Tools, Toys, Truisms, and Theories: Some Thoughts on the Creative Cycle of Theory Formation

Klaus Fiedler
Department of Psychology
University of Heidelberg

*Theory creation underlies the basic laws of evolution, calling for both random varia-
tion (loosening) and critical selection processes (tightening). Theory construction re-
quires support of both components. However, whereas psychology has created a re-
fined methodology for the tightening stage, attempts to understand the loosening
stage of the creative cycle are conspicuously missing. I propose that both aspects of
the creative cycle of theory formation can be acquired and exercised playfully,
through distinct loosening and tightening games. I outline the rules of these games
and illustrate them with examples from contemporary research.*

In my opinion, the topic and objective of this special issue is a timely one. For half a century now, the scientific discipline of psychology in general and social psychology in particular have been flourishing, both in terms of empirical research and fields of application. In an interdisciplinary comparison, psychology has gained a high reputation, almost equivalent to classical science, due to an elaborated methodology; knowledge export to other disciplines; contributions to economical, political, and societal problems; and the charisma of its leading researchers. However, at the contemporary end of this incontestable success story, current (social) psychology is facing a demanding developmental task, namely, to organize the rapidly growing knowledge in theoretical space.

One might characterize the current historical situation as the end of an era of hunters and gatherers that have been chasing after interesting phenomena and "empirical food" for future consumption, thereby enhancing their weapons (methods) and circumscribing their territories (paradigms and subdisciplines). After this productive acquisition period, however, it seems hard to oversee the entire harvest (findings) and to decide which healthy (reliable, replicable) goods should be conserved and which should be discarded as poisonous or hard to digest (invalid, illusory). Theories are required to find out which products of hunting and gathering could be combined to breed something novel that can exist on its own.

The topic of this special issue—that the creative process of theory formation has received much less attention than the methodological issue of theory testing—may be but one symptom of a more general syndrome. The empiricist period described previously is characterized by a general lack of interest in stringent theoretical and metatheoretical thinking. If current social psychologists were more concerned with theory testing than with theory creation, then they should be highly aware of distinct theoretical implications that drive their experiments, theories should be formulated in precise terms, open controversies should be the rule rather than the exception, and empirical constraints derived from theories, their domains, and boundary conditions should be explicit and commonly shared. However, published research even in prestigious journals hardly fits such a profile of stringent theory testing. Theories are rarely encoded in precise terms, strong controversies are rarely entertained, the failure to define the domain of empirical predictions often reduces the theoretical value to simple existence proof ("under some conditions, phenomenon x can happen"), and researchers are rarely explicit on what outcome their preferred theory excludes.

Thus, we are not merely concerned with a stronger emphasis on theory testing as opposed to theory creation, but we are facing a general neglect or agnostic state regarding theoretical issues in general. Industrious research appears to be mainly driven by available research tools (computers, video, neuroscience tools), methods of analysis (structural equations, meta-analysis), and the dynamics of scientific subcommunities but is surprisingly often detached from clearly spelled-out theories. For example, hundreds of labs in the world are busy with more or less sophisticated versions of priming effects, even though hardly anyone cares about alternative theories of priming (cf. Ratcliff

& McKoon, 1988). Very few stimulus-driven phenomenon could not be termed a priming effect, and yet the notion is often used to explain biases in judgments and memory. *Priming* as a unitary verbal label does not warrant an invariant underlying process or law.

Similarly, few serious attempts are made to present a theoretical model of the cognitive and motor processes underlying the Implicit Association Test (Greenwald, McGhee, & Schwartz, 1998), which occupies hundreds of research labs. Ironically, perhaps, the dispensability of theories is also apparent in the modern neuroscience approach to cognitive and social psychology (cf. Ochsner & Lieberman, 2001). This is not to disregard the enormous potential of neuroscience and its fascinating potential, which may actually explain why there is little need for deeper theoretical reflection. Making a cognitive correlate visible in fMRI, just as demonstrating a subliminal priming effect on overt action, are phenomena that speak for themselves, requiring no embedding theory to be admired.

One way to address this problem of theory neglect could be to study the creative process of theory construction and how theoretical thinking can be instigated and trained. That is the aim of this article. To preview, after presenting an evolutionary metaphor of theory formation, I introduce a distinction between two ways in which theories can influence and facilitate research, analogous to the different ways in which majorities and minorities are assumed to exert social influence in Moscovici's (1980) approach. Although compliance with scientific "majorities" is important for the institutionalization of science, genuine theoretical innovation requires the conversion processes that are typically solicited by "minorities" who militate against paradigmatic conventions. I describe and exemplify this creative process of theory conversion and the resistance experience that accompanies it. I argue that this creative process is not at all mysterious but can be learned and exercised using a number of game-like heuristics. As expected of contributors to this special issue, I illustrate these points from my own personal research perspective. Finally, some useful implications are derived for future theorizing, teaching requirements, and rules to be implemented in the reviewing process, and some visions on the future of social psychology are discussed.

