

Running Head: EDITORIAL

Editorial: Barriers to Converting Applied Social Psychology to Bettering the Human Condition

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Why we do what we do

There is a problem with applied social psychology research—what might be considered the proverbial elephant-in-the-room—that it is the rare applied social psychology article, or set of articles, that is eventually implemented into intervention or policy to better the human condition (Bickman & Rog, 2008; Bhave et al., 2016; Dasgupta & Stout, 2012; Dir et al., 2021 Kantilal et al., 2020).¹ The aim of this editorial is to consider the barriers that arise when going from applied social psychology research to applications of the findings to an actual policy utilized in a company setting (e.g. reducing discrimination in the work place) (e.g. Cesario, 2021), an educational sector (e.g. increasing reading ability of children) (e.g. Preece & Levy, 2020), a gym (e.g. surmounting social anxiety of those that are reluctant to enter a gym) (e.g. Horenstein et al, 2021), or a lunchtime canteen (e.g. increasing the consumption of healthier meal options) (e.g. Lassen et al, 2014). Each of these can be seen as practical efforts to better the human condition.

Why we can't always achieve what we want to do

There are barriers that are specific to the generalization of findings from a laboratory or field study to a practical setting (e.g., scalability) and there are barriers that concern the differences between different communities of experts (e.g., scientists, policy professionals) that are involved in the process of traversing the distance from scientific evidence to practice. The former concerns issues specific to scientific matters, which we first discuss. To the latter, the goals of an applied social scientist can be misaligned with the goals of a public policy professional, which need explication because they are rarely considered in our community. Thus,

¹ Human condition can refer broadly to Arendt's (1958) categories: Labor – biological life of humans and how to satisfy needs for survival (e.g. grocery shopping, child care), Work – the artificial objects that humans have structured and built that aren't just means to a specific end (e.g. the production of cultural artifacts [books, painting, poetry] for public good), Action – production of individual pursuits (e.g. improving personal reason, understanding and judgment of the world).

we discuss the various barriers for each community (science, policy) to understand why it is that so little applied social psychology research better the human condition.

Social science research follows a basic drive to investigate phenomena by identifying, classifying, measuring, and then understanding the mechanisms that cause them to occur;² these are uncontroversial characteristics of any basic science. The applied aspect of social science research is one step along, which is to use the knowledge gained to better the human condition. Thus, the goal here must be an effort to convert findings into something that could meaningfully be applied to help people make better life choices, interact with others in ways that improve their lives and the lives of others, and so on. So, the lack of achieving such application can be considered an unsatisfactory state-of-affairs.

In the first section we present some fundamental reasons for failures to better the human condition, by focusing on the scientific barriers that applied scientists face. To this point, perhaps applied social psychology research is simply not good enough to engender feasible plans (alternatively referred to in other sectors as study protocols) to better the human condition. This seems an especially negative and hopeless view to take. In contrast, perhaps some applied social psychology research is good enough, but there is a gap between the research and a feasible plan for bettering the human condition. We believe that the latter is a more plausible reason for the shortfall, though we admit that much applied social psychology research is of poor quality. Thus, even if the requisite research has been performed, and even if it is of high quality, these need not imply a feasible plan of action that could meaningfully better the human condition for a further reason. To expect those in positions of authority (e.g., public policy professionals), who would be the ones to control whether feasible plans are formed and implemented, to be substantive

² Better yet, from a basic research perspective, is the advancement of a full-fledged theory that provides unification (Trafimow, 2012a).

experts too is perhaps unreasonable (Goodvin, & Lee, 2017). Consequently, the onus of responsibility falls squarely back on the applied social scientist, with her specialized knowledge, to provide much more help than hitherto, to the policy professional to traverse the distance from scientific evidence to a feasible plan. The enterprise of evidence-based policy making is consistent with this perspective (Cairney, 2016; Sanderson, 2004; Simon, 1945), whereby policy professionals make decisions that best reflect scientific evidence showing a policy instrument works – because ‘what matters is what works’ (for discussion see Shaw, 2012). Some public policy making is based on a naïve insistence on evidence obtained via randomized control trials (e.g., Halpern, 2015; Halpern & Sanders, 2016), which may be going too far given that randomized control trials carry their own significant limitations with respect to generalizing to field settings (Cartwright, 2010; Ogilvie et al 2020). However, notwithstanding the plusses and minuses pertaining to randomized control trials, the basic idea of using scientific tools in policy making, that emphasizes evidence over opinion, suggests an important role for applied scientists in policy decision-making (Hill & Varone, 2021).

If we assume that the shortcomings of applied social psychology in bettering the human condition is not because it does not have the capacity for providing the foundation for developing useful plans, but rather because of the gap between the research and developing a plan, we can now consider the following. Applied social psychologists, themselves, could propose feasible plans to better the human condition that could then be adopted by policy professionals for interventions or policy changes. In short, the aim would be to have applied social psychologists propose feasible plans for intervention or policy change, based on applied social psychology findings, applicable social psychology theory, validated auxiliary assumptions for applying

social psychology theory, well-supported social psychological mechanisms, or more than one of these.

Consequently, we sent out a call for a special issue of *Basic and Applied Social Psychology* (BASP) for feasible plans to better the human condition. The bad news is that we received only one submission.³ Although there are many potential explanations, based on conversations we have had with interested prospective authors, we believe that many perceived the task of bridging the gap between applied social psychology research and feasible plans to better the human condition too difficult to perform. Perhaps we asked for too much and underestimated the difficulty in converting applied social psychological research into feasible plans for interventions or policy changes to better the human condition. In the following analysis of the issues, we consider the problems faced, with illustrations. We use this as an opportunity to explore the various barriers, with a view to providing practical ways to address them.

Basic scientific problems in translating evidence into feasible plans for implementation

Consider a hypothetical example. A researcher is interested in reducing discrimination towards members of a particular group. The context in which discrimination is considered of relevance is in a workplace setting. To better the human condition, the goal is to develop a plan for changing the working environment to attenuate the outcomes that would constitute discrimination (e.g., harassment of members of a specific group). Thus, the study comprises two elements, the first is to develop a measurement tool to index the degree of discrimination and the second is to devise a set of candidate interventions designed to reduce discrimination. The researcher performs a set of test construction and validation studies to provide a good case that she has a valid instrument for measuring discrimination. Subsequently, she performs a large-

³The manuscript is still under review, as of this writing.

scale correlational study measuring *A, B, C, D, E, F,* and *G,* to attempt to predict discrimination towards members of the group. She finds, say, that *A* is the best predictor of discrimination as measured by her validated instrument, and performs much better than any other predictor variable. Questions remain. The most obvious of these is: How do we know that manipulating *A* will decrease discrimination, to a meaningful extent towards the outgroup? To answer this question, the researcher might attempt to manipulate *A* and obtain an effect on discrimination, as measured by the validated instrument.⁴

As mentioned earlier, we speculate that a significant barrier to developing plans for bettering the human condition from applied social psychology literatures is the difficulty in converting from evidence into an implementable plan. Therefore, a researcher may face several significant questions that then lead to a conclusion that the enterprise is too difficult to achieve. For instance, consider the following bullet-listed questions.

- Is the size of the effect sufficiently large to justify a large-scale intervention or policy?
- Even if the effect is that large—and very little research in applied social psychology can boast impressive effect sizes at the experimental level—how can we have reasonable assurance that the effect size would not shrink importantly outside the laboratory?
- What is an acceptable effect size to justify efforts by policy professionals?
- Is the behavioral change sustainable over time? In the applied world, be it trialing an intervention that impacts an organization (e.g., private sector), or society at

⁴ An alternative is to perform a mediation analysis where the arrows in the path diagram are presumed to indicate causation. However, this is an extraordinarily weak form of evidence as attested to in the 2015 special issue of *Basic and Applied Social Psychology* on mediation (e.g., Grice et al., 2015; Kline, 2015; Tate, 2015; Thoemmes, 2015; Trafimow, 2015a).

large (e.g., public sector social policy making), it often matters that behavioral change is sustained over time, and better yet generalizes beyond the context in which it was originally implemented.

Back to our illustration. Suppose the researcher conducts more research. She shows that she can manipulate A , with an impressive effect size on discrimination, and she performs yet further experiments showing that the effect size remains impressive even with important changes in the experimental paradigm, thereby providing some reason to believe that the effect size generalizes to the workplace setting where generalization is needed.

Giving that the effect size generalizes to the workplace setting, difficulties nevertheless remain. For one thing, a manipulation of A that is reasonable in a laboratory setting might not be reasonable on a large scale in a field study. To take a silly example that steps on nobody's toes, suppose that paying undergraduates in the laboratory to perform one pushup every day for two weeks, in front of a research assistant, decreases discrimination with Cohen's $d = 0.90$ —an impressively large effect. Suppose further that the sample size is sufficiently large that we trust that the sample effect size is a good estimate of the population effect size, an issue that has received insufficient attention from applied social psychologists. If the sample effect size is not a good estimate of the population effect size, it makes little sense to base interventions or policy changes on it. Trafimow (2019) has reviewed this issue in detail and Trafimow and Myüz (2019) showed that most sample effect sizes, across psychology domains, are poor estimates of population effect sizes. Trafimow, Hyman, and Kostyk (2020) provided a similar demonstration for marketing research. On the other hand, medical research in top journals seems better off in this respect (Trafimow et al., 2019); though some may argue that medicine may not be immune to the same problems (Boulesteix, et al., 2020).

To continue with the example, suppose the finding replicates with participants of different ages, occupations, and so on. Unfortunately, even with all this, we are still not in business. To see why, consider that a researcher with a large grant could feasibly pay people to perform one pushup a day for two weeks in the experimental condition and not in the control condition, in the presence of a research assistant. But how is this to be extended to the general population of well over 300,000,000 Americans? Even if it were feasible to pay all of them to perform one pushup per day for two weeks—and it is not—how would they be induced to do it in front of a research assistant? Then, too, how do we know that the effect of the pushups would last long enough to make the whole endeavor cost-effective?

In fact, these issues have been specifically discussed in the domain of nudging; the application of cognitive and social psychology to develop interventions that have real world consequences in social policy making (Thaler & Sunstein, 2008, 2021). Osman (2022) discussed, amongst various concerns with nudging, the significant issues in establishing a viable effect size in laboratory/field studies that would justify a nudge being rolled out population wide. Arguments for the use of nudging, especially when implicating details of effect sizes, are that, even if the effects are small, if the intervention is applied to a population, then an effect of 1-4% translates to thousands or tens of thousands experiencing positive behavioral change. Therefore, at that level of scalability, nudges are justifiable assuming they are relatively less costly to implement than more conventional regulatory instruments (e.g., taxes, mandates, bans). But are they? In fact, there are three matters that limit this justification. First, the overall effect size of nudges as a means of achieving reliable behavioral change is still debated. A recent meta-analytic study (Mertens et al., 2022) has come under fire because the overall effect sizes of nudges appeared to be suspiciously high, and for including studies that have been retracted due

to faked data (see <https://statmodeling.stat.columbia.edu/>). There have now been calls to submit a correction of the original meta-analysis. Second, even if there are better attempts to ascertain accurate effect sizes, we know that there are limits to their effectiveness in real world settings. When large field studies are conducted to test the applicability of nudges from laboratory to real world settings, their effects dwindle (DellaVigna & Linos, 2020; Hummel & Maedche, 2019). Furthermore, the effects are fragile because they do not reliably sustain over time. Third, it is not clear yet whether the cost-benefit analysis of the implementation of nudges works out in favor of the benefits outweighing the economic costs. The costs can be relative to other more conventional policy instruments (e.g., taxes) (e.g., Carlsson et al., 2019; Ito, 2015) or other background costs not originally considered (e.g., Atkins, 2019; Nwafor et al., 2021). In short, the bullet-listed questions we posed in the context of a hypothetical example are not just an intellectual exercise. They are ubiquitous questions that need to be addressed wherever there is a pressing need to employ psychological insights to help solve problems that individuals and groups face in the real world.

Applied scientific problems in translating evidence into policy

Here we focus on the misalignment of the goals of the applied scientist and the goals of the policy professional. An applied scientist may have a narrow definition of success, that the intervention worked, assessed against a statistical convention, and that it can be promoted via publication of the manuscript. For a policy professional, success is much more broadly defined. There is consideration of the tradeoff between costs and benefits of an intervention or policy change, including the practicality of scaling up from laboratory or field demonstrations. Then, too, there are political considerations such as whether the legislation for a policy change passed without amendments, the degree of political support, and implications regarding the popularity of

the government (Marsh & McConnell, 2010). Moreover, there are programmatic dimensions of success: operational (e.g., will the intervention be implemented as originally envisaged?), outcome (did the intervention achieve the predicted outcomes?), resource (e.g., was the intervention an efficient use of public funds?), actors/interest (e.g., did the intervention benefit a particular subset of the population or was it generalizable to a wider set?). Policy professionals often arrange advisory committees to address these issues, informing policy making with evidence (Parkhurst, 2017). Thus, much goes into policy making that is not always appreciated by applied social psychologists.

The primary question that an applied social psychology researcher confronts when moving from their understanding of the substantive literature to an implementable plan, concerns a single programmatic dimension that policy makers are concerned with, namely the outcome (behavioral change achieved via the intervention). In fact, once past the exploratory stage, the only real matter of interest for a scientist is to set up an experiment to demonstrate that a trialed intervention produces a meaningful effect in the direction predicted. But when the researcher considers other aspects of success, mentioned above, aligned to those interests of the policy maker, they face a host of other practical constraints on what intervention is feasible, for who, and how the intervention can be implemented. Could it be that when inviting the academic community to submit to a special issue of *BASP* for feasible plans to better the human condition, these factors played a role in limiting submissions?

The question suggests potential issues, such as that entering the applied domain presents researchers with perceived as well as real problems regarding what role they should take and the extent to which engagement with policy professionals may interfere with maintaining objectivity and independence (Nielson, 2001). The concern voiced by scientists is that of the blurring of

lines (Gluckman, 2014; Hetherington & Philips, 2020; Singh et al., 2014; Weingart, 1999).

Where one might see oneself as acting as an expert by providing advice based on evidence, the worry is of veering into the role of advocate because one's evidence can be used to further a particular agenda (Motta, 2019). Relatedly, there is the potential danger of politicizing science: some might object to an undue influence of science on the process of policy making (Weingart, 1999) or to altering university research foci into too concentrated an emphasis on generating evidence for societal impact (Chan et al., 2005, Hetherington & Philips, 2020). However, as many have pointed out, scientific work—particularly applied scientific work—is never truly value-free (Brown, 2015; Douglas, 2009; Fernández Pinto, & Hicks, 2019; Pielke, 2007).

Moreover, it is possible to argue that the mere fact that a person has signed onto being an *applied* social psychologist, as opposed to a *basic* social psychologist, implies an a priori commitment to an extra-scientific goal, that of bettering the human condition in some way. Acknowledging this obvious implication indicates it is ingenuous for applied social psychologists to insist that there are no extra-scientific agendas.

Of course, we recognize the hypothetical possibility that policy makers may misuse scientific applied social psychology for agendas unanticipated by scientists, though we are much more worried about the ubiquitous lack of policy makers using applied social psychology. As applied science is not value-free, some argue that scientists are responsible for engaging other audiences (e.g., policy makers, private sector, public, media) and correcting misapprehensions of their work (Ish-Shalom, 2009). Others suggest that scientists should understand the policy making process to exert more control over how their research is used (Hetherington & Philips, 2020). Furthermore, scientists could commence the research process with stronger engagement with policy professionals (Bednarek, et al., 2018; Gluckman, Bardsley, & Kaiser, 2021; Pielke,

2007). Such engagement could increase the ability to communicate outside of academia and increase the likelihood of the work having a meaningful impact. An advantage of early engagement, from the point of view of a policy maker, may be that the participation of an applied social psychologist could aid in choosing promising variables and increasing the probability that the policy to be designed will be in accord with the science of social psychology (e.g. co-production).

In raising these matters, we hope to expose the full range of factors that fall into scientific issues, as well as practical policy issues that inevitably ensue when working in an applied field and endeavoring to provide ways to better the human condition. In principle, the agenda for basic research is unbounded because it is designed to increase knowledge for its own sake, and that need not restrict what phenomena are examined and how they are examined.⁵ Applied research is bounded because the agenda is to produce research that can work in practice, just as engineers are constrained by the physical properties of the world to provide solutions to physical problems. An applied goal necessitates that there are additional boundaries including the vast array of issues that confront policy makers when converting applied social psychological research into practice. Thus, a discussion needs to be had pertaining to practical considerations that applied social psychology researchers should bear in mind beyond scientific ones, to endow their proposed interventions with the best chance to better the human condition.

