familiar pattern of deductive reasoning ('All men are mortal, therefore I am mortal'). Generality is in fact a semantic concept because the phrase 'all things' acquires a definite meaning with pointing out a set *S* whose elements (or rather their names) are to be substituted for *s* in the proposition 'for all *s*, *v*(*s*).' *Laws* are usually construed as *strictly general* statements, which means that they should hold true in *domains* with infinitely many objects, or indefinitely many (however large is the set of all men who have ever lived or will live on earth, it is finite).

Universality should not be confused with generality. 'A universal statement is a statement whose truth is independent of time, space, or historical circumstance' (Cohen 1989, p. 78). To ascertain whether an empirical theory is universal, one must test it in at least two settings that differ with space-time or sociocultural coordinates. In the social sciences, 'the cross-national and cross-cultural replication experiment is the only method of testing a theory for universality' (Szmatka 1997, p. 95).

According to Cohen (1989, p. 178), universality and deductive systematization are both required of a collection of conceptually interrelated testable statements in order that it can be called an *empirical theory*. If universality is skipped as a too restrictive condition of theoreticity, it returns as the basis of the traditional distinction between *nomothetic* and *idiographic* (*historical*) sciences, the former being defined as those capable of producing universal theories. The scope conditions of a universal theory do not state *when and where* in the *real world* to find systems to which the theory applies. Nevertheless, one must show that such systems do exist because otherwise the theory would not be testable. An empirical theory need not claim universality. In order to be testable, it must also have a definite scope that is specified by indicating the time, place, nation or culture where the theoretically predicted regularities should occur.

Attempts to generalize a theory as much as possible and make it universal may result in disregarding other, no less important, goals of science that are usually easier to achieve under more restrictive scope conditions. Generality and universality really count only if they go together with *precision* and *accuracy*, as is the case with Newton's laws of motion, which not only apply to a broad class of mechanical systems, but yield *specific, quantitative predictions* which agree remarkably well with measurement results. 'Although a theory may generate predictions that are highly precise, the *accuracy* of those predictions – their correspondence to empirical observations – may vary' (Markovsky 1997, p. 19). There exist sociological theories which offer exact predictions of the behaviour of some social systems, yet the gap between observed and predicted results is often too wide and contingent on uncontrollable events. Hence, the social sciences on the whole cannot yet be counted among *exact sciences*, or those nomothetic empirical sciences that meet the standards of precision and accuracy to a high degree.

In all empirical sciences, the quest for precision forces the transition from concepts to variables. To transform a *concept* into a *variable*, one must first select an appropriate *unit of analysis* (the concept may admit of more than one option in this matter). Next, the *domain* of the variable should be pointed out – as the set of objects varying in the respect considered important by the researcher. Lastly, there must be invented a way of assigning *values* (usually numerical) to the elements of the domain. Constructing variables and theory building always go together. While in well-developed sciences this takes the form of *fundamental measurement* based on laws relating theoretical quantities to each other, in the social sciences the prevailing approach is *measurement by fiat*, as Torgerson (1958, p. 21−25) called taking an *operationally* defined variable to represent a *latent* theoretical variable on the basis of '*presumed* relationships between observation and the concept of interest' (p. 22).

In formal *set-theoretic* terms, a *variable*[6] is a *mapping* of a set of objects under study into a set of numbers. In the empirical sciences, variables are used to formulate theoretical hypotheses and their directly testable consequences. What can be studied for a single variable is merely the *distribution* of its values assumed in a set of objects (the whole population or a *sample* taken from it). Given two or more variables, one wants to know how their values *co-vary* over the common domain. To construct a theory whose propositions have the form of interrelated 'covariance hypotheses' (Blalock 1969), one has to select a set of variables and decide which of them are to play the role of *independent variables* in relation to the remaining variables called *dependent*; it is a matter of theory to *predict* values of the latter from the known values of the former. If there are few independent variables, they are assumed to vary independently of one another, which in an experimental setting should be guaranteed by a proper *research design*. Even though theories in empirical sciences are often constructed so as to render *causal* linkages among variables, it is the concept of *dependence* (statistical or functional) rather than causality that is given a more technical meaning in theory and research.

Patterns of theorizing and experimenting in social science

Presuming that the context will steer the reader to the proper understanding of *science* and *social*, I have not yet explained what is meant in this paper by *social science*. The singular is used to highlight *methodological* unity of social sciences, as well as to leave aside the question of where to trace out the borders between social psychology, a predominantly experimental science, sociology, and economics. *Substantive* unity of 'social science' is founded on making interaction of members of the human species the most elementary object of investigation. Any social scientific study of the processes going on between two or more persons must take into account not only natural (physical or biological) aspects of these processes but also

[6] Variables – in this meaning – should not be confused with logical variables. The latter are symbols (in formal languages) or common nouns (in natural languages) that enable us to speak of things, points, numbers, or other entities without the necessity to point out concrete elements of appropriate sets.

the very fact that people communicate with each other with the use of certain codes (systems of signs) which are part of *cultural reality* (Znaniecki) equivalent to the Popperian 'third world' (see more in the chapter mentioned in footnote 5).

'Science can be thought of as consisting of theory on the one hand and data (empirical evidence) on the other. The interplay between the two makes science a going concern' (Torgerson 1958, p. 2). The meaning of 'science' in the expression 'social *science*' is as *broad* as in the cited statement. The saying attributed to Rutherford – 'In science there is only physics or stamp collecting' – denigrates many empirical sciences, which, unlike physics, do not yet meet and some possibly will never meet all of the 14 requirements I listed at the beginning of the previous section (the last two of them I have not yet mentioned are (11) *parsimony* and *simplicity*, and (12) *abstractness*).

Contemporary mainstream *social theory* has gone far away from positivist and postpositivist (Popper, Lakatos, Toulmin) meta-theorizing on social science. What is labelled as *positivism* is criticized either for theory-free 'stamp collecting' or importing to the social sciences the patterns of doing theory that are believed to be unique to the 'natural sciences.' While some views traditionally associated with positivism, such as the idea of *theory-free sense data*, deserve rejecting outright for having little to do with real science, a few other postulates, also considered untenable by leading figures of contemporary social theory, are worthy of defence (Turner 1985). I mean, first of all, the principle of *demarcation* between *empirical* and *formal* sciences on the one hand, and *hermeneutic* or *philosophical* sciences on the other.[7] The demarcation principle does not remove from science the questions of existence. Kurt Lewin was right to claim that 'The taboo against believing in the existence of a social entity is probably most effectively broken by handling this entity experimentally.' (Cartwright, Zander 1960, p. 18).

Experimental testing empirical theories in exact sciences resembles demonstrating consistency of *formal theories* through *constructing* their *semantic models.* Similarly, the experimenter's task is to build an *empirical system* in which *observational statements* derived from the theory are true. Theoretical predictions, or *empirical consequences* of a *formalized empirical theory*, are *deduced* from the formal theory (the one which was used to formalize the empirical theory that is to be tested) and certain rules linking abstract objects and variables with their observable counterparts.

[7] The recent dispute in Poland over the prerogatives of the Constitution Court encourages non-lawyers to raise the problem of what epistemic status should be attributed to the *legal sciences*. As a sociologist and mathematician, I would like to know if the *science of law* is a formal science or an empirical science. If neither of the two is true, should *assessing consistency* of bills with the constitution be regarded as a task requiring *philosophical* competence? Do the experts in constitutional law who are making judgements in such matters resort to yet another kind of knowledge? When I found convincing, however on a purely *intuitive* basis, some arguments – presented by a few lawyers with academic degrees – in defence of the position of the government and ruling majority, I asked an eminent professor of sociology to let me know his position in the debate. He replied to my letter by sending me *solely* the list of outstanding professors of law who used their *scholarly authority* to back the parliamentary opposition and the chairman of Constitution Court. My curiosity about the nature of the legal sciences remains unsatisfied.

Physics has always been perceived as an embodiment of the *ideal type* of *exact* science. *Sociology*, first called by its father 'social physics' (Comte abandoned this name, having noticed that Quetélet used it to denote the study of *statistical* regularities) emerged from social philosophy to gradually achieve the status of a *normal* empirical science ('normal' in the sense proposed by Thomas Kuhn in *The Structure of Scientific Revolutions,*1962). The publication of *Le Suicide* (1897) played an essential role in that process. The research paradigm underlying Durkheim's landmark work does not envisage experimental testing theoretical hypotheses on the dependence between the phenomena abstractly defined in his theory. While *sociology* has remained until today an overwhelmingly non-experimental science, an experimental design is constitutive of many paradigms in *social psychology*, including the *together and apart* paradigm, which appeared in social science at more or less the same time (Triplett published his paper in 1898) as Durkheim's *sociological* theory relating the frequency of suicide acts to the level of social integration of a group. The experimental paradigm in question consists in *comparing individual* performance measured in the baseline situation in which the person is set to work alone with the performance observed in the situation where the task is being done by the person in the *presence* of another performing the same task simultaneously. Whereas the baseline situation is defined unambiguously, the 'social' ('co-action') situation, for being defined as mere presence of the other person doing the same, can be enriched with additional characteristics bearing on further theorizing inspired by *inventing* the paradigm. For instance, the experimenter may encourage the subject to compete with the co-actor as was in the case of original Triplett's experiment. Thus the paradigm leaves room for introducing into the experimental setting manipulable factors to learn the sufficient and necessary conditions for the effect of *social facilitation* (significantly better performance in the social situation) to occur. Willer and Walker (2007) point to the advantages of *theory-driven experimenting.* The idea of *experimentally driven theorizing* is no less promising and compatible with the practice of social research.

Robert Merton (1968) saw in Durkheim's suicide theory a classic example of a 'theory of the middle range.' He believed that theories of this kind would successfully challenge 'total systems of sociological theory' as he called *conceptual* images of the social world. Such a general conceptual framework may lead to formulating proper theories (a *theory* must contain interrelated propositions apart from concepts) explaining some phenomena. Szmatka and Sozański (1994, p. 225–231) called that product of old and new sociological theorizing – also known as 'grand theory' – *theories of the first generation*. These theories are *abstract* (they contain terms like 'social system') but suffer from the lack of testing procedures and explicitly stated scope conditions. Sociological theories that are free from these deficiencies form two other 'generations.' Since the latter word suggests the process of replacing old products with new ones, Szmatka and Lovaglia (1996) changed 'generation' to 'genus' to concede that all three kinds of theorizing co-exist in contemporary sociology and none of them is going to supersede others in the foreseeable future.

Theories of the second genus are expected to provide a systematic account of multidimensional differentiation that is actually observed in natural social settings and concrete populations where regularities usually occur in a blurred form due to complex and casual ties within the multitude of variables. If the main sources of variation and specific patterns of dependence cannot be identified prior to data collection – for the lack of a 'theoretical model' – one may try to construct a 'methodological model' (Skvoretz, Fararo 1998), or try to extract regularities directly from the data by means of standard procedures of multivariate statistical analysis. The choice of variables is then subordinated to the main goal defined as explaining the largest possible share of the total variance of each dependent variable. An experimental test of a theory of the second genus takes the form of *factorial experiment* classified by Willer and Walker as *empirically driven experiment*.

Theories of the third genus unlike the theories of the second genus are *abstract* and claim *universality*. They are constructed with the aim of bringing the social sciences closer to the *exact* natural sciences. While precision and accuracy are highly desirable properties, the focus is on parsimony and simplicity. The postulate of *parsimony* states that in science 'entities must not be multiplied beyond necessity' where 'entities' may be primary terms, axioms, laws, variables, etc. The postulate of *simplicity* means, in particular, preference for the use of simple functions or formulas to describe inter-variable relationships. The power of exact sciences lies in that generality and universality, parsimony and simplicity need not be sacrificed for the sake of precision and accuracy.

Each theory of the third genus describes the behaviour of a class of abstract or *ideal* systems by means of a *small* set of *theoretical* variables. Some of them, though not necessarily all, must have *observable counterparts* in *empirical replicas* of abstract systems. In regard to *natural sciences*, Toulmin (1953, p. 44–56) used much similar criteria to contrast 'physics' with 'natural history.' 'Natural historians' want to *explain* facts they observe in the world. To do this, they invoke 'general laws' of the form '*all* As are Bs.' 'But so long as one remains within natural history there is little scope for *explaining* anything: "Chi-chi is black because Chi-chi is a raven and all ravens are black" is hardly the kind of thing a scientist calls an explanation.' (Toulmin 1953, p. 49).

Lee Freese (1980, p. 191–192) presented a similar distinction between the 'generalizing view' and the 'instrumental view' of theories and laws.

> Laws ... are not meant to be generalizations about the world of everyday experience. The regularities they describe exist in a theoretically possible world but not in the actual world. … If theories are construed as describing some idealized state of affairs in a closed system ... then they [laws] are devices for calculating changes in the system when other things are equal. Though other things are never equal outside of the closed theoretical system … laws may serve as tools for engineering some change in an open empirical system whose departures from some theoretically true state of affairs can be measured.

The *instrumental* view of laws may appear incompatible with the realist stance in the philosophy of science. Actually, a law, which in its abstract form applies *directly* to a class of 'theoretically possible' systems, applies *indirectly* to relevant real-world systems. Its successful indirect application to an 'open system' should be possible due to universality. However, even in laboratory systems the impact of extraneous variables can be so strong that the law fails to provide accurate predictions. Szmatka and Sozański, referring to Willer statements (1987, p. 221), addressed this problem in the following passage (1994, p. 230–231).

> In a laboratory system, the experimenter can, to be sure, control the structural conditions of human actions but must always fill positions in the system with concrete individuals shaped in a particular sociocultural context. 'Why is it then that Galileo did not consider the colour of his shirt or the phase of the moon when he evaluated the results of his trajectory experiments?' (Willer 1987), and why do sociologists, in order to explain the behaviour of experimental subjects, sometimes need to consider such factors as personality or situation variables thought of to be 'at work' in a given setting? 'The answer does not lie in the difference between animate objects which we investigate and the inanimate objects which he investigated. Instead the answer lies in the evidently clean results of his experiments and in the fact that they could be reproduced by him or *by others* as needed.' (Willer 1987).

Why are *some* empirical sciences able to produce general and universal, precise and accurate, parsimonious and simple theories? Certainly, the ability to obtain 'evidently clean results' in repeated experiments depends to a high degree on the choice of a suitable *mathematical representation* and *research design.* According to Willer (1987, p. 220), what makes an exact science exact 'is the exact use of theory, not necessarily the exact production of clean results … the criterion should be that a better theory is one which *can* produce cleaner data, not that it would always do so.' However, a *precise* theory becomes practically useful insofar as it can provide *relatively accurate* predictions *relatively independently* of the context in which it is being applied every time. If very restrictive conditions need to be imposed in order to produce sufficiently 'clean' data, then the theory becomes useless outside the setting in which it has passed the experimental test, that is, outside the setting in which prediction accuracy has reached the level considered satisfactory in a given discipline. Hence, there is another methodological standard that *experimental exact science* must meet besides high precision and accuracy. The results of experimental tests should be *stable*, which means that a small a change of the setting in which a given regularity has been detected in its purest form should cause a relatively small decline in prediction accuracy.

Regularities in the social world

The 'criterion of truth' upon which scientific knowledge is validated is *coherence* of theory and evidence. However, once experimental evidence is *produced* by the researcher, one may be interested to know to what extent coherence, desired so much, depends on '*building* the experiment,' and to what extent it hinges upon the existence of some *regularity* or 'order' in the world out there. The passage quoted below (Willer 1987, p. 12–14) documents that Willer would like to dismiss the question but in the last resort he tends to attribute more creative power to the theorist-experimenter than to the world, thus subscribing to the viewpoint of instrumentalism.

> Within the process of scientific inference, no assumptions are made concerning the regularity or irregularity of the world. No such assumptions are needed because the relations among objects and events are first drawn in theory and only then compared point by point to bits of information from the world. … Does replication [of experiments] prove that the world is regular? No, for replication proves only that theory can so organize the world and our view of it that at least some parts of our perceptions can be made to appear regular – and that is quite another thing.

Willer is right to say that exact sciences do not start from the *assumption* that the world is regular. The *hypothesis* of regularity is arrived at through systematic *observation.* Ancient astronomers *did not assume* that celestial bodies behave regularly. They *discovered* that the position of these objects in the sky at any moment can be predicted with great accuracy. The discovery of a 'natural order' in some area of the social universe may result in formulating a theory of the second genus. For Jacek Szmatka it was no more than the first step. He believed that any scientist oriented toward 'hard science' should attempt to explain any regularity by offering a theory of the third genus, a testable, general, universal, precise theory that *abstracts from* particular occurrences of the regularity in concrete *empirical objects.* When we discussed the problem of how second and third genus theories are (or should be) related to each other, I argued that a move in the opposite direction, the transition from a given third genus theory to a second genus theory may appear necessary in some circumstances. When a theory of the third genus fails to provide accurate predictions, or, as Willer would say, when the data from a theory-driven experiment are not 'clean' enough, then one may try to 'improve' the theory – at the cost of 'spoiling' it in other dimensions (universality, parsimony) – by appending certain variables that do not fit the abstract theoretical model but make it possible to reduce unexplained variance. For example, a theory that is intended to predict outcomes of a game played by rational actors can be modified by adding actors' gender to the set of variables which are suspected to affect the decisions made by the players.

Some advocates of the *idealization strategy* claim (Wysieńska, Szmatka 2002) that testing third genus theories is conducted within the 'theory world' that transcends the concrete 'external, phenomenal reality.' Actually, *ideal* systems which serve as models of *empirical* systems are part of the mathematical world as they are *sets endowed with structures* (Bourbaki's term; see the chapter mentioned in footnote 5). *Laboratory replicas* of abstract systems do not differ in the stuff they are made of from empirical systems studied by the theories of the second genus. It is not true that 'the social laboratory, unlike the physical laboratory, may be cleanly separated from the phenomenal world outside' (Willer 1987, p. 214). Willer would be right if *live* subjects were replaced by computer programs, yet *simulating* a theory-predicted process is not equivalent to *testing* the theory. The 'theory world' can only be conceived as one of mathematical domains and set-theoretic constructs. Having entered this world, you can verify *logical consistency* of a formalized empirical theory, which, once *formulated*, has to be confronted with the data coming from the world we perceive with our senses and transform with our actions.

Theory and *evidence* should be conceived of as *two* distinct *independent* sources of information about *one* world of experience. These sources must not be attributed equal credibility as the 'voice' of the data should always count more. *Independence* does not mean that the data generation procedures must be 'theory-free.' Even a police *investigation* into a crime is not confined to gathering facts connected somehow with it. The investigation is 'driven' by the prosecutor's theory, which of course shall be modified as new facts are becoming known. In exact sciences, *relevant* experimental evidence is generated through fundamental or derived measurement. The instruments with which theoretical variables (or their empirical realizations) are measured are themselves constructed according to the prescriptions based on the theory being tested.

Physics and sociology differ in entities studied, variables chosen to describe them, paradigms, theories, and data-generation procedures. Do these sciences also differ in *general* patterns of theory testing? Let us compare a sociologist studying a task group in a laboratory with a physicist investigating the motion of a bullet. Both experimenters can trigger off some processes in empirical systems whose behaviour is going to be observed, yet the physicist cannot *tell* the bullet to move along the theoretically calculated curve, whereas the sociologist, owing to his ability to communicate with human agents, can make them familiar with his theory and induce them to behave accordingly. If we catch a sociologist talking experimental subjects into the behaviour predicted by his theory, should we blame him of violating a *methodological norm* or should we rather recognize his *communicative action* as a 'legal' way of testing a *sociological* theory?

If the only purpose of an experiment were to 'reproduce' the *form of a regularity*, then it would suffice to *simulate* theoretical behaviour in a 'virtual system' where 'virtual' does not mean 'imaginary' or 'mental.' A 'virtual system,' on the one hand entirely artificial, is 'real' as constructed within the real world with the use of technical devices. For example, a *virtual dyadic social system* can be composed of; (1) two interacting programs running on two networked computers; or (2) an individual interacting with a computer program or even a pair of persons – provided that *live* human agents, even though they act 'consciously,' have been 'programmed' by the experimenter to 'reproduce' a theoretically predicted regularity. Therefore, when we need to learn – what we cannot know in advance – if real actors actually behave as regularly as our theory claims, we have to carry out an experimental test on a system that is real rather than virtual, that is, a system whose behaviour is 'driven' by internal objective forces rather than by the theory to be tested, or, more exactly, by the experimenter armed with his theory and techniques.

The nature of *regularities* in the social world has intrigued old and new 'masters of sociological thought.' Anthony Giddens (1984, xix), a leading figure in contemporary theorizing of the first genus, equates regularities with 'generalizations,' thus agreeing in this respect with the positivist tradition that he criticizes for the neglect of human subjectivity and creativity.

> Some [generalizations] hold because actors themselves know them – in some guise – and apply them in the enactment of what they do … Other generalizations refer to circum-

> stances, or aspects of circumstances, of which agents are ignorant and which effectively 'act' on them … 'structural sociologists' tend to be interested in the generalizations in this second sense … But the first is just as fundamental to social science as the second.

Sociologists often explain regularities characterizing 'social practices' observed in certain typical situations by attributing to the people the *knowledge* of certain *rules.* Giddens believes that the knowledge of these rules prompts to the actors what they should do in these situations. He defines (1984, p. 21–22) 'rules of social life' as 'techniques or generalizable procedures applied in the enactment/reproduction of social practices.' Seen in this perspective, the soldiers' obedience to their commanders results from their knowledge of the rules that establish behavioural dependence between the occupants of inferior and superior positions in social systems of the kind called by Max Weber *Herrschaftsverband.*

Regularities of the first type consist in *enacting theories.* The 'laws' of such theories, even though they may be formulated by sociologists, do not essentially differ from 'social laws' of which the use by the actors makes the combinations of their actions predictable. However, if we use a 'theory' of which we know only that 'knowledgeable agents' accept it to explain the very fact that they 'enact' this theory, then we have to abandon even the humanistic (Weberian) conception of social *science*. Nevertheless, in many situations we must admit explanations of observed behavioural regularities in terms of 'reproducing' certain patterns (no matter whether regular behaviour was taught to the group or emerged as a result of a natural group process). It is debatable whether such an explanation can be accepted as the *only* way to account for regularities 'produced' in the lab with the use of 'theory-driven' experimental procedures.

Giddens' typology of regularities ('generalizations') has a counterpart in economics. It is the opposition between command economy and market economy. In a *market* economic system, agents freely negotiate exchange rates in transactions among one another. If the same agents are forced to act in an economic *imperatively coordinated association* (Dahrendorf's translation of Weber's *Herrschaftsverband*), they will 'reproduce' the exchange rates taken from the theory they are told to 'enact.' In a command system, the actors behave 'theoretically' for fear that they would be worse off if they did otherwise. In a market system, every actor can improve his own situation through interacting with others, which results in the formation of theoretical (equilibrium) prices. In both systems, the interaction process takes place in a structured environment. In the market case, 'freedom of choice' is institutionalized by means of definite rules concerning legal possession, production, and exchange of valued resources.

Smith preceded his paper (1982) on experimental microeconomics with the motto (from Louis Agassiz) 'Study nature, not books.' I studied both, which encouraged me to compare Giddens' meta-theorizing with the viewpoint on social regularities that grows out of the practice of experimental research. Our colleagues from the department who practiced 'social theory' or historical studies, seeing Jacek Szmatka and me doing experiments on abstract exchange systems, commented on our activities in two ways roughly (but not exactly) corresponding to Giddens' two types of social regularities. Some, impressed by detailed instructions we read to our experimental subjects, blamed us of training them to behave theoretically and thus of misconstruing theory testing; others, who took notice of standardized conditions leaving little room for creative 'defining the situation,' criticized us for treating the subjects like rats, or as Giddens put it, as 'agents ignorant of the circumstances which effectively act on them.'

It is true that the subjects in any experimental social system are taught and induced to act in accordance with well-defined rules. Moreover, the experimenter's task is to ensure that the subjects will co-act upon a common *definition of the situation* that is given in the instructions the subjects must properly understand (and the experimenter must check if they did). On the other hand, in our experiments they were given enough freedom in decision making. Their behaviour could not be interpreted as 'enacting' a theory translated into a script.

In network exchange experiments, the enforcement of a social regularity of the first type is not an end in itself. It is needed only to set the stage for the interaction process that is expected to display a regularity of the second type. Preparing the experimental setting includes establishing definite *structural constraints* and opportunities for negotiating transactions, namely, group members are *instructed* as to with whom they are permitted to initiate and conclude transactions. A fixed set of communication channels can be easily enforced on a group with the use of a *computer network*. Since the system's structure alone cannot force 'agents' to negotiate and conclude transactions, the experimenter-theorist, in order to set the 'interaction machine' in motion, must not only induce subjects to comply with the *rules* but ensure that their actions are guided by an appropriate *motivation.* This is done by having subjects read statements, for instance, like this (Willer 1987, p. 121): 'Your goal should be to get the best score that you can for yourself through arranging the transactions most favourable to you.' Inducing the required motivation allows the experimenter to work without assuming that self-interest is a *natural* human disposition. However, it may appear unfeasible to make subjects behave selfishly as it would require that they suspend the 'natural' or learned inclinations they bring into the laboratory from the external world. Indeed, one of the first experiments on network exchange (Cook, Emerson 1978) confirmed the significance of a preference for equal division of rewards.

If there are reasons to believe that *structural* and *motivational* scope conditions of the theory being tested are met, then the observed outcomes of the group process can be compared with theoretically predicted outcomes. One may ask if the pattern that is expected to emerge from joint action will actually arise if it is known to the actors before the experiment. If the subjects come to know the predicted negotiation outcomes, they may attempt to affect the result of the experiment. How to interpret the case in which the order found to be produced by 'naive experimental subjects' does not occur when the experiment is repeated with 'knowledgeable agents'? Should we conclude that social regularities of the second type lack the 'necessity' that is attributed to the 'laws of nature'? Economists believe that the 'laws of market economy' cannot be changed by those who do not approve of some of their consequences (e.g., highly uneven income distribution). What we know from

20th century history is that people who do not like the 'natural economic order' can knock it down by destroying the structural and/or motivational scope conditions under which economic laws operate. In experiments, the most likely cause of the 'knowledge effect' is not knowledge itself but the appearance of motives that suppress or interfere with those assumed in scope conditions, e.g., the subjects behave so as to show their superiority over 'ignorant rats.'

The truth, rather obvious for sociologists (Willer 1985), about social-structural scope conditions of the laws of market economy, has long been overlooked by most economists. 'Incredibly, it is only in the 20 of these 200 years [of the history of economics] – as Vernon Smith noted (Smith 1982, p. 952) – that we have seriously awakened to the hypothesis that property right institutions might be important to the functioning of the pricing system!' Smith demonstrated that not only property right institutions do matter. His 'experimental handling' of auctions in simple markets has undermined the widespread conviction that economics, like astronomy or meteorology, has to rely on observation of real-world processes. He wrote (Smith 1999, p. 197): 'My view is that the reason economics was believed to be a nonexperimental science was simply that almost no one tried or cared.' The Nobel prize for Vernon Smith (2002, with Daniel Kahneman) gave moral support also to a group of sociologists, who were unaware of experimenting that was going on in economics, but tried and cared to do laboratory experiments on exchange, guided by sociological theories of the third genus.

Network exchange experimental paradigm. Experiments done by the Chair of Research on Group Processes at the Jagiellonian University (1989–2001)

Experimental research (unknown to economists) on exchange systems with *network structure* was initiated in *sociology* at the end of the 1970s by Richard Emerson and his collaborators (Cook, Emerson 1978) to be subsequently directed to a new path by the Elementary Theory group (Willer 1987; Markovsky, Willer, Patton 1988; Szmatka 1997). Classical *economics* has shown little interest in the study of socioeconomic systems endowed with *social constraints* (in particular *network constraints*) that *forbid* some actors from concluding some *physically possible* and mutually beneficial transactions in contrast to *free market systems* where every two *owners* of valued resources are allowed to transfer them between each other on the terms both parties *voluntarily* accept.

In any *exchange system*, a *legal* change in the allocation of control over valued resources can take place only through voluntary give-and-take actions of the actors. The *private property rule* means that each actor has *exclusive* control over some resource. The *reciprocity rule* means in turn that each party of a voluntary agreement has to give up its resource to the other party as soon as the latter has fulfilled its part of the contract. These rules constitute the fixed *institutional* ground for the functioning of any *exchange system*.

Both free market systems and *network exchange systems* can also be endowed with explicitly stated *negotiation rules*, or the rules that establish legal ways of negotiating and concluding transactions. Smith (1982) treated these rules as a social-structural factor subject to experimental manipulation. I discovered independently the theoretical importance of negotiation rules when I carried out my replication (Sozański 1993) of an experiment done by David Willer (1987, Chapter 6). My aim was to test Willer's predictions pertaining to the behaviour of a social system in which a 'manager' (in abstract language, an actor who occupies the central position *connected* to a set of peripheral positions which are not connected between each other) negotiates with candidates for vacant jobs the financial terms of their employment. In a *hierarchical centralized network exchange system* there are as many candidates as vacancies. A *mobile hierarchy* is established by enforcing the rule that the pay guaranteed by the manager to the next applicant must be lower than that awarded to the first applicant already hired. Such a structural constraint forces peripheral actors to compete between each other for being first to reach agreement with the central actor. The competition results in accepting a pretty low pay by the winner of the *auction* to the benefit of the manager. Below I quote the conclusion from the English summary of my paper (Sozański 1993, p. 308).

> The power advantage of the 'centre' over the 'peripherals' has been observed, however, to a lesser degree than in the original experiment … the difference can be explained in terms of different modes of negotiating. The rules (imposed by the experimenter or adopted spontaneously by the subjects) which organize the negotiation process can enhance or weaken the competition among peripheral actors.

In my experiment, the 'manager' had to hear initial demands from all 'applicants' and propose himself the pay for the next person to be hired. Technically, every negotiation round began from a 'complete bidding' in which all 7 subjects (6 in peripheral positions and one in the central position) were called (by the computer program) one by one in a random order to present their proposals. Under such a *negotiation protocol* (Sozański 1993, p. 249–250), there occurred 'class solidarity' among the peripherals, counterbalancing within-class competition to some extent. While in Willer's experiment the 'applicants' went on outbidding one another, in my experiment they often demanded the same pay and accepted the uncertainty about who of them would be hired on the terms they all tried to defend.

Napoleon Bonaparte used to say: 'For war we need three things: money, money and more money.' Although many scientists repeat the same with 'war' replaced by 'research,' I always tell to my students that what we need first of all to do research are ideas, good ideas, and better ideas. In 1990–1991, when I designed and carried out my replication of Willer's experiment, the research unit founded by Jacek Szmatka (see Appendix) had just one 8-bit computer with built-in interpreter of Basic programming language. At that time Jacek was running a series of experiments designed as replications of Willer's experiments (Szmatka 1997). When I was watching my colleague creating in his lab what he called *experimental replicas* of exchange networks, I had not yet been fully acquainted even with Willer's papers (Willer 1981a,b) from which his Elementary Theory has grown. Before I began to read his book (1987), I studied the 'nature' of *empirical* microsocial systems I could see in action in a room which was turned by Jacek into a laboratory then equipped solely with cardboard barriers needed to restrict communication between occupants of network positions.

In a *face-to-face interaction setting*, network positions do not need to have physical counterparts. The actors do not even have to realize that they 'occupy positions in a system.' For example, if we want to construct a system with *network structure* having the form of the *graph* B_1—A—B_2, and with actors s_1, s_2, s_3 occupying A, B_1, B_2, the experimental instructions may reduce to telling actors s_1 and s_3 that they are allowed to communicate only with s_2, while s_2 can communicate with s_1 and s_3. A barrier can be placed between s_1 and s_3 to make sure that they will comply with the ban on communicating with each other.

Having learned from observation the first network exchange experimental paradigm[8] that Jacek Szmatka had come to know in the University of South Carolina laboratory, I noticed that the first technical problem to be solved was recording the groupwide (however running in dyads) negotiation process going on in each round.[9] With one computer at hand, I managed to solve the problem by writing a Basic program. However, the solution involved introducing definite negotiation rules which in some respect could be considered inconvenient, namely, my program did not enable the actors to freely choose the time for making their offers to potential partners or responding to others' offers. Consequently, competition among peripheral actors for the mere opportunity to present their offers to the central actor was eliminated, which on the other hand resulted in revealing the effect from negotiation protocol.

The research grant won by Jacek Szmatka in 1994 from the Polish counterpart of US NSF recalled the truth that money does matter in doing science too. We were at long last able to equip our lab with a *local computer network* made up of 7 personal computers (the server and 6 workstations). John Skvoretz, then working with David Willer at the University of South Carolina, made available to us his program Exnet (written in Quick Basic 4.5 working under the Novell Netware operating system); he was also kind to help us install it in our laboratory, which in 1995 became ready for running technically advanced network experiments.