The Creative Cycle: Loosening and Tightening

In his seminal work on personal constructs, George Kelly (1955) pointed out the notion of a creative cycle that encompasses two stages, loosening and tightening. By analogy to evolution theory, loosening corresponds to the production of random variation and new creatures through recombination of genes, whereas tightening corresponds to a subsequent selection stage that serves to separate the strong and weak products of the preceding loosening stage (cf. Fiedler, 1988). In scientific evolution, the tightening stage corresponds to theory tests, whereas the genesis or invention of new theories corresponds to loosening. Social psychologists, like other scientists, have developed a rich repertoire of tightening tools suitable for separating valid from invalid "exemplars." In contrast, neither the philosophy of science nor cognitive psychology have made serious attempts to describe the loosening stage. Yet both stages are equally important and indispensable. If either stage is blocked, there can be no scientific growth.

From this sketch of the creative cycle, some insights concerning effective theorizing become immediately apparent. Good scientific practice ought to support loosening and tightening. It should include both a strong generative component to facilitate productivity and a methodological component to warrant scrutiny and validity checks. This double goal entails a conflict between a nonconformist and a conformist tendency, namely, to break up with established theories (loosening) and to get a new theory established (tightening). Let us first elaborate on these different tendencies before we turn to the question of what it means to be productive in theoretical thinking and how this scientific skill can be acquired.

Two Ways of Exerting Theoretical Influence: Compliance and Conversion

Theoretical influences vary markedly in the extent to which they rely on loosening and tightening. Imposing a simple dichotomy, one might crudely distinguish two survival strategies. On one hand, unconventional, challenging theories that entail courageous loosening are typically met with strong resistance during the tightening stage. They have to overcome a particularly stringent tightening stage. On the other hand, the loosening may be kept to a rather modest, cautious level within conventional confines; such a more cautious strategy may then render the tightening task more lenient and less adversary.

These two influence strategies can be compared to the respective ways in which minorities and majorities exert social influence, according to Moscovici (1980). The former strategy, radical loosening plus strict tightening, corresponds to Moscovici's notion of *conversion*. For such innovations to succeed, they really have to be convincing. A sufficient number of researchers in the scientific community have to become converts to the new idea. If one witnesses the peer reviewing process of such contributions, the scrutiny and resistance is markedly enhanced. Not surprisingly, the few exemplars that survive such strict selection must be strong and persistent. In contrast, the second strategy corre-

sponds to Moscovici's *compliance*, which characterizes the way in which majorities exert their influence, through dominating consensual decisions, determining conventions, and defining decision rules.

Examples for Illustration

Just as the minority-group members who bring about conversion are typically disliked, many theoretical ideas of this kind are low in attractiveness, unwanted, and associated with affective and aesthetic aversion. With reference to my own research, a prime example is the notion of *statistical regression*, which has an enormous theoretical potential, offering parsimonious explanations of a whole array of phenomena. Statistical regression as an empirical law is universal, incontestable, and clearly defined by two parameters, the amount of noise and the unreliability and extremity of target events in an underlying distribution. Whenever the correlation between an input variable (e.g., an attitudinal disposition) and output variable (e.g., an attitude-dependent behavior) is less than perfect, they will regress on each other. High output variable values will be less high than expected and low values will be less low than expected. This erosion-like effect—to use a more understandable geographical metaphor—increases with the amount of noise ("climatic disturbance causing erosion") in the system, and it increases linearly with the extremity of stimulus objects in an underlying distribution.

Space permits no more than a superficial sketch of the enormous explanatory power of the regression concept (Fiedler, 1996; cf. Fiedler & Walther, 2004; Gigerenzer & Fiedler, 2004). As the degree of regression increases with decreasing reliability and unreliability is a direct function of paucity of information, differential regression affords a parsimonious account of various phenomena that share an amount of information, or sample size, as a crucial ingredient. For example, the mere thinking effect (Tesser, 1978), confirmation bias through lopsided information searches (Fiedler, Walther, & Nickel, 1999), ingroup-serving biases and outgroup-homogeneity effects (Fiedler, Kemmelmeier, & Freytag, 1999), group-polarization effects (Myers, 1978), and illusory correlations involving minority groups (Klauer & Meiser, 2000) can be explained parsimoniously as due to differential sample size. Moreover, regression can explain a number of biases in frequency memory (Fiedler, 2002; Fiedler & Armbruster, 1994; Sedlmeier, 1999), and it leads to many enlightening discoveries, such as Erev, Wallsten, and Budescu's (1994) ingenious demonstration that overconfidence can be turned into underconfidence within the same data array, if one regresses subjective on objective values rather than the other way around (as is customary in the pertinent paradigm).

However, in spite of this enormous power, regression does not have the taste of an attractive and popular concept. It has the smell of an artifact; regression is rarely accepted as a genuine psychological explanation. After all, the term is borrowed from statistics and methodology, as if it were just a failure to control a bias in design, although in fact regression is a major property of the stimulus environment (cf. Gigerenzer & Fiedler, 2004). Nevertheless, the ignorance and avoidance of the undesirable concept of regression has a long tradition (cf. Furby, 1973; Rulon, 1941). It does not have the advantage of many fashionable, plausible, and easily digested constructs. Regression is imported from outside psychology; it is abstract and defined in an algebraic notation; it offers the same explanation for seemingly different phenomena, which decreases plausibility; it is hard to teach and it seems to account (at first sight) only for unsystematic error, as opposed to systematic bias. For all these reasons, this extremely useful and powerful concept still has only a few friends. Regression accounts have a hard time being published in major journals. Their minority proponents not only have to explain a difficult, unfamiliar, and unwanted construct to their readers, but also have to turn reviewers into converts who give up more desirable accounts.

Let us now turn to typical examples of compliance-type theory developments. In strong contrast to conversion, these are big attractors and centers of common interest. A good deal of major research revolves around fundamental questions, which are basically conventional in nature. For example, countless studies have been conducted during the past few decades that aimed at clarifying whether, say, affect or cognition is primary, whether single or multiple systems have to be postulated, whether cognitive representations involve exemplars or feature abstraction, and whether behavior is automatic. A final empirical answer to these fundamental questions is impossible. Whether affect precedes cognition depends on arbitrary operational definitions, that is, whether we assign the fastest response components to the category of affect or cognition. Similarly, whether specific responses are classified as automatic depends on arbitrary operational definitions or cutoff points on a continuum. Similarly, if we find (e.g., using fMRI) that cognitive or affective reactions involve different areas or circuits in the brain, it is a matter of arbitrary convention to refer to different brain systems or to different functions of the same system. Yet, a whole industry of empirical research and countless grant proposals revolve around these conventional issues.

Here we encounter the domain of scientific majorities and their compliance-type influence. The majorities determine conventional (i.e., ideological) positions, and that exerts conformity pressure, making the tightening process much easier for compliance than for conversion influences. For example, I find the tighten-

125

ing process (i.e., selection or peer reviewing) that the modal contribution to (social cognitive) neuroscience has to undergo is typically rather lenient. Whether an expert in fMRI studies merely demonstrated correlates of psychological processes or solved a genuine theoretical problem is hardly ever noticed. Too impressive is the technological apparatus and too influential is the consensus among leading scientists that neuroscience is important to raise old-fashioned tightening issues (i.e., narrow-minded validity questions) that can only hinder the development of a new favorite area.

Polemics is indeed not intended here. I do not want to suggest at all that compliance-type theorizing is worthless or illusive. The neuroscience example is quite representative in showing that the compliance influence of leading majorities is often tied to methodological developments. These developments are extremely important for the professionalization of a discipline. The methods carrying majority research programs are important for the discipline's prestige gained in the public and in society, for the allocation of research grants, to get tenure positions, for academic socialization and teaching of research skills, for the accrual of well-established, often replicated laws, and for the common ground that lends a discipline its identity. The leading methodologies and paradigms are responsible for the reputation that contemporary psychology has gained in society, for the steadily increasing number of young students choosing psychology as their major, for the increasing investment in grant money, for leading textbooks and literature databases. Minority positions that can only be understood by a small subset of experts could hardly accomplish such progress and success.

Nevertheless, although there is no reason for degrading either side in the dialectic interplay of loosening and tightening, or conversion and compliance, one has to acknowledge the basic asymmetry that exists between the two types of influence. Ultimate innovations are brought about by minorities in an effortful conversion process. Construction of new theories takes place in those who are playing the minority part and try to cast the majority into doubt. I am not necessarily attributing this role to particular persons, as if it were a stable trait. The same person can of course play different parts at different times. But in any case, creative theorizing means not only to be creative in a vacuum but to play an undesirable role, to break up with norms and conventions, to criticize old, established theories, to come up with unfamiliar metaphors and notations, and to challenge fellow researchers, editors, and reviewers.

Theoretical Creativity Can Be Trained Through Distinct Games

Like all complex, higher order skills, such as language, sportive competitions, hunting, and fighting,

theorizing skills can be acquired and optimized through games. Parenthetically, survival in evolution also depends crucially on ritualized games (such as mating games) and playful competition (like young animals who train to fight without killing each other). It is my firm conviction—shared by the most distinguished scientists I have met—that strong theoretical progress only emerges in playful environments. To facilitate effective theorizing therefore means to encourage scientists to engage in pertinent games. Table 1 affords a summary of distinct theory games categorized, according to their major function, as loosening versus tightening games. In discussing these games, I refer to theorizing on constructive memory—an area in which I was myself involved. But I do not want to attribute anything to myself as a unique individual. Rather, it is my conviction that no special talent has to be assumed and that many people can learn to play these games, which provide an efficient training program in theory formation.

The first entry in the left column of Table 1 is what I have called the *pluralistic-competition game*. It highlights the desirability to engage as many researchers as possible in theory negotiation, rather than leaving theorizing up to a few privileged people. Every researcher is invited to join this game; the basic rule is that a theory must exist independently of its originator's opinion. Once a theory has been stated and its boundary conditions have been set, everyone has an equal right to derive implications and to evaluate the theory in the light of empirical evidence. Asking the originator what the theory really says (e.g., "ask Leon what dissonance theory would say") violates the central rule of this

Table 1. *Theory Games Supporting Loosening and Tightening*

Loosening Games	Tightening Games
Pluralistic-competition game: Engage as many researchers as possible in pluralistic negotiation on what theories really imply.	*Falsification and challenging game*: This game, which is very prominent in journal reviewing, focuses on discriminant and internal validity of empirical findings.
Distal-import game: Borrow metaphors and notations from epistemic sources as distant as possible, to maximize innovative variance.	*Method-development game*: New measurement tools and analytic techniques can be used to examine metric or qualitative implications of a theory.
Playing around with tools and methods: Generate or simulate predictable and unpredictable (random) outputs using the very methodological tools that govern certain paradigms. The output will be well adapted to the paradigmatic field.	*Uncovering pseudotheories*: Some theories turn out to be empty or circular on closer inspection. Detecting these pseudotheories is not always easy and obvious.

game. Only when theories are detached from individuals and many different researchers (working in different labs and driven by different motives) participate in a pluralistic endeavor can the full potential and the weaknesses of a theory be expected to unfold. Including many competitors in pluralistic theorizing helps to sharpen concepts and definitions and to develop tools for future competitions about the assets and deficits of a theory. Note again the analogy to biological evolution, to which unrestricted pluralism is essential. Restricting the mating game to but a few nearby exemplars of a species would lead to incest and degeneration. Needless to say, pluralistic competition can be classified as a loosening game.

Self-evident as the call for pluralism may appear, it is more the exception than the rule. The greatest part of current social psychology is compartmentalized into a few work groups who are responsible for theories in their own domain, with little intrusion into other theorists' domains and with the remaining researchers confined to applying, extending, and testing the boundary conditions of fashionable theories. Thus, the leading theoretical approaches in these days—such as implicit social cognition, stereotype threat, mortality salience, and the revival of social comparison theory—do not have to fear being confronted with nasty questions, logical scrutiny, and threatening alternative accounts. The price for this peaceful, compartmentalized coexistence and the lack of challenging competition is, unfortunately, that theory development may be retarded and intriguing questions may be never asked. Could it be that the activation view of priming is wrong and has to be replaced by a compound-cue approach (Ratcliff & McKoon, 1988)? Could mortality salience effects reflect something basically independent of mortality? Does the theorist have no problem when stereotype threat is turned into stereotype boost (Shih, Ambady, Richeson, Fujita, & Gray, 2002)? Why don't theorists feel dissonance if an opposite effect exists for almost every effect predicted by their theory: stereotype boost for stereotype threat; spotlight effect for unrealistic optimism; false consensus for personal uniqueness; or a contrast outcome yoked to almost every assimilation effect?

I believe that it is worthwhile to transgress against the peaceful coexistence rule and play the pluralism game. For some reason—perhaps because my very first experiment as a student was on Brehm's (1972) reactance theory—I have taken deviant viewpoints notoriously and competed with theory leaders since I was a student. This has not always been met with applause, but it has never been boring and has often been quite fruitful and motivating. In some cases my critique or alternative conception was premature in the first place, but the emerging debate forced me to become more precise and to spell out theoretical implications in a more pointed fashion. Examples that come to mind re-

fer to diverse topics and such areas as causal schemas, cognitive fallacies, the relation of language and cognition, illusory correlations, lie detection, hypothesis testing, and more recently the Implicit Association Test. However, the topic of this article is constructive memory and judgments.

The notion of constructive memory highlights that memory is not a passive store of copies of perceived information but an actively construed, permanently changing system in which traces of externally provided stimulus perceptions always interact with internally generated information. The most prominent evidence for constructive memory originated in experiments on eyewitness testimony (Loftus, 1979) in which eyewitnesses were led to report information that had not been included in the originally witnessed episode. For instance, when witnesses who saw a traffic accident were then asked if there was a stop sign when in fact there had been a yield sign, they later reported to have seen a stop sign. Apparently, answering the question about a stop sign required participants to build a stop sign into a transient memory representation, which was then confused with the original car accident.

The fascination with this new topic facilitated the uncritical distribution of Loftus' (1979) work that was first met with leniency. This lack of pluralistic competition rendered the constructive memory paradigm vulnerable to the first serious attack, which came from McCloskey and Zaragoza (1985) and which was so simple that one might wonder why it took more than 10 years to emerge. Accordingly, constructive memory intrusions may reflect nothing but guessing effects, or demand effects, based on experimenter suggestions. Thus, when the experimenter asks whether the mechanic in the video had a screwdriver, when in fact he had a hammer, a high proportion of participants reporting a screwdriver may just be guessing. Another 10 years of research on constructive memory were necessary to overcome this theoretical challenge and to substantiate genuine false memories that were resistant to McCloskey and Zaragoza's critique. Lack of pluralistic competition may have thus retarded progress of research on constructive memory by almost one decade.

The next loosening game in Table 1 is termed *distal-import*. Just as bastards or mixed-race exemplars are often superior to genetic products stemming from closer relatives, theorizing greatly profits from the import of distal rather than proximal ideas. To be sure, creative ideas are rarely completely new but almost always have their predecessors or analogies borrowed from other areas of knowledge. If the origin is too close, the product does not appear to be new, or it may even resemble plagiarism. However, if an idea, a model, or a metaphor is imported from a sufficiently distant epistemic origin, then the new theory appears innovative.