Practical considerations for applied social psychology researchers

Although we believe that some applied social psychology research is applicable, there is much that either is not, or is very far away from being applicable. For example, there are issues even at the level of measurement, where many researchers seem to believe that obtaining a nice

⁵ BASP will continue to publish high-quality basic social psychology research, regardless of practical implications.

factor structure is sufficient for demonstrating construct validity. It is not. As Cronbach and Meehl (1955) carefully explained, construct validity concerns establishing a matching of empirical and theoretical relations. Implications are that measures of the same constructs should correlate extremely highly (sometimes termed “convergent validity”), measures of different but theoretically connected constructs should correlate, and measures of theoretically unconnected constructs should exhibit very small correlations (sometimes termed “divergent validity”). At best, establishing a nice factor structure can be considered a promising beginning on the way to demonstrating construct validity, but a promising beginning towards a goal hardly can be considered equivalent to achieving that goal.⁶ Clearly, if the measures themselves are of questionable validity (Loken & Gelman, 2017), feasible plans based on invalid measures are out of the question. In addition, if there is no theory, then there are no theoretical relations, and no way for empirical relations to match them. In this latter case, measurement validity would have to be established outside of construct validity considerations. As will be discussed later, it is possible to perform useful applied research sans theory.

Leaving the validity of the measures aside, it is inescapable that the vast majority of applied social psychology research is correlational, often supplemented by mediation analyses. But as explained earlier, obtaining a correlation falls far short of indicating that manipulating one of the variables would cause an effect on the other. Nor does supplementation via mediation analyses help; mediation analyses provide an extremely weak form of evidence, as the articles in the special BASP issue on mediation, in 2015 demonstrate (e.g., Grice et al., 2015; Kline, 2015; Tate, 2015; Thoemmes, 2015; Trafimow, 2015a). Because of that extreme level of weakness,

⁶ It is not clear that attaining construct validity ought to be the goal (Slaney & Garcia, 2015; Trafimow, 2012b; in press), but that it is a matter too far afield from present purposes to be discussed further. For now, it is sufficient that if researchers claim to have attained construct validity, they ought to have done it.

such research fails to move the field an important amount of distance towards feasible plans to better the human condition. An exception would be if that research were to be supplemented by different research paradigms or contributions from other disciplines. Another exception might be if effect sizes are so large as to render alternative explanations implausible. Research on how smoking causes cancer might exemplify both exceptions (see Brandt, 2007; Proctor, 2011 for reviews). A worry of ours is that correlational applied social psychology research somehow tends to fail to engage these exceptions and remains perpetually “exploratory” and not useful to humanity.

In addition to the problem of perpetual exploratory research, sometimes applied social psychologists plain get it wrong or exaggerate. For instance, consider the much-touted link between violent video games and violent behaviors; applied social scientists made a plethora of strong assertions that violent media is responsible for 10% to 30% of violence in society (Comstock, & Strasburger, 1993; Strasburger, 2007). There also have been allegations that perpetrators of gun violence, such as the Columbine High School Massacre, were fans of violent videogames, again asserting a causal connection between violent media and violent behaviors. Similarly, the American Psychological Association presented repeated resolutions concerning the causal effect of violent media on violent behavior (American Psychological Association, 2005; 2015a; 2015b; 2019; 2020). However, the United States Secret Service did not find a consistent pattern linking violent videogames to school shootings (2002), nor did the US Supreme court find the evidence persuasive in *Brown v. Entertainment Merchants Association* (Ferguson, 2013). That the Supreme Court likely was right, along with other governmental organizations (e.g., Australia, 2010; Sweden, 2011; US Dept. for Education, 2018), and the psychologists likely wrong, was testified to by repeated failures to find a reliable causal association between

media violence and violent behavior that included large-scale research (Dummond et al., 2021), longitudinal work (Ferguson & Wang, 2019; Kühn et al., 2019), and meta-analyses (Drummond et al., 2020; Ferguson, 2015). Others suggested that the causal link exists but is much weaker than it has been touted to be (Bushman & Anderson, 2021; Greitemeyer & Mügge, 2014; Mathur & VanderWeele, 2019; Sherry 2001). Either way, authorities seem to have been right to discount the applied social science research, which highlights that if applied social psychologists are to have a positive effect on society, they had better get the research right.

