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Ambivalence about social relevance? How we can reframe academic research and reaffirm our commitment to the public good

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ABSTRACT

Although psychologists are well positioned to address social problems, our public impact has remained relatively limited. Collectively, we might wonder why psychology, as a discipline, has not had the level of public impact we would ideally like it to have. I believe many of us struggle with some degree of ambivalence about the nature and value of research oriented towards public import and impact, perceiving incompatibility between scientific pursuits and their potential for real-world application. However, we can reframe much of our work to focus simultaneously on conducting rigorous research that addresses questions of practical value – which I refer to as *socially relevant* research. In this paper, I discuss what we stand to gain by focusing on the social relevance of our research, or what we might lose by *not* doing so, using some of my own scholarship to provide illustrative examples.

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Within each generation of academic psychologists, perennial, soul-searching questions have emerged regarding whether and how our research can actually make a difference in the “real world”. Psychology scholars have long noted that our field is especially well suited to aid in the analysis and implementation of strategies to address the many social problems human societies face (e.g., Kaslow, 2015; Korten et al., 1970; Miller, 1969; Zimbardo, 2004) and we have witnessed a renewed commitment to translating academic research for the public good in recent years (see, e.g., Lewis & Wai, 2021; Tormos-Aponte et al., 2020; Tropp, 2018). Still, while calls for conducting and disseminating research that promotes the public good span many decades (see Kelman, 1968; Ring, 1967), psychologists also recognise that our

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impact in the public sphere has remained relatively limited. Indeed, scholars have recently argued that we are now facing a “crisis of relevance” due to “a failure to connect theory with the lives of everyday people” (Giner-Sorolla, 2019, p. 3), which, as noted by Cialdini (2009), has contributed to an “eroding [of] the public’s perception of the relevance of our findings to their daily activities” (p. 6).

This emergent trend is particularly disheartening given that, historically, the field of psychology has prided itself on its ability to generate scientific research that could contribute to the public good; indeed, psychology enjoys a rich and storied legacy of using academic research to address pressing social issues. As early as the 1930s, with the rise of fascism in Europe, many academic psychologists expressed “great interest in movement to apply psychology to political problems” (Krechevsky, 1936) and dedicated themselves to conducting scientific research to promote positive social change (Morawski, 1986). Often regarded as the founder of social psychology, Lewin (1947) further stressed that it is not sufficient to do research simply for the sake of academic publication and that academic research should ideally lead to some sort of social action. Yet, psychology’s explicit commitment to conducting research that can be used to benefit society appears to have waned, or at least to have been tempered, in recent decades.

Collectively, we might wonder why this is the case: *why has psychology, as a discipline, not sustained the level of real-world or policy impact that we would ideally like it to have?*

In this paper, I wish to share some reflections in response to this question, based on what I perceive as ambivalence regarding the value of what I term *socially relevant research* – which has (accurately or inaccurately) often been described as “applied” research – relative to the value of what has traditionally been described to as “pure science” or “basic research” (see, e.g., Morawski, 1986). In so doing, I specify several factors that I believe contribute to researchers’ ambivalence related to socially relevant research, and I offer a new frame for how we might conceive of psychological science and application in relation to one another.

Basic research, applied research, and how researchers are often caught in between

Within psychology, there have long been distinctions, and even some tensions, between basic and applied research (Chein, 1966; Giner-Sorolla, 2019; McGuire, 1967; Medin, 2012; Stokes, 1997; Zimbardo, 2004). Generally speaking, basic research refers to investigations driven by theory and designed to test scientific principles and processes towards the generation of new knowledge, whereas applied research refers to investigations driven by a desire to address social problems and designed to test possible

interventions in real-world contexts using scientific insights (Breckler, 2006). As explained by Stokes (1997), “if basic research seeks to extend the area of fundamental understanding, applied research is directed toward some individual or group or societal need or *use*” (p. 8, emphasis added).

With respect to the field of psychology, some have referred to the distinction between basic and applied research as a “false dichotomy” (see Breckler, 2006) or even as a “dangerous dichotomy” (see Medin, 2012), challenging the common presumption that the more a scientist cares about the issues and populations under study, the less likely they are to conduct sound research. At least in part, this tendency for potential applications of our research to be construed as being at odds with the scientific enterprise is likely a function of how we have been trained. As academic researchers, most of us were taught to strive first and foremost to make research contributions that are theoretically novel and empirically sound (Giner-Sorolla, 2019), which may or may not correspond with producing insights of interest and practical value to the broader society (Stokes, 1997). By contrast, practitioners engaged in implementing programmes and policies typically “want to know what works”, such that they are “unlikely to care about how new the effect is, [and are] instead far more interested in how it can be used” (Dalton & Bazerman, 2018, pp. 84–85). Academics who desire for their research to have a broader public impact can end up getting caught somewhere in the middle of these two extremes, such that even scholars who are strongly motivated to conduct research with public impact in mind may have some hesitation or ambivalence about heading down this path. I believe several factors likely contribute to this predicament, which may grow from long-standing views and incentive structures surrounding psychology as an academic discipline.

Wanting to be taken seriously as scientists

In part, psychologists’ reticence and ambivalence about conducting research that lends itself to application stem from our desire to be taken seriously as “real” scientists (see Benjamin & Crouse, 2002; Forscher et al., 2020; Lilienfield, 2012; Rozin, 2001). As a discipline, psychology was “put down for decades by the so-called *hard* sciences as not being a real science” (Sperry, 1993, p. 878), and tensions emerged between those who sought to regard psychology as a “science” as compared to a “practice-oriented field” (Boneau, 1992, p. 1588). Consequently, debates about psychology’s identity and validity as a discipline have endured among scientific researchers in the academy (*cf.* Berezow, 2012; Wilson, 2012) as well as in public discourse more generally (*cf.* Jogalekar, 2013; Tannenbaum, 2013). Some researchers in psychology have also expressed scepticism about psychology’s status as a scientific discipline (Lilienfield, 2012), which is especially unfortunate given all

psychologists have to offer by using scientific rigour and advanced statistical analysis towards greater understanding of social phenomena (Rozin, 2001).

Striving for higher professional status

Relatedly, part of academic psychologists' ambivalence about conducting research with applied value likely grows from a recognition that basic research tends to be regarded as having greater prestige, value, and respect than applied research (see Breckler, 2006; Giner-Sorolla, 2019). This evaluative chasm is so great that some would argue that applied research is "typically accorded second-class status, if not denigrated" (Zimbardo, 2004, p. 340), while those oriented towards more basic research questions must guard against "scientific or academic hubris" (Omoto, 2012, p. 823) or "[looking] toward applied problems with highbrow aversion" (Lewin, 1951, p. 169). Complementing these views, Medin (2012) also highlights that "many of us in academia may be walking around with an implicit or explicit 'basic is better' attitude", and he goes on to present a useful case for us to consider:

Imagine two assistant professors coming up for tenure and one has plenty of publications in Psychological Science and the other has plenty in Applied Psychological Science (a hypothetical journal). Which of the two has a better chance of getting tenure? Correct me if I'm wrong, but it seems to me that – hands down – it is the former.

In his commentary, Medin (2012) further suggests that someone who conducts "psychological research that has high fidelity to real-world circumstances" may be "*accused* of doing applied research" (emphasis added). His use of the term "accused" is particularly noteworthy, in that he intimates that those who conduct socially relevant research may risk being viewed through a criminal lens by some of their professional colleagues. Thus, to the extent that academic psychologists strive to raise or maintain their professional status, they may be wary of conducting research with clear implications for application, given such apparent status differences that pervade our field.

Believing that values have no place in science

Another factor underlying ambivalence about pursuing research with applied value involves broader, perennial debates about the role of values in scientific research, and correspondingly, the roles that scientists should or should not play in relation to the research they conduct (see Pielke, 2007; Schalet et al., 2020). How values relate to science is an issue that has long been debated across many disciplines (Allchin, 1998; Machamer & Wolters, 2011), and specifically within the field of psychology (*cf.* Crosby et al., 2004;

Kendler, 2004). While some have argued that science should be value-free, others contend that values play a central role in all scientific endeavours (Kurtines et al., 1990), as the research questions we choose to pursue cannot easily be disentangled from our own morals, perspectives, and political orientations (Giner-Sorolla, 2019; Howard, 1985; Kelman, 1968). The view that science cannot be fully objective or value-free has gained traction and has become more widely accepted in scientific circles in recent decades (see Machamer & Wolters, 2011). Moreover, scholars within and beyond psychology have argued that scientists have a responsibility to conduct research to promote the public good (Kaslow, 2015; Omoto, 2012). Still, on the whole, many researchers in psychology vacillate between wanting their research to make a meaningful difference in the world, and concerns that doing so may undercut the perceived scientific integrity of their work.

Moving beyond ambivalence and envisioning alternate pathways forward

Whether we observe this ambivalence in other scholars or we recognise it in ourselves, we can acknowledge that the choices we make as researchers are likely informed both by what pathways to success we believe are possible – however we choose to define such success – as well as our fears about potentially choosing a “wrong” path (see, e.g., Cross & Markus, 1991; Markus & Nurius, 1986). Our thoughts about what might be possible, or what might lead to failure, are also likely to depend on apparent constraints and the extent to which we perceive a “fit” between the paths we would ideally like to pursue and those that are expected or normative for the given context (Jahoda, 1961). As such, if the only possible pathways we see before us involve a forced choice between basic and applied research, it is understandable that we might experience some apprehension about choosing one or the other, especially if our ultimate goal is to pursue research that includes elements of both.

But other pathways are indeed possible. For instance, some academic researchers may focus more on basic research during the early stages of their career, to then become more involved in applications of their research at later career stages. This temporal progression – to pursue basic research first and later its application – is particularly common, given the desire to have a firm research foundation from which applications can be developed (see Stokes, 1997) and existing incentive structures and requirements for tenure that place greater value on basic research than on its application (see Giner-Sorolla, 2019).

Another alternative is to be more explicit about our commitment to conducting rigorous research that seeks to extend existing theory while simultaneously being driven by concern about pressing social issues –

which I will henceforth refer to as *socially relevant research*. Deeply rooted in the original founding of social psychology as a subdiscipline (see Lewin, 1947), socially relevant research can be interpreted as representing a “harmonious fusing of both applied and theoretical interests” (Ring, 1967, p. 114) that combines “high scientific standards with a concern for social issues and the problems of the ‘real world’” (Kelman, 1968, p. xi) or that which is “driven by the joint goals of understanding and use” (Stokes, 1997, p. 15).

Many current research programmes provide illustrative examples regarding how scholars can simultaneously pursue the goal of enhancing scientific understanding while taking into account the social relevance and practical utility of scientific insights. Examples range from investigating the drivers and effects of spreading misinformation (e.g., Fazio, 2023; van der Linden, 2024), to identifying effective strategies for boosting organisational diversity (e.g., Onyeador et al., 2021), to understanding how political polarisation might be enhanced or reduced (e.g., Hartman et al., 2022; Moore-Berg et al., 2020), to name just a few. Despite such examples, however, the broader narrative regarding the perceived incompatibility between basic and applied research goals has persisted and continues to permeate our field.

Investigating fears underlying ambivalence about pursuing socially relevant research

My sense is that the underlying perceptions of incompatibility between basic and applied research – whereby academic researchers may feel compelled or forced to choose one over the other – are a number of fears about what we might stand to lose by focusing on social relevance within our research programmes. For instance, as suggested above, when we highlight the social relevance of our research, we may fear that we will lose status or credibility, or otherwise be taken less seriously, as rigorous academic researchers (see Lilienfield, 2012; Moriarty, 2016). We may fear losing control as we attempt to understand complex (and often messy) social processes in less controlled settings in the real world (see Cialdini, 2009). Alternatively, we may feel the contributions we have to offer are minimal or inconsequential in light of the major social problems we face (see Lewis, 2020) or that broader societal forces exert influences beyond what we could realistically address (see Acar et al., 2020). We may also fear losing control over how other people use or interpret our research findings once we begin to “give psychology away” (Kaslow, 2015), where others’ representations of our work could expose us to “the risk of miscommunication” and potentially lead to “inflated claims, wrong claims and oversimplification” (Giner-Sorolla, 2019, p. 22).

At the same time, and given the many demands we face in our lives as academics, we may fear that a focus on social relevance would lead us

to lose precious time and energy that we could reserve for other activities more likely to enhance our chances of professional success (see Giner-Sorolla, 2019; Kaslow, 2015; Watermeyer, 2015). Such fears are understandable given existing reward structures within the academy. Presently, academic institutions tend not to incentivise scholarly activities that constitute pathways to enhanced social relevance, such as those that involve testing the practical application of theory (Berkman & Wilson, 2021; Giner-Sorolla, 2019; Hoffman, 2016; Kirchherr, 2018), and engaging in outreach and dissemination of research findings to maximise public impact (Kaslow, 2015; Omoto, 2012; Schalet et al., 2020).

Regardless of the nature of our fears, however, we psychologists know a great deal about what happens when we fear loss. Typically, when we experience fear of or aversion to loss, we focus more attention on potential negative outcomes, relative to the degree to which we focus on potential positive outcomes (Baumeister et al., 2001; Kahneman & Tversky, 1984). This focus on the potential for negative outcomes also often reflects a fear of the unknown, which commonly inhibits our willingness to explore, experiment, take risks, and try new things (Samuelson & Zeckhauser, 1988; Tversky et al., 1992).

What, then, can we do in response to our fears about loss, at the same time as we may still feel motivated to make our work relevant to social issues? Prior psychological theory and research point to a couple of key strategies. One strategy involves reframing how we think about gains and losses in relation to the social relevance of our work. That is, instead of focusing on *what we might lose* by conducting socially relevant research, we can reframe our thoughts to instead focus on *what we might gain* by conducting socially relevant research – or alternatively, *what we might lose* by *not* conducting socially relevant research (see Tversky & Kahneman, 1990). In the following section, I will elaborate on what academic psychologists stand to gain by focusing on the social relevance of our research, using some of my own academic scholarship and experiences as illustrative examples.

What we stand to gain from focusing on social relevance

Oftentimes, when academic psychologists highlight the value of conducting socially relevant research, there is a prevailing view that this work may be well-intentioned, yet not rigorous, playing into old tropes about ways in which psychological research is a part of the “soft sciences” (Sternheimer, 2012). But in our quest to pursue lines of scientific research that offer ever-greater levels of predictive precision, we run the risk of losing sight of why the research matters, and why we may have chosen to conduct this research in the first place. Idiomatically speaking, we may lose sight of the forest for

the trees, such that a sustained focus on specific psychological processes as component parts may not ultimately translate into greater insights regarding the human condition as a greater whole (see Asch, 1952; Rozin, 2001 for extended discussions). But we as psychologists have much to gain by reflecting on and connecting our research to its broader social relevance across many stages of the scientific process.

Commitment to ensuring the replicability of research findings

As a first point, by enhancing our focus on social relevance, we can cultivate greater commitment to testing the replicability of our research findings. Not only is replication central to the scientific enterprise (see Berg, 2019; Warren, 2018), but it is crucially important for maintaining the health and integrity of psychology as a discipline, and for creating a sturdy foundation onto which future generations of psychological research can be built (see Giner-Sorolla, 2019; Greenwald & Greenwald, 1976). Concerns about the replicability of research findings also have direct implications for realising the potential of our research findings to have real-world impact. If we seek to share scientific knowledge with policymakers and practitioners in order to address social problems and contribute to positive social change, then, at the very least, we need to make sure that we are confident in our results. As Forscher et al. (2020) succinctly state, “sound applications require sound science.”

Such concerns served as a key motivation for the meta-analytic research Thomas Pettigrew and I conducted on intergroup contact effects (see Pettigrew & Tropp, 2006, 2008; Tropp & Pettigrew, 2005a, 2005b), since meta-analyses can be particularly useful for assessing the degree to which research findings do or do not replicate across many studies (Johnson & Eagly, 2000; Rosenthal, 1991; Shrout & Rodgers, 2018). Like so many well-established theories in social psychology (see Asch, 1952), theoretical perspectives on intergroup contact have their roots in tackling pressing social issues (Allport, 1954; Pettigrew, 1988; Williams, 1947). Indeed, decades of intergroup contact research have sought to address the perils of segregation and promote strategies that could effectively reduce intergroup prejudice and facilitate social integration among people from different racial and ethnic backgrounds (e.g., Lemmer & Wagner, 2015; Pettigrew, 2008; Pettigrew & Tropp, 2011; Schofield, 1989). Nonetheless, past reviews of this vast literature had often come to conflicting conclusions regarding the likely effects of intergroup contact. A number of reviews showed general support for the notion that intergroup contact would reduce intergroup prejudice (e.g., Cook, 1984; Harrington & Miller, 1992; Jackson, 1993; Pettigrew, 1998), while others reached more mixed conclusions (e.g., Amir, 1976; Forbes, 1997), and yet others concluded that contact would do little to shift intergroup prejudice (e.g., Ford, 1986; McClendon, 1974; Rothbart & John, 1985).

We therefore pursued a meta-analytic investigation of intergroup contact effects, to determine whether findings across varied contact settings would replicate. Over a period of approximately 6 years (1998–2004), we gathered as many empirical contact studies as we could find. Our view has long been that if we truly wish to understand the nature, contours, and outcomes of intergroup contact, then we should do our best to learn – as conclusively as possible, and in light of the entire existing evidence base – what insights the contact research literature could offer overall. Ultimately, our final meta-analytic dataset included 515 contact studies from the 1940s through 2000, which included data from more than a quarter of a million individual participants in 38 countries (Pettigrew & Tropp, 2006). In more than 90% of the cases, we found that greater levels of intergroup contact corresponded with lower levels of intergroup prejudice. We observed comparable contact-prejudice associations among studies that were conducted in controlled laboratory settings and studies that were conducted in field settings outside of the laboratory (see also Lemmer & Wagner, 2015). Moreover, we observed that associations between contact and prejudice tended to be stronger not only for studies that employed more rigorous research methods but also for those in which the contact was more intimate, and where the contact situation was more intentionally structured to promote prejudice reduction (e.g., by fostering cooperative interdependence and equal status among members of the different groups; Allport, 1954; Pettigrew, 1998). These patterns of findings may be seen as encouraging to both scientific researchers and practitioners, as they suggest that well-designed and thoughtfully implemented contact interventions, tested using rigorous research methods, have considerable potential to replicate desired prejudice-reducing effects (see also International Organization for Migration, 2021; Tropp & Morhayim, 2023).

Understanding both generalizability and variability in effects

By enhancing our focus on social relevance, we can also shift the presumed goals of our research from simply seeking to replicate findings, to understanding how the phenomena we study function, and whether they should or should not be expected to generalise across cases and contexts. Indeed, the more we attend to how social psychological phenomena operate in the real world, the more we should care not only about testing for consistency and replicability but also about identifying and testing possible moderators and boundary conditions for effects – and this is particularly important if we wish to use research insights as a basis for practice and application (see Shrout & Rodgers, 2018). A natural part of the scientific process involves identifying conditions under which a principle or process holds, as well as the conditions under which a principle or process no longer holds, which can generate new

ideas and research directions (*cf.*, Kuhn, 1970; Popper, 1983). By focusing on the broader social relevance of our research, we can come to recognise that knowing which conditions do not yield desired effects is just as valuable as knowing which conditions will facilitate desired effects.

In line with other work seeking to understand how social contexts shape the nature and magnitude of observed effects (Asch, 1952; Pettigrew, 2018), we sought to conduct more extensive analyses with our meta-analytic dataset, to consider both generalisability of and variability in intergroup contact effects (see Rosenthal, 1991). We felt this research extension would help us to draw tighter links between initial tests examining consistency and replicability in contact-prejudice associations and the potential for applying insights from our research within complex social contexts (Omoto, 2012; Pettigrew, 2018). Here, we began by testing contact effects across many different types of intergroup contexts and contact settings, informed by the proliferation of research studies and applied programmes that have sought to use intergroup contact to transform relations between varied groups of people (e.g., racial and ethnic groups, people with and without physical or intellectual disabilities, people with and without mental illness), as well as across varied settings in which contact occurs (e.g., schools, neighbourhoods, workplaces; see Pettigrew & Tropp, 2006). Overall, we observed that while there was some variability in the magnitudes of contact-prejudice associations, the basic patterns of effects were quite similar across varied types of group relations and varied contact settings. Nonetheless, further analyses of the meta-analytic data, as well as subsequent lines of research, have revealed some key moderators of intergroup contact effects.

Differences in group status as a moderator of contact effects

In part, we and others have found that salutary contact effects tend to be weaker among members of minoritized groups who engage in contact than among members of groups that constitute the dominant majority in a given context (Tropp & Pettigrew, 2005b; see also Binder et al., 2009). Further research also suggests that greater perceived discrimination against one's group can undermine the potentially positive effects of intergroup contact among minoritized group members (Tropp, 2007). Once we attend to differences in group status, it is fairly easy to envision reasons why contexts that perpetuate systemic inequalities would curb the potential for intergroup contact to achieve its intended effects (see J. A. Dixon et al., 2005). Given how strongly societal inequalities can affect people's everyday lived experiences as group members (Swim et al., 1998) and long-standing relations between social groups (Bobo, 1999; Richeson & Sommers, 2016), it makes sense that contact would ultimately be shown to have distinct effects among members of groups who occupy different status positions in society. However, uncovering such divergent trends requires that we examine the

perspectives of group members on each side of the interaction, an approach that has only become more common in the last 20 years (see, e.g., Richeson & Shelton, 2007; Tropp, 2006). It is probably no accident that studies of both groups' perspectives on intergroup contact have become more numerous in recent decades, just as contact research has expanded to address new themes beyond prejudice reduction, which had traditionally focused only on the perspectives of dominant groups (Dixon, Tropp, et al., 2010; Tropp et al., in press).

This growing body of research indicates that greater contact typically corresponds with greater support for collective action and social change to promote intergroup equality among members of dominant groups, whereas it is typically associated with less support for collective action and social change towards equality among members of minority groups (see, e.g., Hässler et al., 2020; