Agenda

Connecting the dots: Mobilizing theory to reveal the big picture in social psychology (and why we should do this)

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Abstract

During the past 20 years, practices in social psychology have drifted toward the publication of brief research reports as the main outlet for empirical findings, resulting in an exponential increase of the number of publications in our field. Recent developments questioning the reliability of these findings have increased the focus on (methodological) details and have prompted efforts to establish the robustness of isolated phenomena. Both types of developments carry the danger of impeding rather than promoting progress in the field. We can only build a cumulative knowledge base when we succeed in connecting these dots. Developing and examining broader theories about psychological processes and their implications can help connect different insights and elucidate their further implications in a way that can be used and understood within and beyond the boundaries of our discipline.

The Dutch psychologist Piet Vroon (1939–1998) used to invoke the metaphor of an “exploded confetti factory” to characterize the results of psychological research: Many colorful bits of paper freely floating in the air. During the past years, journal practices in social psychology have drifted toward publication of brief research reports as the primary way of communicating novel findings. This has greatly increased the amount and diversity of “things we know” about social psychological phenomena and the ways in which they can occur. However, in and of itself, this accumulation of research findings does not necessarily build a cumulative knowledge base in social psychology. On the contrary, the sheer number of isolated findings has made it more difficult to understand whether and how these relate to each other or extend existing insights. All the “things we know” can only contribute to social psychology when they are connected to broader theoretical perspectives and integrated with existing insights.

RECENT DEVELOPMENTS IN SOCIAL PSYCHOLOGY

During the past years, societal developments demanding greater efficiency of publicly funded organizations and requesting transparency of results have also impacted on the way social psychologists conduct and publish their research. Increasingly, funding agencies—even if their mission is to promote basic science—take into account the potential for “valorization” of emerging insights when evaluating research proposals. Likewise, universities and scientific journals employ press officers, whose job is to look out for studies that can attract media attention (see also Strack, 2012). Of course, it is important that social psychologists (like all scientists) are aware of the applied implications of their work and make an effort to elucidate these for the general public. However, as resources for research are scarce—and have been further reduced during recent years—it is inevitable that these incentives impact on the direction of research and publication efforts, even when researchers would prefer to pursue issues that are driven by more basic scientific questions. Because, as the work motivation expert Kerr (1975) noted some time ago, it is folly to reward people for A while hoping that they will do B.

As a result, the scientific waters have been muddied by the pressure to do work with this type of impact. The desire to demonstrate counterintuitive, “sexy,” or otherwise provocative findings as attention grabbers for the general public has elicited a stream of quick studies in which theoretical meaning and methodological precision seemed less important. Likewise, research prompted by the desire to provide immediate and concrete solutions to pressing political or societal problems more often than not has yielded highly specific case solutions, generating insights that cannot be transferred to other cases or issues. Thus, from a scientific point of view, there is reason to be critical of this type of knowledge acquisition, even when it clearly pays off in terms of public attention or fund raising.

These developments have become an important subject of discussion and debate about current research practices in social psychology, which have risen to an unprecedented level during the past year. Partly fuelled by several highly publicized cases of extensive fraud and non-replication of prior results,

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broader concerns about the incidence of “sloppy science” have induced considerable soul searching and reconsideration of appropriate standards for doing science in our discipline (Levett, 2012; Stroebe, Postmes, & Spears, 2012). These developments too have raised the question of how to create cumulative knowledge, in this case narrowing down on the question whether methodological guidelines can increase the robustness of results from specific studies. But in the zeal to avoid “sloppy science,” the field is at a risk of moving to the opposite extreme, focusing on methodological concerns alone. Examining whether and how specific findings relate to a broader theoretical perspective provides an alternative way to assess their plausibility and added value.

As researchers, our only valid anchor and drive are the intrinsic motivation to understand certain phenomena and our persistence in examining issues of interest (see also Mummendey, 2012). Focusing on the process of gaining insight instead of the goal of publishing papers will help us do the right things for the right reasons, despite recent developments (see also Van Yperen, Hamstra, & Van der Klaauw, 2012). In the remainder of this contribution, I will argue for a social psychology that develops and progresses through theoretical and integrative work as much as through original research. Regaining a focus on the big picture—instead of continuing to accumulate details—is the only way forward.

CHANGING PUBLICATION PRACTICES

Over the years, publication practices in social psychology have changed substantially. The increasing adoption of neurocognitive and biological methodologies and measures has shifted the ambitions of social psychologists to publish their work in journals that were previously the exclusive domain of cognitive or neuroscientists (e.g., Neuron), or in high impact general science journals (e.g., Science). Perhaps inspired by the publication formats that are typical of other domains of science, in parallel to this development, findings from social psychological research nowadays tend to be published in short papers reporting a single study or documenting a single phenomenon (see also Ledgerwood & Sherman, 2012; Strack, 2012). Almost every self-respecting journal in social psychology has a section with “short notes,” “flash reports,” or “fast track reports,” and some journals are entirely devoted to this type of publication (e.g., Psychological Science and Social Psychological and Personality Science).

Obviously, this shift in publication practices has offered relief for researchers who feel frustrated by journal editors who expect each manuscript to contain multiple studies, seemingly endless rounds of reviews, repeated requests for additional data, and long publication delays. Shorter papers and quicker turnaround times can make a crucial difference, especially for junior researchers at the beginning of their scientific career. Having those first publications come in on time (or not) determines their eligibility for academic positions and funding opportunities. But in view of the complex and context dependent nature of the issues we study in social psychology, there might also be important drawbacks to these developments.

To quantify the developments outlined above, the database of the Center for Science and Technology Studies in Leiden, the Netherlands (CWTS), was used to examine changes in publication behavior in social psychology between 1991 and 2011 and to detect developments in citation practices during this period (Van Leeuwen, 2013). This database contains bibliometric characteristics of all journal publications incorporated in Web of Science and is considered a complete and objective record of the journal literature, which can be used to evaluate social science research (Van Leeuwen, 2006).

The analysis of journal publications in social psychology reveals a clear pattern, indicating substantial shifts in publication practices between 1991 and 2010. In this period of 20 years, the number of journals covering the field of social psychology has increased by about 50%, from about 40 to about 60. The number of short papers—containing less than 10 printed pages—has doubled from 600 to 1200, with a dramatic increase from 2006 onward. In 2010, 40% of all papers published contained 10 printed pages or less. By comparison, the number of review articles is quite small and has remained at a constant level during the past 20 years, with 40 to 60 publications per year. Thus, review papers consistently make up only 1–4% of all Web of Science publications.

The net result of these developments is a dramatic increase over the past two decades in the number of journal articles in social psychology appearing each year (Van Leeuwen, 2013). In 1991, the total number of journal articles in social psychology was 1700; in 2011, this had almost doubled to 3200. Although researchers from the USA provide the largest contribution to the social psychological literature the proportion of journal publications by non-US scholars increased from 34% in 1991 to 46% in 2010.

The sheer number of publications that accumulate in this way raises the question of how individual researchers or research groups can possibly incorporate and connect all this knowledge. An answer may be found—at least in part—by examining how literature references (also contained in the CWTS-Web of Science database) have evolved during these same 20 years. This reveals that the number of references cited in each publication has slightly increased (from 36 in 1991 to 50 in 2011). Nevertheless, it is important to note that this by no means reflects the rate at which the number of social psychology publications available has increased.

At the same time, the proportion of references in each list made to other publications in Web of Science has grown from 55% of all references made in 1991 to 59% of all references in 2001 and to 68% of all references in 2011, which does not reflect the rate at which the number of journals covered in the Web of Science data base increased during those periods (Van Leeuwen, 2013). This suggests that published papers increasingly refer to other (empirical) journal articles rather than connecting the findings they report to broader overviews provided in books or book chapters.

Finally, the distribution of “reference histories” has remained the same. Typically, about 50% of all citations in a reference list refer to work published less than five years before, with the remaining 50% of the work cited being published between 5 and 10 years before; the same distribution is visible in 1991, in 2001, and in 2011. This suggests a “moving window” of knowledge cited instead of cumulative
knowledge base, as each author only connects their work to findings published in the 5 to 10 years preceding their own research.

Together, these developments paint a slightly disturbing picture of the field, bringing to mind the metaphor of the confetti factory as a tempting description. Indeed, as indicated above, during the past 20 years, social psychologists have not only published many more papers than before, but these publications also contain an increasing proportion of (very) short papers. By definition, this implies that less ground can be covered in each single publication. At the same time, citation patterns display a decreased proportion of references to non-journal publications and a pervasive tendency to connect novel findings only to relatively recent work. Thus, despite the ever-increasing number of potentially relevant insights that have amassed during the past 20 years, there is less evidence of connections being made or of cumulative insights that are built in this way. Indeed, from this point of view, social psychology mainly seems to be amassing dots—as more and more colorful but isolated effects are documented—without necessarily making clearer the overall picture (see also Ledgerwood & Sherman, 2012; Strack, 2012).

**ON METHODOLOGICAL FETISHISM**

The famous quote ascribed to Benjamin Disraeli, “There are three kinds of lies: Lies, damned lies, and statistics,” seems to have acquired new meaning during the past year. Social psychology has been the focal point of several—sometimes very public—discussions on research practices, publication habits, procedures for data analysis, and the appropriate use of statistics. In an open letter, Daniel Kahneman expressed regret about the lack of systematic replication attempts in social priming of behavior that make the field vulnerable to criticism about reliability of findings (see *Nature*, doi: 10.1038/nature.2012.11535). Several researchers have indicated their concern about “false-positive” psychology because of the tendency to only report observations that are “statistically significant” (Bakker, Van Dijk, & Wicherts, 2012; Ferguson & Heene, 2012; Simmons, Nelson, & Simonsohn, 2011). Finally, it has been observed that in social psychology, there tends to be less correspondence of data collected in the laboratory to data collected in the field than in other subfields of psychology (Mitchell, 2012).

The field has clearly taken to heart these concerns, as evidenced by contributions in the *European Bulletin of Social Psychology* (Mummenedey, 2012; Strack, 2012), the Newsletter of the Society for Personality and Social Psychology (*Dialogue*, volume 26, no. 2), and a special issue on replicability and research practices of *Perspectives on Psychological Science* (Volume 7, Issue 6, publicly available at http://pps.sagepub.com/content/7/6.toc). In addition, concrete initiatives have been developed to address the “file drawer problem” (see http://psychfiledrawer.org/) to organize and publicize replication studies (see http://openscienceframework.org/project/VMRGu/node/rd5tS/) and to find practical ways to report the observation of non-significant results (Simmons, Nelson, & Simonsohn, 2012).

Nevertheless, the prevailing sentiment remains that there is a steadily accumulating catalogue of sloppy research practices, bemoaned by researchers in social psychology as well as outside. The (perhaps understandable) response in social psychology has been that we should focus on improving the quality of procedures for the collection, storage, and analysis of data, double-check and share information regarding our research procedures, avoid making broad claims that go beyond the actual observations we made, and always try to be as precise and specific as we can in doing this. Indeed, adding rules, checks, and sanctions is a standard response displayed by organizations that try to cope with errors that have been made (Koppell, 2005).

However, empirical research has revealed that this is not necessarily the best way to learn from previous problems or to improve collective performance (Van Dyck, Frese, Baer, & Sonnentag, 2005). Measures focusing on rules, moral obligations, and sanctions emphasize situational demands instead of coping resources, thus raising the experience of threat (Does, Derks, Ellemers, & Scheepers, 2012). Reward systems in which collective outcomes depend on the worst-performing individual elicit a focus on prevention (Faddegan, Ellemers, & Scheepers, 2009), which is associated with a pre-occupation with details—instead of big pictures—in attention and information processing ( Förster & Dannenberg, 2010; Förster & Higgins, 2005). Indeed, the predominance of (valid) concerns about ways to prevent fraud, the desire to be more certain about our findings, and the ambition to find ways to convince others (or even ourselves) that our work is not deficient have all resulted in a greater focus on *proper procedures* in research and publication.