Our plan was to examine *all* 8 smallest non-isomorphic *exchange networks with one-exchange rule* (*one-exchange networks* for short), 2 networks with 3 positions, and 6 with 4 positions. The *one-exchange rule* means that every actor is allowed to

[8] There are many experimental paradigms and theories which have been proposed after Emerson and Cook's published their seminal paper (1978). A recent comprehensive account can be found in Molm's (2014) chapter in the 2nd edition *Laboratory Experiments in the Social Sciences.*

[9] To generate data for a single network with n positions you need at least one set of n subjects. Each set of subjects can be used in multiple rounds in which the assignment of actors to positions remains fixed. Such a sequence of rounds is called a *period*. A *session* with one group may consist of a few periods with a different actor-position assignment in each. The *rotation* technique used in the University of South Carolina laboratory allows every actor to occupy all positions in one network throughout the session. Rotation can be criticized for *systematic* use of too few out of many possible assignments of actors to positions (e.g., in a 4-point network only 4 out of 4!=24 are used). A random selection of assignments for use in one session seems to me a better method for controlling *subject variables.*

conclude no more than *one* transaction per round. This rule implies that the negotiation process in the network with the transaction opportunity graph of the form B_1—A_1—A_2—B_2 may end up with 5 outcomes: (1) no transaction; (2) a transaction between A_1 and B_1; (3) a transaction between A_2 and B_2; (4) two transactions: one between A_1 and B_1 and the other between A_2 and B_2; (5) a transaction between A_1 and A_2.

I refer to this one-exchange network and the underlying graph as the 4-Chain.[10] I will take this network, which has so far been studied most frequently of all, to describe constitutive components of the network exchange experimental paradigm used in our laboratory.

1. The first component is an operational definition of a *transaction*. In network exchange experiments, a transaction is not what the word 'exchange' suggests (a mutually agreed-on bilateral flow of valued resources). Instead, a transaction is understood as a bilateral agreement on a division of a pool of M points (usually M=24).
2. The *transaction opportunity graph* is the second component of the paradigm. The *points* of this graph are called *network positions.* Two actors *s* and *s*' who are placed in positions *P* and *P*' in a given *negotiation round* are permitted to conclude a transaction if their positions are *connected* in the network, that is, *P*—*P*' is a line of the transaction opportunity graph.
3. The third component specifies the range of transaction configurations which may occur in one round. The first assumption is that any pair of actors is allowed to conclude no more than one transaction per round. With this assumption made, an *exchange regime*[11] is defined as a collection of transaction sets. Any *transaction set* consists of lines which can be the locus of transactions within any round. A round ends if all transactions in a *maximal transaction set* have been concluded. In the 4-Chain network, one-exchange rule generates the exchange regime with 5 transactions sets of which two are maximal: $\{A_1$—$A_2\}$ and $\{B_1$—A_1, A_2—$B_2\}$. Non-maximal transaction sets, such as $\{B_1$—$A_1\}$, may also appear in a round, which happens when the time allowed for negotiations has expired.
4. The *negotiation protocol* specifies the range of *actions* available to the actors and defines a sequence of actions that must take place to be automatically followed by a transaction. The protocol implemented in the version of Exnet we used in our laboratory admits of three types of actions. The first of them consists in sending an *offer* (a proposed division of the pool of profit points) by an actor to one of his neighbours in the transaction opportunity graph. If the recipient of the offer *accepts* it, the sender may *confirm* his offer. If he does it, the sequence of three actions initiated by him is followed by the transaction. Due to this condition all offers are tentative. The actor whose offer has been

[10] The name 4-Line is used more often in the literature but 4-Chain is a better name because it avoids confusion with the term *line* (edge, link) commonly used in *graph theory* to denote a pair of connected points (vertices, nodes). Notice that the 4-Chain graph has 3 lines.

[11] The term and the germ of the idea I elaborated in my paper (Sozański 2006, p. 398–399) comes from Friedkin (1992).

accepted may confirm it, but he may well send a new offer to the same partner or stop communication with him and start negotiations with another potential partner.[12]

5. The paradigm must also contain assumptions on the actors' *motivation* or *goal-orientation*. Each actor is supposed to negotiate so as to maximize his own profit only; 'tactical' decisions on how to pursue this goal are left to himself.
6. All actors are assumed to have *full information* about the system and its components. They are also given an opportunity to watch the course of negotiations throughout the session in which they participate.

The experimental paradigm I have just described suffices in itself to predict that only maximal transaction sets should be observed in any negotiation round. The full information assumption (6) implies that if the actors in positions A_1 and B_1 have agreed on a pool split, their transaction becomes known to the actors in positions A_2 and B_2. Their awareness of this fact motivates them to conclude a transaction on any terms. Otherwise none of them would gain any points, which is in contradiction with the assumption of individual rationality (5). As a consequence, such a round must end with the maximal transaction set $\{B_1—A_1, A_2—B_2\}$.

However, the paradigm does not by itself imply any *concrete theory* which would generate specific *predictions* as to how the pool shall be divided between the partners in each transaction within a maximal transaction set. One can only require that any plausible theory must be *structural*. Any *structural theory*, applied to the 4-Chain network, predicts the same pool split in network lines $A_1—B_1$ and $A_2—B_2$; the occupants of positions A_1 and A_2 (B_1 and B_2)[13] are expected to earn on average the same number of profit points.

If we define a *structural parameter*[14] suitable for measuring the *bargaining power* of a position in a one-exchange network, what we need in order to construct a *precise* theory predicting the negotiation process outcome in a round is a *formula* that will allow us to *calculate* the theoretical payoffs of the partners from the values of the chosen power parameter. When the first power parameter (*Graph-theoretic Power Index*, GPI) devised by Markovsky, Willer, and Patton (1988) appeared inadequate for *weak power networks*, the quest for new power parameters and new theoretical formulas began (Lovaglia et al. 1995) to continue until recently.

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[12] I proposed (Sozański 1997, p. 314–316) an alternative protocol under which an actor addresses the same offer to all his neighbours. He may also choose one of them as the current target of his proposal. The transaction between two actors is assumed to follow automatically as soon as they choose each other and make complementary offers (agree on a split of the pool). The sequence composed of two last offers and two last partner choices can contain the four actions in any order.

[13] Positions labelled with the same letter are *automorphically equivalent*, that is, one of them is the image of the other through an *automorphism* of the transaction opportunity graph. The one-to-one mapping F of the set of 4 positions $\{A_1, A_2, B_1, B_2\}$ onto itself such that $F(A_1) = A_2$, $F(A_2) = A_1$, $F(B_1) = B_2$, $F(B_2) = B_1$ and the identity mapping are the only automorphisms of the 4-Chain graph.

[14] A *structural parameter* of a point in a graph is defined by the condition of assuming the same value for any two automorphically equivalent positions.

At this point I must stop discussing further theoretical developments and return to the story on experiments carried out in the 1990s by the crew of the Chair of Research on Group Processes at the Jagiellonian University. The series of experiments we started in 1995 was aimed at testing predictions derived from various specific theories proposed until then, their unity being based on a common *scope* and on the use of *structural variables.* Instead of focusing on the study of larger networks in which our American colleagues found certain peculiarities, we decided to concentrate our efforts on *systematic* examination of all small-size one-exchange networks in order to assess prediction *accuracy* of each *precise* theory and identify among them the one which provides best fit to the data generated for the networks for which any *general* theory of network exchange should do particularly well.

We completed our experiments by the end of 1996, yet the results, which have so far been presented only at two conferences[15], are still waiting for being published in a research paper or book chapter. Jacek found for himself another research area (Szmatka et al. 1998), *conflict networks*, also suitable for experimental treatment.[16] The cause of my long-lasting neglect was that my enthusiasm for doing *empirical* theory and research weakened a lot when I plunged again into solving *mathematical* problems (Sozański 1997, 2006). But every scientist who has once come to know the taste of experimenting will long for a return to this exciting activity. For me a return to laboratory work will no longer be possible but at least I can enjoy discussing *methodological* issues and the intricacies of the *technology* of experimenting with my colleagues who reveal equally strong commitment to experimental social science.

Conclusion and an introduction to the collection of papers that follow

Experimental social science was born more than a hundred years ago. Today it is a well-established way of doing *theory and research* in sociology and related disciplines. The first upsurge of interest in experimentation, which took place in the 1950s, yielded classical studies of *group dynamics* (Cartwright, Zander 1960). Those studies became widely known due to their coverage in social psychology handbooks (Collins, Raven 1969). A new wave of *theory-driven experimenting* and *experimentally driven theorizing* came in the 1980s as a consequence of successful attempts to construct sociological *theories of the third genus* (see earlier in this paper). This way of doing theory, which for a long time has not been recognized as a serious challenge to the first genus theorizing, is now considered legitimate as evidenced by the entries in George Ritzer's *Encyclopedia of Social Theory* (2005) devoted to the *Elementary Theory* and its authors (David Willer, Barry Markovsky).

In the 1970s, *social theory* underwent a change described by the French saying *le roi est mort, vive le roi*: Anthony Giddens, the author of *Central Problems in*

[15] The ASA Annual Meetings, San Francisco, August 1998 (Regular Session: Group Processes–Theory and Experiment on Power and Exchange) and Fourth International Conference on Theory and Research in Group Processes and Social Psychology, Cracow, June 2004. The latter conference, dedicated to the memory of Professor Jacek Szmatka, was co-organized by ISA RC #42 (Social Psychology).

[16] Experiments on conflict networks were done in our lab by Joanna Heidtman, Ph.D.

Social Theory (1979) and of another treatise (1984) I quoted earlier in this paper, became the successor of Talcott Parsons as 'king' of this *genre* of theorizing within the Anglo-Saxon world. I did not care too much about what excited at that time many sociologists in Poland and abroad. In that decade for me the most important book, the reading of which prepared me for joining Jacek Szmatka's research team in 1990 (see Appendix), was volume 2 of *Sociological Theories in Progress* (1972), a collection of articles edited by Joseph Berger, Morris Zelditch, Jr. and Bo Anderson. Another book (Berger et al. 1977), published next, was my first source of information about *Expectation States Theory* (EST)[17]. This theory was later counted by Jacek Szmatka (Szmatka, Sozański 1994, p. 229) along with the Elementary Theory (his favourite example) among the few theories epitomizing the third way of theorizing. In the 1970s – it was the time when I began my scientific activity – my interest in EST focused on *formalizing* this theory with the use of *signed graphs*, a special area within *graph theory.*[18] Later it became clear to me that the core of EST is a procedure (Berger refers to it as the 'Standardized Experimental Situation') which is used in laboratory experiments to endow a dyad with an artificial *status structure.*

Joseph Berger, Bernard P. Cohen, and Morris Zelditch, Jr. are credited with making EST a *theoretical research program* (Berger 1974) which 'consists of a set of interrelated theories, bodies of relevant research concerned with testing these theories, and bodies of research that use these theories in social applications.' (Berger 2014, p. 269). Berger, now Professor Emeritus of Sociology at Stanford University, has been the leader and currently is the senior member of the circle of scholars working under the banner of 'theory and research on *group processes*.'

In Expectation States Theory, the distinction between *low* and *high* status is defined in terms of unequal levels of *competence* (in performing a task or a special kind of tasks) that group members attribute to each other. Such a meaning given to the concept of *status* departs from the traditional Weberian understanding of status structure.[19] The same can be said about *power* and other old concepts, which

[17] To get familiar with the basics, see Martha Foschi's (2000) excellent encyclopaedic article.

[18] The English title of my Ph.D. thesis (in Polish, 1982) is: 'Structural Balance Model. Theory of Signed Graphs and It's Applications in the Social Sciences.'

[19] According to the traditional European approach to status (prestige) structure, unequal level of competence in a given area of human activities (e.g. , in doing science) need not be the main reason for unequal distribution of respect or 'status honour' (*ständische Ehre*, Weber's term). The differential *evaluation* of various kinds of tasks may also generate a hierarchy with definite consequences (e.g., interpersonal influence) for social interaction. For instance, white-collar workers enjoy a higher status than blue-collar workers because what the former do to earn a living is believed to be a 'nobler' kind of job. In the Middle Ages, knights, or those who were attributed a good command of the sword, were higher in the status hierarchy than peasants expected to be competent in operating a plough. In the academic world, both bases of inequality are found. The competence-based status structure, which exists within each discipline, in Poland has the form of a two-grade system with 'low' and high 'status' marked by 'dr' (roughly the counterpart of Ph.D.) or 'dr hab.' placed before a scholar's name. Another status hierarchy, which depends on which branch of science you deal with, is less clear but certainly expertise in *social science* is not as highly evaluated as doctorate or habilitation in *legal science*.

re-appeared in a new shape within theories rooted in experimental paradigms. Giddens is right to claim (1984, p. 283) that 'There is no more elemental concept than that of power … Power is one of several primary concepts of social science, all clustered around the relations of action and structure.' Even though Willer invokes Max Weber many times in his book (1987), again there is at best a loose connection between the Weberian understanding of power and the meaning of this concept within the theoretical research program (Willer, Markovsky 1993) that had its origin in Willer's analysis (Willer 1981a) of elementary dyadic social relations.

However weak the ties may be between *experimental* social science and sociology at large, it is not my intention to call for soft divorce with the sociological tradition. Wherever sociology is taught, its two traditional pillars remain the knowledge of major 'masters of sociological thought' and that of 'the basics of social research.' As regards this second pillar, it should be noted that experiments done in the 1980s have been noticed and appreciated outside the Group Processes circle. The experiment designed by Martha Foschi and her two younger collaborators (see Foschi, Warriner, Hart 1985) was used by Earl Babbie in the 5th (1989) and all later editions of *The Practice of Social Research* to illustrate experimentation in sociology.[20]

The Group Processes circle[21], which came into being in the 1980s, has since then remained incessantly active until today, publishing every year since 1984 a successive volume of the book series *Advances in Group Processes*.

> Early experimental research documented how the ideas and norms of one generation feed into another generation even when the members of the preceding generation are no longer present.[22] This lasting of our intellectual ancestors is clearly demonstrated in

[20] In that experiment, Martha Foschi used for the first time standards as an independent variable, a *standard* being defined as the lowest level of performance (e.g. a score in psychological test) that must be attained in order that a person's performance could be recognized as 'satisfactory' or taken as a proof of competence. Foschi later published several articles about standards, in particular, the practice of *double standards*. One of her papers (*Podwójne standardy oceny konferencji: najnowsze wyniki i nowe kierunki*, translated by Z. Karpiński) appeared in Polish in Heidtman J., Wysieńska K. (eds.). (2013). *Procesy grupowe. Perspektywa socjologiczna.* Warszawa: Wydawnictwo Naukowe Scholar. This volume edited by Heidtman and Wysieńska also contains articles (Polish translations) by other contributors to this issue of *Studia Sociologica*.

[21] Most of the members of the Group Processes circle have been American scholars or their collaborators from other countries: Canada (Martha Foschi), Japan (Toshio Yamagishi), Turkey (Hamit Fişek), and last but not least, Poland (Jacek Szmatka and me, Jacek's former students: Joanna Heidtman, Zbigniew Karpiński, and Kinga Wysieńska-Di Carlo). Let me add in this connection that in 1994–2014 four internationally known members of this circle (Karen Cook, Guillermina Jasso, Edward Lawler, and Cecilia Ridgeway) were presidents of ISA Research Committee #42 (Social Psychology).

[22] Among those who 'are no longer present' there is one scholar whose name should be recalled here. I mean Bernard P. Cohen, the author of *Developing Sociological Knowledge* (1989), the book which heavily influenced methodological views of many scientists doing theory and research in group processes. Cohen's earlier book (*Conflict and Conformity*, 1963) about an application of *Markov chains* (a probability model) to the data from Asch's experiment was one of my first readings in *mathematical sociology*.

> the Group Process meetings[23] by the continuing collective commitment to theoretical development, methodological precision, and the integrity of the scientific process.

The quoted passage closes the Editors' Preface to the 2nd revised edition of *Laboratory Experiments in the Social Sciences* (1st ed. 2007). My recent appointment as head of the Chair of Methodology of Social Research, a unit within the Institute of Philosophy and Sociology at the Pedagogical University of Cracow, my intention to enrich with an international event the celebration of the 70th anniversary of founding the school, which became my workplace 10 years ago, and lastly but not least importantly, the appearance in the same year 2014 of the 2nd edition of the aforementioned book – all that inspired me to organize the *International Symposium on Experimental Research in the Social Sciences* to be held in Cracow next year.

To attract participants I sent a call for papers to ISA Research Committees #42 and #45 (Rational Choice) of which I am a member, and I addressed specialists in methodology of experimental research. My plan appeared workable in part. Martha Foschi and Murray Webster, Jr. accepted my invitation to deliver keynote lectures. Murray Webster, the 1st editor (with Jane Sell as 2nd editor) of *Laboratory Experiments*, is a key figure in experimental social science. In 2015 he received the Cooley-Mead award.[24]

The time chosen for the symposium, 12th and 13th June, 2015 might have been inconvenient for those potential participants who were more interested in attending in June the annual Sunbelt Conference organized that year in the UK by the International Network for Social Network Analysis. As a consequence, among 'selected topics in experimental social science' you will not find the one which has always been closest to my research interests and experience, namely *experiments on network interaction systems*, or the topic which might have been treated most competently by Professor John Skvoretz, President of INSNA, 2010–2016, once a long term collaborator of Jacek Szmatka's Chair of Research on Group Processes at the Jagiellonian University.

My editor's hard work that followed the reviewing/revising phase of preparing this special issue of *Studia Sociologica* has ended with accepting 8 articles for publication. All of them except one (Szymon Czarnik's paper) are extended or re-edited versions of the papers presented at the June 2015 symposium. Since the abstracts written by the authors themselves present an overall description of their respective contributions, I will limit to a minimum my introductory comments, highlighting

[23] Since 1988 conferences on theory and research in group processes have been organized each year as an event accompanying the Annual Meetings of the American Sociological Association.

[24] The Cooley-Mead selection committee noted that 'in his distinguished career of nearly 50 years, Murray has been a leader in developing expectation states theory, identifying the processes by which status characteristics … shape and organize social interaction, and promoting rigorous, state-of-the art experimental scholarship.' The Cooley-Mead award was established in 1978 by the Social Psychology Section at the American Sociological Association. Webster joined the list of winners containing names known to every sociologist (e.g. Goffman, Homans, Bales, Merton), as well as those of several members of the Group Processes circle (Joseph Berger, Morris Zelditch, Jr., Edward Lawler, Bernard P. Cohen, Karen Cook, Cecilia L. Ridgeway, and Linda Molm).

solely some topics or questions that seem to me interesting; the readers need not of course share the commentator's view in this matter.

Martha Foschi's article (*Experimental Contributions to Sociological Immigration-Research*) reports on the results of an extensive literature search for papers in which immigration topics were investigated experimentally. She selected nine studies 'to illustrate the variety of factors and designs that have been used in this area.'[25] These studies, each interesting in itself, are compared with respect to the type of manipulated independent variables and the type of dependent variables (the latter 'consist of either written responses or actual behaviours concerning immigrants'). My observation is that most experimental studies of attitudes toward immigrants (e.g., presented as 'job applicants') are 'empirically driven,' even if manipulating independent variable consists in introducing into a vignette factorial design a variable *abstractly* defined by mere distinction between 'immigrants' and 'non-immigrants.' Some general topics in the methodology of experimentation ('artificiality' of an experimental setting, 'generalizability' of experimental findings) are examined at the end of the article. The author's conclusions clarify some matters that are often considered controversial.

The article by Murray Webster, Jr. with Jane Sell as 2nd author (*The Present Status and Future Prospects of Experiments in the Social Sciences*) begins with a presentation of the basics of the experimental method. In particular, the authors highlight the importance of what they call *strong instantiation*. Instantiation 'means creating a concrete instance of the abstract concepts in a theory or in a hypothesis, and it should be done as clearly and as powerfully as possible. Subtlety is out of place in experimental design … Weak instantiation of independent variables risks producing high variance within conditions and small overall difference across conditions.' I agree with the authors that the problem of reducing 'variance within conditions' is crucial for the success of the experimental testing of a hypothesis no matter whether the latter is derived from an *abstract* theory or comes from an analysis of a concrete experimental setting. In the second part of their paper, Webster and Sell deal with more technical matters, for instance, they 'trace developments in a standardized design that has been widely used to study status and expectation state processes' and present 'some new designs [that] are being developed to study interrelations of vocal accommodation and group position.'

In their methodological article (*Assessing Epistemic Claims by Experimental Evidence*), Robert K. Shelly and Ann C. Shelly analyse 'three ways in which epistemic claims may be advanced and assessed: triangulation, multitrait-multimethod, and meta-analysis'. 'Triangulation and multitrait-multimethod provide strong answers to the question of how do we know what we know by specifying the links between theory, data, and measures. Meta-analysis is not quite as robust on this issue…'

Two short papers that follow deal with the role of socio-cultural context in experimenting. Jane Sell and Murray Webster conclude their second contribution (*The Importance of Cross-Cultural Experiments for the Social Sciences*) – it can be regarded

[25] Quotation marks that appear here and further in this section delimit pieces of text taken from commented papers.

as a supplement to the first one – with a remark that cross-cultural replications of an experiment need to be done to demonstrate that 'general principles apply even in very different contexts and initial conditions' but also to examine how cultural specific *initial conditions* affect general laws.

In her paper (*Can Socio-Cultural Context Affect Experimental Results? The Case of the Zimbardo Prison Experiment Repeated in Poland by Artur Żmijewski*), Iza Desperak describes a repetition of the Stanford Prison Experiment[26] in Poland. The makeshift prison in Stanford had to be closed after 6 days because the 'guards' abused the power given to them by the experimenter. By contrast, in Poland an intervention of the artist playing the role of 'prison governor' was not necessary: the 'guards' and 'prisoners' resolved together to stop the performance they had been induced by him to take part in. Why? A tentative answer is given by the author in her paper.

Marcel Kotkowski is the author of the last of eight articles (*Psychophysiological Techniques for Measuring Emotion in Social Science*). His paper can serve as a useful source for any sociologist who would like to gain elementary knowledge of various techniques for measuring emotions. 'A note on each technique points out the dimension of emotion (valence or arousal) that is measured with a given technique, and informs on its previous use in sociology, as well as its major advantages and disadvantages.'

Two contributions that remain to be presented here are good examples of doing theory-informed experimental social science. In their paper (*Modelling Social Situations: Trust and Cooperation Among Strangers of Unequal Status*) Zbigniew Karpiński and Kinga Wysieńska-Di Carlo[27] report on the results of the two experiments they designed to test hypotheses that relate frequency of cooperation in certain social situations (modelled by two-person Prisoner's Dilemma game) to the configuration of partners' statuses (Low-Low, Low-High, High-Low, High-High). To derive their predictions, the authors invoke status characteristics theory as well as collective action theories, making an attempt to *integrate* 'theories originating in distinct general research programs.' The article also has didactic value: it is instructive to see how an analysis of the results of the first experiment leads the authors to design the next one.

Szymon Czarnik's article (*Reading Minds of Experimental Subjects. Insights from Pre- and Post-Experimental Surveys in a Redistribution Game Experiment*) is also instructive as it demonstrates how large can be the range of social phenomena amenable to laboratory experimentation. For the purpose of his experiment Czarnik placed each pair of subjects in a *socioeconomic system* in which: (1) The actors work and earn money proportionally to the amount of work done; (2) Their incomes are subject to taxation with the rate of linear tax depending on the actors' decisions (they are asked to reveal their preferred tax rates) and on a 'democratic' rule (the rate to be implemented in the system is computed as the average of the

[26] Willer and Walker (2007, p. 100) comment on Zimbardo's experiment in the following words: '…we are unable to identify any theory or theoretical model under test. Consequently, it is neither a method-of-difference nor a theory-driven experiment.'

[27] The co-authors declare having contributed equally to their product.

rates proposed by the actors); (3) A fixed fraction of the total tax collected from the 2-person group is lost and the rest is divided evenly between two actors to the effect that one of them benefits from the *redistribution* while the other loses some part of his income earned before taxation and redistribution; (4) At the last stage the actors are given an opportunity to make *voluntary* money transfers to each other.

Czarnik had already published a report on this experiment in a 'hard science' journal (Czarnik 2006). The paper that I received from him after the June 2015 symposium begins from recalling the redistribution mechanism and analysing the two-step game that is obtained by having the subjects make decisions in phases (2) and (4) of the system's functioning. In the current article, mathematical considerations are only a prelude to an examination of the *subjective* dimension of the collective behaviour within such as system, including the subjects' declared motives and those attributed to others ('We find experimental subjects to be predominantly negative in their assessment of intentions behind their partners' decisions …').

I conclude this last section of my introductory article by stating 5 postulates or principles guiding theory and research on group processes.[28]

1. Both *natural sciences* and *social sciences* are empirical sciences. Although they differ in the nature of objects to be studied, general methodological norms that apply to all *empirical sciences* remain valid for social science.
2. The aim of *basic social science* is to study *abstract* social systems (e.g., network interaction systems) rather than historical concrete objects (such as 'Polish society AD 2016').
3. *Theories* that are to describe regularities that characterize the functioning of these systems should be *parsimonious* in making assumptions on the nature of human actors (assumptions concerning people's motivation or their knowledge of the conditions of action). Where psychologists, whose task is to deal with human subjectivity, need to invent complex models of an 'individual in action,' social scientists should instead simplify, focusing on building more or less complicated models of 'social systems in action' in which the *form of structure* of a social action system (such as an exchange network) is taken as the central factor in explaining the system's behaviour.
4. Theory and research should begin with the study of *elementary* social phenomena or processes (power, status, influence, cooperation, etc.).
5. *Laboratory experiment* is the best method for testing theories that deal with these phenomena.