127

Part of the stagnation in constructive memory research was due to a limited repertoire of theoretical constructs, revolving around replacement and confusion of memory traces within a rather old-fashioned conception of human memory. Theoretical myopia, due to the failure to play the distal-import game, prevented researchers to notice the relevance of a number of other paradigms akin to constructive memory: the perseverance effect (Ross, Lepper, & Hubbard, 1975), innuendo effects (Wegner, Wenzlaff, Kerker, & Beattie, 1981), the hindsight bias (Hertwig, Gigerenzer, & Hoffrage, 1997), the impact of repetition on perceived validity (Begg, Armour, & Kerr, 1985), and the impact of imagination and explanation on reality beliefs (Koehler, 1991). All of these paradigms point out multiple ways in which people may come to believe in the truth of false or hypothetical information.

Gilbert, Krull, and Malone (1990) also offered a theoretical account of how people may come to believe in the truth of wrong propositions. This approach nicely illustrates the fresh impact that distal import from remote sources can have on theorizing. Gilbert and colleagues imported the notion of default truth values from an old Dutch philosopher, Baruch Spinoza (1632–1677). The origin of this idea is distal in terms of discipline (philosophy), time (17th century), and unfamiliarity to most fellow psychologists. The idea was that possible propositions cannot be represented in memory. As long as hypothetical propositions do not undergo a critical reality assessment, they have to be represented as facts; that is, their default truth value in memory is positive. Experimental evidence indeed showed that people who are distracted from a critical reality check though cognitive load will retain the default truth value and believe in false propositions, whereas undistracted people will engage in critical reality checks and finally discard incorrect propositions.

Again, one need not be born with mysterious talents to participate in the distal-import game. I dare to pretend that most researchers can participate and train the skill to seek remote theory metaphors. Indeed, we could try it out immediately, generating examples of remote theory input on the spot. Thus, let us just take another philosopher, preferably an older one, such as Schopenhauer (1788–1860). In his writings, we can find the seed for a modern theory of self-representation, namely, a self structured into three facets: where a person originates, what he or she possesses, and what he or she represents in public encounters. Alternatively, the term *distal* need not refer to distance in time and discipline space, but to distance in general. A highly general concept, like Einstein's relativity, can easily be used to create a psychological approach to relativity, highlighting the existence of different realities in persons having different vantage points and accounting for characteristic biases and perspective effects (Fiedler & Walther, 2004), much like in physics and astronomy. Also, almost any connectionist model imported from computer science comes with a guarantee of effective loosening.

Playing around with methodological toys is a very adaptive, fertile heuristic for loosening and theory creation, because it employs the same tools or instruments that are later used to validate theoretical implications in the tightening stage. One particularly productive game is signal detection analysis (cf. Swets, Dawes, & Monahan, 2000), a universal tool for distinguishing genuine cognitive discriminations from shallow response tendencies. The old research on constructive memory is characterized by a conspicuous neglect of this instrument, which is ideally tailored to diagnose the nature of memory intrusions. Whoever played around with this instrument on his or her computer, trying out different hit rates and false alarm rates and thinking of factors that might affect hits and false alarms differentially, will agree that numerous ideas come to mind of how to locate constructive memory in the theoretical space of genuine discrimination and response bias. Refined recent evidence on false memories draws heavily on signal detection analyses, pointing out that significant response bias and genuine memory changes can both be at work (Wixted & Stretch, 2000).

Within a connectionist framework, too, just playing around with a package of computer simulation routines can train researchers to commit themselves to well-articulated, algorithmic process theories, which can be implemented as computer programs. Such theories do speak for themselves; they do not require their originator's verbal comments. Moreover, playing with simulation tools will reveal fresh insights incidentally. With regard to constructive memory, the need to represent a possible proposition and a true proposition as distributed vectors of information will force one into recognizing how likely the two traces can be confused. That prompting memory with a possibility will often produce an echo that mimics a fact will appear quite normal after this "treatment." Imaginative simulation-model games loosen many restrictions in thinking about stereotypes or social cognition. Algorithms producing assimilation will produce contrast at the same time; just as heuristic and systematic processing can co-occur, positive and negative priming, facilitation and inhibition, and biased outcomes do not require biased cognitive or motivational processes (Fiedler, 1996).

Note that this haphazard way of creating new theoretical ideas incidentally, as a by-product of playful activities, is quite opposite to the planned derivation of theorems within axiomatic approaches, which I believe is often overrated. The methods-toy game, too, can be played deliberately and does not require any mysterious talents. It can be played right here. Let us take spectral analysis, a method quite common in physics, astrophysics, and biological science. Extending an ap-

128

proach suggested by Gilden (2001), we might arrive at an innovative theory that states that individuals as well as groups underlie periodic, system-immanent variations that can be much stronger in power than external treatment variance. Or, we may want to play around with the sequential testing tool (Wald, 1945) to arrive at an attractive random-walk model of consumer choice.

Although the games in the right column of Table 1 are primarily devoted to tightening, they nevertheless inform a feedback loop that instigates loosening and new theory creation. For example, the game of *trying to falsify and challenge theories* is amply played in the tightening process of peer reviewing. Participants not only sharpen their theoretical arguments and analytical instruments but, as a by-product, constantly generate new rival theories, if only to justify their critique. Peer reviewing and mutual open criticism among researchers is a major forum of idea production and a strong motivational force. Again, peer reviewing and critical exchange can be trained and played deliberately, affording a systematic way of theory production embedded in a motivating social game with specific rules that emphasize tightening but at the same time encourage notorious questioning and loosening.

Regarding constructive memory, I report the following episode to illustrate how rivalry in peer reviewing led us to invent a particularly robust demonstration of genuinely constructive effects. The aim of this investigation (Fiedler, Walther, Armbruster, Fay, & Naumann, 1996) was to rule out guessing and demand effects and at the same time to disconfirm Gilbert et al.'s (1990) aforementioned restriction that false propositions will only be upheld if participants are distracted from critical truth assessment. Driven by the motive to prove that the phenomenon is less restricted than sometimes assumed, we thought of an experimental design that could cogently demonstrate memory intrusions even when information had been critically assessed and correctly denied as false in the first place. Participants saw a video showing the interior of a flat. Right after the presentation of the original information, they were asked to verify whether they had seen a number of objects (e.g., furniture, clothes) that had been present or absent in the flat. Most absent objects were correctly denied in response to this questioning treatment. Nevertheless, the very experience of having answered questions about nonexistent objects increased the likelihood of remembering these objects in a delayed recall or recognition test. Methodologically, a novel aspect of our experiments was that we analyzed memory contingent on the outcome of critical assessment. Even when analyses were confined to those items that had been correctly denied immediately after presentation, the proportion of intrusion was significant and almost as high as in the overall analysis. This finding showed that memory intrusions can survive

critical reality checks and point in the direction opposite to explicit guessing responses. Thus, rather than arising from shallow processes during encoding, deeply considering a nonexistent object for a moment, if only to deny its presentation accurately, nevertheless resulted in false memories. Had Loftus (1979) been forced through a strict reviewing process to present the same contingent analysis—showing that many witnesses who correctly deny having seen a stop sign will nevertheless remember a stop sign on later occasions—her theory might have been much less vulnerable to the critique of McCloskey and Zaragoza (1985) and others.

The *method-implementation game* is primarily aimed at setting new methodological standards for measurement devices, data-analysis techniques, tests, or technical equipment. Although clearly a tightening game, it often assists in discovering new theoretical problems as, for example, when the construction of a personality test reveals that a trait is not one-dimensional or when standards for representative sampling cast the external validity of major phenomena into doubt. Although Juslin (1994) and others have shown that cognitive biases can reflect researchers' tendency to select overly difficult and tricky stimuli and tasks, constructive memory effects might be due to using stimulus objects that are of little personal interest to participants (e.g., an umbrella stand in a flat). However, given that false memory intrusions can be produced with stimulus events as significant and involving as sexual abuse (cf. Wells, 1993), this challenge will soon be discarded.

The last game included in Table 1 is the one that, in my opinion, has been mostly neglected. I refer to it as *uncovering pseudotheories*. Even in expert peer reviewing, this is often overlooked; I believe it ought to possess a central place. It is not concerned with the validity of empirical findings but with the form and logical structure of theories. Many theories in contemporary social psychology, especially some of the most popular ones, can be criticized as lying at the edge of tautology in that they cannot really be falsified. For instance, the attribution-theoretical implication that student success on a difficult task is attributed to high ability can hardly be falsified, because the very concept of difficulty is defined as the property of a task that can only be solved by people with high ability. If experimental participants happened to "falsify the theory," attributing success on a very difficult task to something different from high ability, this would merely question the participant's language ability, but not the theory (Brandtstäedter, 1982). In a similar vein, I wonder how it is possible to falsify theoretical statements such as mood is used as information (how could it not provide information?), the theory of reasoned action predicts reasoned action (excluding all nonreasoned behaviors; Greve, 2001), and implicit attitude measures capture

129

FIEDLER

hidden implicit attitudes (however weak they relate to other measures).

Sensitizing theorists and reviewers to the problem of tautology or empty theories is a demanding task for current and future (social) psychology, even more difficult to overcome than the paucity of theoretical innovation or lack of crucial validity checks. However, note that the tricky game of uncovering pseudotheories is not confined to tautology tests. Its domain is much broader and often very subtle, calling for a specific terminology and metatheoretical notation, and for collective efforts. Single fighters alone will hardly convince the community, for instance, that there is no logical ground for assuming that factors that have facilitated the evolution over thousands of generations are also functional within the present generation. Collective activities are sorely needed to eliminate many pseudotheoretical elements that originate in the confusion of implication direction $p \rightarrow q$ does not mean $q \rightarrow p$, the failure to recognize the asymmetry of universal laws and existence proofs, the different use of the same technical terms in different contexts, and violations of syllogistic rules.

As our final illustration, let us again refer to an example involving constructive cognition. In theorizing on the so-called confirmation bias (Snyder, 1984; Zuckerman, Knee, Hodgins, & Myake, 1995), almost everyone (students like *Journal of Personality and Social Psychology* reviewers) agrees that two assumptions are logically sufficient to explain the tendency to subjectively verify a hypothesis in the absence of sound empirical evidence. First, when experimental participants are asked to test the hypothesis that their interview partner is extraverted, rather than introverted, they typically engage in positive testing; that is, they ask more questions referring to extraverted behaviors (going to parties, telling jokes) than introverted behaviors (reading a book, being alone at home). Second, interviewees tend to provide more yes than no responses, a tendency called acquiescence. Positive testing and acquiescence together (that is, the tendency to ask many extraverted questions and the tendency to provide many yes responses) seem to explain (or rationalize) the belief that the interview partner is actually an extravert.

Or does it? Logically, two skewed base rates (mostly extraverted questions, mostly yes responses) do not make a contingency. Given both base rates (i.e., positive testing and acquiescence), the relative rate of yes responses could be higher for extravert questions, higher for introverted questions, or equal for both question types (cf. Fiedler, Walther, et al., 1999). In any case, the syllogistic conclusion "This person is likely an extravert" does not follow from the premises that "This person was asked many extravert questions" and "This person provided mostly extraverted responses to all questions."

Games Played by Majorities and Minorities, and By Myself

Theoretical contributions by majorities and minorities, that is, influences of the conversion and compliance type, are typically characterized by different profiles or weights given to the theory-formation games described previously (Table 1). In the absence of systematic data, I suspect that conversion-type changes are facilitated to the extent that theorists play the pluralistic competition game, the falsification and challenging game, and the distal-import game, and that they play excessively with method toys. Conversely, theoretical implementations of the compliance type seem to involve these games to a lesser degree; they seem to particularly avoid the pseudotheories game. Compliance-based paradigms often place a strong emphasis on the methods-development (i.e., standard-setting) game, and of course they provide the most input to the falsification and challenging game played in peer reviewing.

Being asked to reveal my own perspective and experience within the framework depicted so far, I do not hesitate a moment to admit that my ideal self wants to locate myself on the "wicked side," in the minority party. Both my own personal motivation as a social psychologist as well as my appraisal of the current historical situation of our discipline tell me that more conversion is sorely needed to overcome some old compliance states. Moreover, as emphasized at the outset, this orientation involves enhanced occupation with theory and metatheory, with criteria of sound validation, conceptual precision, search for conflicts and debates, open negotiation of theory constraints, and getting rid of taboos to touch holy compliance territories. Assuming that fertile psychological theorizing calls for such goals and that past theorizing has neglected them, I can express frankly my preference, without depreciating the other position.

However, in reality, perhaps as a function of getting older and taking over more and more senior academic functions, I have to admit that an increasing proportion of my everyday involvement in peer reviewing, research planning, supervising, and shaping of my own articles for publication is of the compliance type. Indeed, it matters little at the individual level what game one is involved in—provided one has learned and felt the entire repertoire of loosening and tightening games. I believe it is no problem if individual scientists play only the games that best fit their personal mentality or their current role in the academic endeavor. Other scientists will compensate for those parts that one person cannot play alone. However, such compensation is not possible at the collective level, I'm afraid. Our discipline as a whole is in a state, I repeat, in which theory mindedness has been largely sacrificed for industrious empirical research, and theoretical reflection and scru-

130

tiny is now called for. Tightening games alone will hardly solve this developmental task. Just like all innovations call for minorities to bring about new perspectives in a tidy conversion process, I believe it is time for contemporary social psychology to support and implement loosening games in its curricula, in funding policy, in peer reviewing, and in the relationship between senior and junior scientists.

References

Begg, I., Armour, V., & Kerr, T. (1985). On believing what we remember. *Canadian Journal of Behavioral Science, 17,* 199–214.

Brandtstädter, J. (1982). Apriorische elemente in psychologischen forschungs-programmen (A priori elements in psychological research programs). *Zeitschrift für Sozialpsychologie, 13,* 267–277.

Brehm, J. W. (1972). *Responses to loss of freedom: A theory of psychological reactance.* Morristown, NJ: General Learning.

Erev, I., Wallsten, T. S., & Budescu, D. V. (1994). Simultaneous over- and underconfidence: The role of error in judgment processes. *Psychological Review, 101,* 519–527.

Fiedler, K. (1988). Emotional mood, cognitive style, and behavior regulation. In K. Fiedler & J. P. Forgas (Eds.), *Affect, cognition, and social behavior* (pp. 100–119). Toronto: Hogrefe.

Fiedler, K. (1996). Explaining and simulating judgment biases as an aggregation phenomenon in probabilistic, multiple-cue environments. *Psychological Review, 103,* 193–214.

Fiedler, K. (2002). Frequency judgments and retrieval structures: Spliting, zooming and merging the units of the empirical world. In P. Sedlmeier & T. Betsch (Eds.), *Frequency processing and cognition* (pp. 67-87). New York: Oxford University Press.

Fiedler, K., & Armbruster, T. (1994). Two halfs may be more than one whole: Category-split effects on frequency illusions. *Journal of Personality and Social Psychology, 66,* 633–645.

Fiedler, K., Kemmelmeier, M., & Freytag, P. (1999). Explaining asymmetric intergroup judgments through differential aggregation: Computer simulations and some new evidence. *European Review of Social Psychology, 10,* 1–40.

Fiedler, K., & Walther, E. (2004). *Stereotyping as inductive hypothesis testing.* Hove: Psychology Press.

Fiedler, K., Walther, E., Armbruster, T., Fay, D., & Naumann, U. (1996). Do you *really* know what you have seen? Intrusion errors and presuppositions effects on constructive memory. *Journal of Experimental Social Psychology, 32,* 484–511.

Fiedler, K., Walther, E., & Nickel, S. (1999). The autoverification of social hypothesis. Stereotyping and the power of sample size. *Journal of Personality and Social Psychology, 77,* 5–18.

Furby, L. (1973). Interpreting regression toward the mean in developmental research. *Developmental Psychology, 8,* 172–179.

Gigerenzer, G., & Fiedler, K. (2004). *When research neglects ecological structure.* Manuscript submitted for publication.

Gilbert, D. T., Krull, D. S., & Malone, P. S. (1990). Unbelieving the unbelievable: Some problems in the rejection of false information. *Journal of Personality and Social Psychology, 59,* 601–613.

Gilden, D. L. (2001). Cognitive emissions of 1/f noise. *Psychological Review, 108,* 33–56.

Greenwald, A. G., McGhee, D. E., & Schwartz, J. L. K. (1998). Measuring individual differences in implicit cognition: The implicit association test. *Journal of Personality and Social Psychology, 74,* 1464–1480.

Greve, W. (2001). Traps and gaps in action explanation: Theoretical problems of a psychology of human action. *Psychological Review, 108,* 435–451.

Hertwig, R., Gigerenzer, G., & Hoffrage, U. (1997). The reiteration effect in hindsight bias. *Psychological Review, 104,* 194–202.

Juslin, P. (1994). The overconfidence phenomenon as a consequence of informal experimenter-guided selection of almanac items. *Organizational Behavior and Human Decision Processes, 57,* 226–246.

Kelly, G. (1955). *The psychology of personal constructs.* New York: Norton.

Klauer, K. C., & Meiser, T. (2000). A source-monitoring analyses of illusory correlations. *Personality and Social Psychology Bulletin, 26,* 1074–1093.

Koehler, D. J. (1991). Explanation, imagination, and confidence in judgment. *Psychological Bulletin, 110,* 499–519.

Loftus, E. F. (1979). *Eyewitness testimony.* Cambridge, MA: Harvard University Press.

McCloskey, M., & Zaragoza, M. (1985). Misleading postevent information and memory for events: Arguments and evidence against memory impairment hypotheses. *Journal of Experimental Psychology: General, 114,* 1–16.

Moscovici, S. (1980). Toward a theory of conversion behavior. In L. Berkowitz (Ed.), *Advances in Experimental Social Psychology* (Vol. 13, pp. 208–239). New York: Academic.

Myers, D. G. (1978). Polarization effects of social comparison. *Journal of Experimental Social Psychology, 14,* 554–563.

Ochsner, K. N., & Lieberman, M. D. (2001). The emergence of social cognitive neuroscience. *American Psychologist, 56,* 717–734.

Ratcliff, R., & McKoon, G. (1988). A retrieval theory of priming in memory. *Psychological Review, 95,* 395–408.

Ross, L., Lepper, M. R., & Hubbard, M. (1975). Perseverance in self-perception and social perception: Biased attribution processes in the debriefing paradigm. *Journal of Personality and Social Psychology, 32,* 880–892.

Rulon, P. J. (1941). Problems of regression. *Harvard Educational Review, 11,* 213–223.

Sedlmeier, P. (1999). *Improving statistical reasoning: Theoretical models and practical implications.* Mahwah, NJ: Lawrence Erlbaum Associates, Inc.

Shih, M., Ambady, N., Richeson, J. A., Fujita, K., & Gray, H. M. (2002). Stereotype performance boosts: The impact of self-relevance and the manner of stereotype activation. *Journal of Personality and Social Psychology, 83,* 638–647.

Snyder, M. (1984). When belief creates reality. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 18, pp. 247–305). New York: Academic.

Swets, J., Dawes, R. M., & Monahan, J. (2000). Psychological science can improve diagnostic decisions. *Psychological Science in the Public Interest, 1,* Whole No. 1.

Tesser, A. (1978). Self-generated attitude change. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 11, pp. 181–227). San Diego: Academic.

Wald, A. (1945). Sequential tests of statistical hypotheses. *Annuals of Mathematical Statistics, 16,* 117–186.

Wegner, D. M., Wenzlaff, R., Kerker, R. M., & Beattie, A. E. (1981). Incrimination through innuendo: Can media questions become public answers. *Journal of Personality and Social Psychology, 40,* 822–832.

Wells, G. L. (1993). What do we know about eyewitness identification? *American Psychologist, 48,* 553–571.

Wixted, J. T., & Stretch, V. (2000). The case against a criterion shift account of false memory. *Psychological Review, 107,* 368–376.

Zuckerman, M., Knee, C. R., Hodgins, H. S., & Myake, K. (1995). Hypothesis confirmation: The joint effect of positive test strategy and acquiescence response set. *Journal of Personality and Social Psychology, 68,* 52–60.

131