Yet, as has been noted before (Haslam & McGarty, 2001), efforts to improve methodological and statistical procedures only target one of many forms of uncertainty that have to be addressed and managed in the process of developing scientific understanding. Thus, as a side effect of recent developments, the current tendency in the field is to focus on the development and adoption of procedures and measures that aim to reassure ourselves and others that we can rely on the (empirical) answers we find. In the terms proposed by Fiedler (2004), the current focus on “tightening” research procedures makes “loosening” work—in which “playing around” with tools and methods is an important strategy to create new theoretical ideas—seem suspect and less scientifically sound. Nevertheless, both types of efforts are needed for creative thinking and progression of science (Fiedler, 2004; Medin, 2012). Thus, the danger in focusing on the achievement of methodological precision is that we forget to consider critically whether we have actually examined the issues at hand in the best possible way, or whether we even are asking the appropriate questions. To the extent that this is the case, we are at risk of becoming methodological fetishists, who measure quality of science by the use of “proper” research procedures, not by the progress we make in understanding human behavior (see also Tajfel, 1981). This would reverse the means and the ends of doing research and stifles the creativity that is essential to the advancement of science (see also Haslam & McGarty, 2001; Mummenedey, 2012; Strack, 2012).

Similar concerns apply to the successful introduction of neuroscientific and psychophysiological methods and measures...
in social psychology. The great enthusiasm for these methods among social psychologists has led some to equate the experimental procedures that can be used to study specific mechanisms with the phenomenon of interest. A case in point is the highly successful “trolley dilemma” that was devised to examine moral decision making with fMRI measures (e.g., Greene, Nystrom, Engell, Darley, & Cohen, 2004). Obviously, there is more to know about moral decision making and morality in general than can be captured with this particular research procedure, and limiting our investigations to the issues that can be examined in this way does not do justice to the range of moral concerns that deserve further examination.

Generating knowledge that advances the field requires that we go beyond such methodological fetishism: Using the “right” experimental paradigms and statistical procedures should not become an end in itself. Indeed, there are plenty of examples of studies that have become classics in the field and still influence our understanding of key phenomena, even if the methodology they used is deficient by contemporary standards (Smith & Haslam, 2012). Even today, it is quite possible to do cutting edge work by employing “low tech” but ingenious solutions to study cognitive processes without any fancy equipment (e.g., Zdrale & Enge, 2013).

METHODOLOGICAL PLURALISM AND TRIANGULATION

It is important to keep in mind that each method—regardless of its sophistication and cleverness—is just that: a tool that has its own advantages and limitations—not an end in itself (see also Spears & Smith, 2001). This is why there is great value in methodological pluralism, as the use of multiple paradigms, methods, and measures allows for the weaknesses of one method to be compensated by the strengths of another (see also Hewstone, 1997; Strack & Werth, 2006). Although the current debate argues for the importance of replication studies, triangulation may provide an alternative—and perhaps preferable—way to assess the robustness and broader implications of specific findings. Examining the same phenomenon from different angles by combining different research approaches and types of measures offers the potential to develop and refine our understanding of important problems (see also Reis & Judd, 2013). This requires that we more directly connect highly rigorous (and for some reductionist) experimentation (that optimizes internal validity) with more “messy” and ambiguous methods or field observations (to establish external validity; see also Mook, 1983).

Going beyond the consideration of certain methodologies as inherently superior or most appropriate also makes it possible to examine whether we are even asking the “right” questions. To some extent, this is a political issue (see also Haslam & McGarty, 2001), as what are considered key questions or main-stream approaches depends as much on power differences in academia and control over resources for doing research as on scientific merit. Priorities of academic policy makers and funding agencies color our impressions of what are important questions, and personal values and priorities of individual academics guide choices made in basic research. To the extent that the individuals who make these decisions are not representative of the general public or the academic community at large, this may result in severe biases and neglected issues.

For instance, across different areas of science, there is by now convincing evidence that the unequal gender representation among scientific researchers has resulted in an incomplete understanding of key issues, sometimes with life-and-death consequences (Schiebinger, Klige, Sanchez de Madariaga, & Schraudner, 2011). Likewise, the highly skewed distribution in political orientations among academic researchers has raised the question whether there is enough diversity of perspective in the field to be able to develop the range of questions we need to examine (Haidt, 2008). Indeed, even though we know from social psychology that creativity and innovation benefit from involving people with diverse viewpoints, interests, and values (e.g., Jetten & Hornsey, 2010; Paulus, 2000; Moscovici, 1980; Nemeth, Personnaz, Personnaz, & Concalo, 2004), this is something we do not systematically practice in the way we develop research groups or research questions.

In sum, it seems that recent developments have increased research efforts toward the improvement of methodological precision and statistical rigor. Although this is an important development that may increase the reliability of specific conclusions, it is not necessarily the best way to connect these isolated conclusions or to obtain novel insights. Indeed, Strack (2012) has pointed out that research methods and statistics merely represent the rhetoric we use to persuade others of the validity of our observations (see also Billig, 1996). From this perspective, the publication of methodologically valid results does not constitute an end point or final goal. Rather than focusing on the empirical phenomena as objects of interest in and of themselves, these constitute a starting point for further discussion, which addresses the processes’ underlying specific observations, the theoretical implications this has, and how this may inform other types of issues or situations.

SHORT AND SWEET OR SUSTAINABLE KNOWLEDGE

The developments outlined above have induced a pendulum swing from quick and perhaps sometimes less clean work toward greater methodological precision. However, social psychology now runs the risk of moving to the other extreme: becoming a discipline that is known for highly rigorous but not always very meaningful research (see also Medin, 2012). The supreme value that is now attached to studies reporting clearly predicted, tightly controlled, and easily replicated results rewards the demonstration of undisputed and perhaps obvious simple effects, in small research designs.

The movement toward larger numbers of shorter papers makes it more difficult to connect these findings or to verify their broader implications. Social psychology is an empirical science, and we make sense of complex phenomena by isolating specific processes for closer examination. But progress can only be made when we move on from single process and effect studies, to considerations of the more complex interplay.
between different concerns (Petty, 1997), the relational contexts that may be relevant (Reis, 2006), and the different levels at which these play a role (Dovidio & Gaertner, 2006). Those who limit themselves to studying only the issues that can be examined as simple causal sequences run the risk of forfeiting the more challenging questions that made them interested in the discipline in the first place. There is a range of real-life concerns we routinely refer to when explaining the focal issues in our discipline for the general public or to motivate the investment of tax payers’ money in our research programs. These include the pervasiveness of discrimination, the development and resolution of intergroup conflict, or the tendency toward suboptimal decision making. A true understanding of these issues requires that we go beyond single study observations, to assess the context-dependence of established findings, explore potential moderations, and examine the combined effect of different variables in more complex research designs, even if this is a difficult and uncertain strategy. Although we tend to test specific predictions that can be deduced from existing theory, further theory development is only possible through retroductive interpretations of unexpected observations (Spears, 2012) or by reconciling divergent findings in more sophisticated models through inductive reasoning (Trope, 2004). Indeed, the type of knowledge acquired in social psychology typically represents a “temporary given” (Zanna, 2004): knowledge that is good enough to help generate new ideas and implications even when we know it is unlikely to provide final or definitive answers.

The tendency in the field to focus on two-cell comparisons in flash reports—extending the level of complexity to two-by-two designs in longer papers—offers a maximum level of predictability and control. However, many questions essential to the social psychological approach cannot be resolved in this way. Simple “either/or” answers may offer a first “proof of principle” for basic mechanisms but should also be the starting point for further examinations of the situations in which these are most likely to emerge, which contextual variables determine this, and what are further consequences when this happens.

Some of the “grand theories” that were originally developed more than 50 years ago still are influential in the field because they offer just that. Relevant examples are Heider’s (1958) attribution theory, Festinger’s social comparison (1954) and cognitive dissonance (1957) theories, Tajfel’s social identity theory (1974), or Allport’s theory of prejudice (1954). These founding fathers of social psychology are still considered influential thinkers who continue to inspire new generations of researchers (e.g., Postmes & Branscombe, 2010; Dovidio, Glick, & Rudman, 2005). They have provided us with sustainable knowledge, even if (by today’s standards) their early experiments were clumsy or methodologically limited.