[28] My current formulation of these principles repeats the ideas already expressed in Polish in my obituary (Sozański 2001, p. 8–9) devoted to Jacek Szmatka.

Appendix

Jacek Szmatka (1950–2001)

The collection of papers *Selected Topics in Experimental Social Science* appears (as part of the current issue of *Studia Sociologica*) 15 years after Professor Jacek Szmatka passed away. This appendix is to recall to foreign readers of this paper my late colleague who was a keen advocate of using experimental method in sociology. Three days after his decease I emailed a letter to his overseas friends and professional associates. In 2004, when my presence in the Internet began, I placed that letter on my personal website (http://www.cyf-kr.edu.pl/~ussozans/) supplemented with the list of Jacek's publications in English. Both are now placed in this special issue of the journal published by Pedagogical University of Cracow to document in print an episode that the history of Polish sociology owes to Jacek Szmatka.

Dear Colleagues

The sad duty has fallen upon me to inform you that Jacek Szmatka passed away October 20, 2001, in Athens, Ohio where he was staying this semester as visiting professor.

We met in 1968 when we began studying sociology at the Jagiellonian University in Cracow. Jacek came to our city from Rzeszów (then a county town east of Cracow) where he was born in 1950. Among the members of our sociology class he was the first to receive his M.A. (1972) and Ph.D. (1975), both from the Jagiellonian University where he worked continuously from 1972.

We met for the second time as assistant professors affiliated with the Chair of Theoretical Sociology headed by Professor Piotr Sztompka. Jacek was then interested, first of all, in general methodology and social theory as documented by the titles of his Ph.D. thesis ('Theoretical Reduction in Sociology'), and that of his first book ('Individual and Society: On the Dependence of Individual Phenomena on Social Phenomena') which he published in 1980 as his 'habilitation dissertation,' a requisite in Poland in order to be appointed to the position of associate professor.

Szmatka's collaboration with American sociologists dates back to the early 1980s. He translated into Polish many classic papers on small groups as well as Jonathan Turner's *The Structure of Sociological Theory* (Polish edition, 1985). Jacek also wanted to make his native country's sociology known abroad. He was invited to the board of the International Advisory Editors of Encyclopedia of Sociology, edited by Borgatta and Borgatta (first edition, New York: Macmillan, 1990-1992) for which he wrote the entry on 'Polish sociology.' Though he conceived of theoretical sociology as a science which should deal with abstract social structures rather than historical societies, he often taught courses on the problems of Poland and Eastern Europe and co-edited (with Z. Mach and J. Mucha) a volume on these topics (*Eastern European Societies at the Threshold of Change*. New York 1993).

Jacek came to the US for the first time in 1983. Since then he was a frequent guest to America where he felt at home nearly as much as in Poland. He worked as a visiting professor at many American universities (University of Kansas, State University of New York, Stanford University, University of Washington, University of South Carolina, University of Iowa) and regularly attended Annual Meetings of the American Sociological

Association (he had been an ASA member since 1991). The circle of scholars which used to meet separately at 'group processes conferences' accompanying the ASA Meetings became his 'reference group'; they helped him reorient his scientific interests from 'grand theory' to 'hard social science.' He made many friends among the members of this group who could certainly add their own memories to this informal obituary.

University of South Carolina was the place in America that Jacek visited most frequently. There he came to know the Elementary Theory (ET) and established close ties with David Willer and his colleagues. The long-term cooperation of Dave and Jacek, which yielded several co-authored papers, began in 1989 with a common research project aimed at testing the universality of ET.

My third encounter with Jacek which gave rise to our cooperation throughout the following decade took place just at the time when Jacek got fascinated with the Elementary Theory. In Spring 1990 somewhat unexpectedly I saw my colleague, whom I had known earlier as a 'grand theorist,' doing 'cross-national experiments' in his office now turned into a laboratory.

The historic year 1989 in which the communist regime fell in our country was equally crucial in his career. Jacek had by then published his second book ('Small Social Structures: Introduction to Structural Microsociology') which established his reputation in Poland as an outstanding specialist in small group theory and research. That same year he was appointed head of the Microsociological Laboratory which he had created in the Department of Sociology at our university. Jacek's achievements had not gone unnoticed. In 1992 he received the title of professor which granted him tenure. In 1995, his research unit (renamed the Chair of Research on Group Processes in 1996) was equipped with a local computer network which, together with software received from South Carolina, enabled him and his team to actively participate in the development of Network Exchange Theory as the first lab of this kind in Europe.

Jacek owed his academic success to his bright intellect, hard work and ambition to keep pace with recent developments in his discipline. With his innovative spirit he was able to locate new research areas such as 'conflict networks' which he began to study with his collaborators a couple of years ago. As a self-made man he welcomed the new funding opportunities opened up to individual scholars when National Committee for Scientific Research (the Polish counterpart of the American NSF) began organizing research proposal competitions. He was among the few Polish sociologists who won research grants three times over the last decade. He also gained an international reputation as a conference organizer and editor of a few collective works of which the most important was the volume Status, Network, and Structure: Theory Development in Group Processes (Stanford 1997) which he co-edited with John Skvoretz and Joe Berger.

In Spring 2000, a sudden attack of strong pain made him seek relief in the hospital. When he learned how serious his disease was he did not fall into depression. He firmly believed he would win the struggle with cancer and worked as hard as he used to. He was planning to upgrade his lab so as to meet the needs of the research designed by his last Ph.D. student, Ms. Kinga Wysieńska, whom he met in Fall 1997. He invited her to join his research team which, until then, included myself and Joanna Heidtman who had been Jacek's primary collaborator in his conflict network research. When he was released from the hospital in Cracow after isotope therapy, his Polish colleagues could see him as active as usual. Soon afterwards, he took part as a session organizer during the 11th

Congress of Polish Sociology in his hometown Rzeszów in September 2000, and then travelled to the University of Iowa to teach and continue his research there as a Fulbright fellow. The therapy he received in Iowa appeared to be working so he welcomed Bob Shelly's invitation to come to Ohio the following year.

I received my last email message from him on September 23. He wrote me that he felt worse again but still believed in his recovery. Some two weeks later I learned from his family that cancer had attacked his lungs and his life was coming to an end. He died on October 20, 2001.

With Jacek's passing Polish sociology has lost an outstanding scholar whose pro-science stance had inspired many students and researchers over the years, even if the radical form in which he occasionally presented his views might have sometimes appeared irritating to some people outside the group processes circle.

He was my friend and closest collaborator with whom I had communicated daily since 1990, regardless of whether he was here in Cracow or somewhere over the ocean (from 1992 to 1998 we exchanged some 1500 email messages). I will remember him, too, as the leader of our small group, formally, the head of the Chair of Group Processes, and last but not least, as the person to whom I owe my contacts with other scholars sharing the idea of scientific sociology which Jacek had outlined in his paper (*On Four Myths about Sociology and Three Generations of Sociological Theories*) which opens the book he co-edited with me ('Structure, Exchange, and Power. Studies in Theoretical Sociology,' in Polish, Warsaw 1993).

May his name and work remain in our memory.

Tad Sozański

Jacek Szmatka's publications in English 1989–2002

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Eksperymentalna nauka społeczna

Redaktorski wstęp do zbioru artykułów w języku angielskim, zatytułowanego *Selected Topics in Experimental Social Science* [Eksperymentalna nauka społeczna: wybrane zagadnienia], wypełniającego prawie cały bieżący numer *Studia Sociologica*, powstał przez rozbudowanie do rozmiaru artykułu prezentacji pokazanej przeze mnie jako organizatora na sesji otwierającej międzynarodowe sympozjum („International Symposium on Experimental Research in the Social Sciences"), które odbyło się Uniwersytecie Pedagogicznym w Krakowie w dniach 12−13 czerwca 2015. Artykuł ten łączy metateoretyczne rozważania na temat teorii i eksperymentu w naukach społecznych z informacjami o eksperymentach laboratoryjnych wykonanych w Zakładzie Badania Procesów Grupowych Instytutu Socjologii UJ od powstania (1989) tej nieistniejącej już placówki badawczej do przedwczesnej śmierci (2001) jej założyciela prof. Jacka Szmatki.

Słowa kluczowe: eksperyment, metodologia nauk empirycznych, trzy generacje teorii socjologicznych, sieciowy system interakcji