In summary, correlational research is not useful unless it leads to further research that more strongly implicates that the alleged cause of an effect really does cause the effect. Without such further research, there is little reason to place confidence in an intervention or policy change based on the correlation. A qualification might be if the intervention or policy change itself serves to provide that further evidence. In turn, such a demonstration is not useful unless the effect size is impressively large, of course, defining ‘impressively large’ by the nature of the problem, the number of people potentially to be affected, the degree to which they are affected, and others. Even demonstrating the foregoing is not sufficient unless the research generalizes to the context and people where it needs to generalize. In addition, there is the necessity for the effect to be sustained for a sufficiently long period to justify the costs. And even if that requirement is met, it might be that a manipulation that is feasible at the research level is not feasible at the level of large-scale implementation; there might not be a feasible way to implement the manipulation on a large scale or commence an effective policy on a large scale. The road from initial applied social psychology research to bettering the human condition is long indeed.

And yet, researchers have been performing applied social psychology research for well over a century (e.g., at least since the elegant study by Triplett, 1898 showing that social facilitation increases bicycle riding performance). Despite the foregoing difficulties, it is not unreasonable to believe that we should have done better. But there is no point in crying over spilled milk. Let us consider what we can do now so that a century later nobody writes a BASP editorial about the lack of applied social psychology work that betters the human condition. To move in this direction, we provide the following encouragements to researchers.

It is impossible to avoid that applied research manuscripts in social psychology have long introductions with significant content about a theory or theories. The tacit assumption is that the applied work is somehow better if it is tied to theory. Although this might be so, it is not necessarily so. As a trivial example, consider Johan Vaaler [March 15, 1866 – March 14, 1910]. He invented the paper clip, a clearly useful invention that nevertheless required no theory. In much applied social psychology research, the cited theoretical work is only tangentially relevant, and insertion is to placate reviewers who insist on connection to theory. In most cases, the quality of the research would not be decreased by simply omitting any mention of theory or only a very brief mention. An exception might be Pearce and Cooper (2021), who based an intervention designed to increase COVID-19 safe behaviors very clearly on Festinger's cognitive dissonance theory, and where the discussion of the theory was both necessary and justified. Future authors should be assured that their applied manuscripts will not be rejected because of insufficient theory unless theory really is necessary for the type of applied research conducted.⁷ More to the point, if theory is to be used, it needs to be brought into the article in a way that

⁷ Trafimow (2016) suggested a taxonomy of applied research that considers theory, auxiliary assumptions, combining theory and auxiliary assumptions, and atheoretical applied work. Thus, atheoretical applied work can be legitimate.

shows its central role in guiding hypotheses and deriving predictions, rather than an add-on for cosmetic purposes to give the impression that the work is informed by theory. For example, to return to nudge, the development of interventions inspired by work from cognitive psychology, social psychology, and behavioral economics is theory light; this is because there are no current theories of nudge, only frameworks (Osman et al., 2021). Here, too, the frameworks only give a general impression of what could be implemented as an intervention to address a social policy problem. Currently, there is a lack of principled means to show how a theory of a psychological process, given a particular context, can be directly connected to a set of viable interventions. Nor have researchers developed a principled way to identify the key target behaviors of interest, the barriers to generating behavioral change, and the affordances in particular contexts that could boost the interventions (an exception to this is Meder et al., 2018).⁸ Even in the absence of theory, or even well formulated frameworks, past evidence can be used to inform prospective plans or trialed interventions, but this seldom has been accomplished. Furthermore, although past failures can be used to inform what can be done better (Hummel & Maedche, 2019; Osman et al., 2020), few researchers have pursued this path.

And there is another problem with basing intervention or policy on theory, even if the theory were certain to be true. For illustration, suppose that we agree on a theoretical assumption that attitudes cause behavior. It is obvious, then, that manipulating attitudes towards eating broccoli should increase broccoli-eating-behavior, relative to a control condition. Suppose an intervention is attempted and fails. One possible interpretation is that the theory is wrong, however, a more plausible interpretation is that the intervention failed to influence attitudes towards eating broccoli (St Quinton, Morris, & Trafimow, 2021; Trafimow, 2015b). McEwan et

⁸ There have been recent attempts to provide a principled causal analytic approach (Causal Bayesian networks) to address this (Osman et al., 2020).

al. (2019) performed a meta-analysis comparing the effect sizes of physical activity interventions based, or not based, on theory. They obtained a trivial difference, thereby suggesting that theory is not crucial for effective interventions. Although the theories could be at fault, the problem may plausibly reside in difficulties implementing them. We believe that an under-investigated category of applied social psychology research is in elucidating and justifying extra-theoretical assumptions, sometimes termed auxiliary assumptions, necessary to implement theories effectively to obtain desired outcomes (Trafimow, 2016). Put in the form of a pointed question, “How does one manipulate people into liking (positive attitude) eating broccoli?”

Omitting theory, in those cases where theory is not centrally relevant, need not imply that applied BASP manuscripts will be short. In the future, authors are encouraged to discuss, with extreme care and detail, what the potential roads are to feasible plans to better the human condition and the value of the presented research in the context of the whole road or roads. It is a plus if authors consider the needs of policy professionals, including why a behavior is viewed as a deficit and the affordances available to translate the deficit into an asset.⁹ If the goal is to reduce racism or discrimination (e.g., Avery, Oh, & Cooper, 2021; Hughes et al., 2021; Lesick & Zell, 2021; Partow, Cook, & McDonald, 2021), there should be a detailed consideration of the various issues mentioned in the earlier discussion of that issue. If the idea is to reduce mobile phone use while driving, the manuscript should carefully consider how to traverse the long road from initial research to a feasible plan to importantly reduce mobile phone use. Such consideration could include a plan for taking a deficit—unsafe driving—and transforming it into an asset—safe driving. And so on for other applied social psychology areas including COVID-19 safety (e.g., Cunningham et al., 2021), social influence (Grzyb & Dolinski, 2021), organizational

⁹ Policy professionals are often concerned with deficits, and transforming them into assets, in the health domain (Morgan & Ziglio, 2007).

practices (Hetrick et al., 2021; Wang, Han, & Li, 2021), healthy eating (Requero et al., 2021), and authoritarianism (Jarmakowski & Radkiewicz, 2021), to name a few that have received recent attention. Furthermore, authors are encouraged to include at least one experimental study along with correlational work. An exception would be if there are special circumstances rendering experimental research too difficult to perform, and authors should make clear if that is so and why. The goal is to reduce what typically occurs, which is that each applied social psychology article fails to result in bettering the human condition, even in combination with future applied social psychology articles.

In addition, authors will be expected to spell out the potential road, or roads, by which a feasible plan to better the human condition might plausibly be expected to come about. Furthermore, in the discussion section, authors should clarify how much distance along such a road the presented research takes us, and how much distance yet remains to be traversed. Authors should also provide a detailed exposition of one or more ways to cover the remaining distance. Alternatively, the authors may feel that their research indicates that a particular road or roads are closed and can contribute importantly by explaining where applied efforts likely would be wasted.

We encourage applied social psychologists to consider connecting their research to a policy area that is known to be of interest to policy professionals. For example, the UK Government publishes areas of research interest (ARIs) that set out ongoing research questions that various government departments assert need addressing (<https://www.gov.uk/government/collections/areas-of-research-interest>). To illustrate, of the list of ARIs the Ministry of Justice published recently (MoJ, 2020), one of the ARIs is: How do geographic, demographic, cultural, and other factors affect people's ability to resolve their legal

problems? This is an invitation to experts in relevant domains to contribute to framing and evidencing a research problem that in turn addresses a policy issue where there is an evidence gap. Taking up the opportunity implies a need to address the critical questions we presented earlier. The example ARI not only articulates the research focus but specifies what needs to be solved. Uncovering the geographic, demographic, and cultural factors that affect people's ability to resolve their legal problems (deficit) may imply ways to surmount the problems (asset). In this case, the practical interventions that are trialed need to be shown to work, where recommendations can then be made that take into consideration the legislative, political, and programmatic dimensions of success that policy professionals care about (Marsh & McConnell, 2010).

Although we are adamant about the need for applied social psychologists to consider policy making perspectives, it is possible to take this too far. We are not advising the following.

Applied social psychologists are not the only people with limitations; policy makers face their own barriers and limitations. Not only are they not scientific experts (though they seek their advice), they face pressures that lead to policies that run parallel to public interest (Flinders et al., 2020). A policy maker might continue to endorse a policy that does not have evidential support (Hill & Varone, 2021; Howlett, 2012; Stevens, 2011, 2021), though in some cases going against social psychology might be warranted (e.g., violent video games). Bearing in mind the interests of the policy maker in no way obligates the applied social psychologist to support a policy that does not work. In such cases, ethical and courageous applied social psychologists should resist the temptation to appease a need to generate work for the sake of making it align with interests outside of what the evidence shows. There is a need for robust discussion from those with alternative interpretations and convictions, and to stand firm against policy agendas where the

evidence conflicts with the current political climate (Goodvin, & Lee, 2017; Osman et al., 2022). Moreover, social scientists should not accept grants to perform research to support policy initiatives just for the sake of attracting funding. The academic institutional structures to which scientists belong should protect as well as enable freedoms from incentive structures that may make this option difficult to resist (Chan, Higgins, & Porder, 2005; Singh et al., 2014).

Relatedly, policy makers may introduce policies that are ‘too big to fail,’ but nevertheless do not work (Bovens, & ‘t Hart, 2016; Dunlop, 2017; Howlett, Ramesh, & Wu, 2015). For example, policies for regulating videogame violence seem admirably suited to address an important problem, and they are vociferously supported by organizations with connections to social science. The totality of social science pronouncements, assertions by organizations, and the regulations themselves, may seem to render the policies too big to fail. However, we have seen that the evidence fails to support the foundational assumption on which all this is based. In general, not only do applied social psychologists have no obligation to support policies unsupported by evidence, but on the contrary, have an ethical obligation to challenge them. Again, there is the necessity to speak truth to power. One reason for persisting with a failing policy is the unwillingness to admit, or fail to appreciate in the first place, the limitations of the evidence on which policies are based (Howlett et al., 2015). Policy professionals tend to assert greater levels of certainty and confidence in a policy than is warranted, short-term or long-term, which Nair and Howlett (2017) referred to as “policy myopia.” In such cases, scientists should use their expert substantive knowledge to seek out appropriate forums and public engagement opportunities, to offer robust but constructive challenges. Of course, as we saw earlier with the violent media example, sometimes it is the researchers themselves who get it wrong.

In making policy decisions, we have suggested the importance of considering political issues, and we continue to support that. But considering political issues need not imply acquiescing with them. If political considerations and the public interest fail to coalesce, the public interest should win. However, there is no avoiding that a sophisticated understanding of the political considerations likely will increase the probability of a win—or at least a partial win—for the public interest. Our recommendation is not that applied social psychologists fail to be idealistic, in fact we encourage the opposite, only that they are realistic in pursuing idealism. Sometimes the perfect can be the enemy of the good, and a realistic and sophisticated understanding of the relevant political issues can aid applied social psychologists in understanding when this is so.

Concluding remarks

Let us return to the larger panorama, which is that applied social psychology research just is not good enough when viewed from the perspective of the number of people who have benefited thereby relative to the time, effort, and money invested for well over a century. Our optimistic hope is that if researchers could be induced to think about roads to feasible plans to better the human condition, applied social psychology would improve, and many would benefit. Obviously, moving in this direction necessitates careful thinking about larger policy perspectives; there is much more to consider than just the relevant substantive literature.

Because the present authors are scientists, we hesitate to recommend that researchers consider extra-scientific issues. But we are forced into this unpleasant recommendation by the blatant historical fact that applied social psychology has performed badly at achieving its goal to better the human condition. At some level, we suspect that applied social psychologists already know this. For example, hall talk among applied social psychologists emphasizes gaining

publications or grants, not bettering the human condition. In fact, in academia, it would be tantamount to heretical to turn talk away from the former and towards the latter; a university administrator might react to the switch with horror. The present editorial might be viewed as an attempt to make it socially acceptable to do what is right by putting humanity first.

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