**BUILDING BROADER THEORIES**

In social psychology, there are various approaches to the formation of theory. Some scholars propose very broad and abstract analytical frameworks, aiming to provide one theory to explain “everything” (like the standard model of elementary particles in physics, see Oerter, 2006). Other theorists offer more modest descriptions of specific mechanisms, sometimes in the form of a single hypothesis. Examples of both these extremes as well as “middle range” theories can be found in social psychology (see Merton, 1949). As a result, the recent *Handbook of Theories in Social Psychology* (Van Lange, Kruglanski & Higgins, 2012) contains accounts of 51 social psychological theories in total. This raises the question of whether every analysis that is referred to as a “theory” actually can be considered as truly offering a different approach, perspective, or range of predictions, or whether it mainly identifies a new principle, implication, corollary, or context in which a broader and previously documented phenomenon emerges. The reverse can also be the case, when efforts to develop a specific tool or intervention elicit a novel theoretical perspective. Indeed, applied questions can very well give rise to theory formation and inform basic science (Stokes, 1997).

What then are the characteristics of a sustainable theory—what should we look out for or try to develop? Obviously, there is no one best answer to this question, and many scholars have provided their own solutions or personal strategies to address this issue (see, for instance, the special issue of *Personality and Social Psychology Review*, 2004, 8 (2)). Nevertheless, there seems to be convergence about some key criteria that characterize a theory that facilitates the connection of different findings into a cumulative science base. These include the following:

(i) **Abstraction**: The theory contains a set of constructs or underlying dimensions that help reduce and compare complex phenomena, by clarifying how these can emerge in different forms or situations. This allows the theory to apply beyond a single case or research paradigm and helps connect experimental findings to real world phenomena (e.g., Kruglanski, 2004; 2008).

(ii) **Sequence**: The theory contains descriptions of series of related events, or sequences of processes explaining how and why one follows from the other. This allows the theory to go beyond a single prediction, to tell a “good story” (e.g., Fiske, 2006).

(iii) **Conditions**: The theory goes beyond a universal principle, by detailing circumstances under which this principle is likely to emerge, in the form of boundary conditions or moderating variables. This makes the theory a social psychological theory, in that social psychology explicitly aims to specify the implications of different situational contexts that are provided by relational interdependencies, historical developments, political circumstances, or cultural differences (e.g., Mischel, 2006).

(iv) **Range**: The theory encompasses and connects phenomenological aspects of a specific psychological mechanism (indicating different conditions and forms), the way it unfolds (in terms of antecedents, processes, and consequences), and implications at different levels (for cognition, affect, and behavior). This allows the theory to connect its different building blocks into a coherent framework that applies to a broader range of outcomes (e.g., Brewer, 2004).

These are many requirements, and theory building typically is a slow and incremental process, where different parts of the theory are developed, tested, and refined in different stages. Nevertheless, having in mind a set of criteria
such as these can help shape and connect these incremental efforts. Specific hypotheses or observations may be better understood when these are explicitly considered in terms of their contribution and relation to the broader theory, and these criteria may function as a template to identify aspects of the theory in need of further development.

There is no shame in using “someone else’s” theory (Mischel, 2006) nor in spending a lifetime to carve out and refine a single theoretical perspective (as John Turner did, see also Haslam, Reicher, & Reynolds, 2012). We need to work together to expand our collective knowledge, and there is much to gain by combining our efforts in this way. Still, research productivity and standing in the field (which in turn offers access to job opportunities or research funds) are measured primarily in terms of the number of published papers, preferably first authored, and preferably in “high impact” peer-reviewed journals. The publication of such papers is highly competitive, with the most coveted journals in the field reporting rejection rates of 90% or more. Success in this competition for journal space is facilitated when authors emphasize the novelty of their approach or demonstrate how their research findings differ from prior observations. Some journals explicitly state this as a selection criterion in their editorial policy. Although such policies would seem to seek out and reward the “news value” of a particular contribution, this does not necessarily guarantee that the work in question contributes to extant insights. Incremental knowledge generation requires connection—instead of separation, and drawing of parallels—instead of emphasizing uniqueness (see also Aronson, 1997; Fiske, 2006; Mischel, 2009). Cumulative insight is acquired through vertical expansion (digging deeper), instead of horizontal expansion (adding phenomena) of existing knowledge. Progress in any area of science requires that we focus on how our work fits with existing knowledge and resembles prior observations, not how it is different from everything we know (Reis, 2007).

Agreement about a limited set of central organizing principles (which may emerge in different shapes or forms) is needed to be able to work together on common themes and goals (Fiske, 2006; Higgins, 2004; Reis, 2007). It may be possible to define some core constructs, concerns, or motives that can be considered in this way at each level of analysis. This type of connection through which a large variety of phenomena and observations is considered in terms of a limited set of common denominators is made by Daniel Kahneman in his book “thinking fast and slow” (Kahneman, 2011). Likewise, Susan Fiske (2010) covered the whole field of social psychology by organizing different theories, predictions, and findings in terms of five core motives (referring to the need to belong, to understand, to control, to enhance the self, and to trust). These are important efforts that can inspire us toward similar attempts at connecting our work to prior insights and identifying the similarities rather than the differences in what we do as a collective.

WAYS TO CONNECT THE DOTS

To be able to use emergent findings to progressively extend a cumulative knowledge base, current research efforts need to be complemented with a different type of work. The isolated findings reported in the empirical literature can only advance the field if they are connected to existing knowledge and integrated into overarching theoretical frameworks (see also Van Lange, 2006). This requires a sustained and long-term effort. Keeping up such efforts despite shifts in the public interest for particular topics, changes in expectations about research output, and current trends in funding is hard.

Still, there are at least three levels of connection that can and should be made:

Within research lines

It is important to combine different methods and measures to capture phenomena in their full complexity. Conceptual replications elucidate the robustness and generalizability of results, and the examination of moderating variables in complex research designs can reveal boundary conditions and conditional effects, which can contribute to theory building as indicated above (Bonnet, 2012). In writing up research, connections can be made by referring to previous formulations of similar ideas, specifying how early work is extended and basic principles can be refined through new observations (see also Haslam et al., 2012). Recent findings can be placed in a broader perspective by citing work that was carried out more than 10 years ago, without having to include a “history of social psychology” in the introduction section of each paper. Preparing periodic reviews and meta-analyses is another way to avoid that we simply collect an ever-expanding set of disjointed findings that is presented in a moving window of references.

Between research lines

In view of the sheer number of social psychology publications coming out each year, it is increasingly difficult to keep track of all the work that is carried out. Following the main developments in our own areas of interest already is a challenge, and it is virtually impossible to digest the implications of what is going on in other domains of social psychology. As a consequence, we run the risk of re-inventing the wheel more than once—even if we call it by a different name. This is why regularly updated handbooks (e.g., Fiske, Gilbert, & Lindzey, 2010), edited volumes, monographs, and review journals have become indispensable resources, as they provide an overview of developments in specific areas, connect disparate research programs to broader theories, or identify common themes that can help generate cumulative knowledge (see also Fiske, 2006). With the limited time available for research and writing, it is tempting to give priority to writing up one’s own data and having these published in peer-reviewed journals. In the end, however, writing a good overview, or providing a new structure that organizes and connects different research programs, may have a larger impact on the field (and attract more citations) than any “ground-breaking” empirical paper (see also Haslam et al., 2012).

Transcending disciplines

Social psychology has the potential to incorporate and connect different systems of analysis, ranging from microlevel biological
CONCLUSION

The field of social psychology is vibrant and exciting, but isolated dots of colorful knowledge do too little to inform or inspire others. It is up to us not (only) to add more dots but also to provide an integrative framework that helps understand how they can be connected. Producing a social psychology that aims to provide sustainable knowledge is the only way to have a real impact on the issues we seek to understand and the problems we aim to address. Why not make this a priority in our agenda for in 2013.

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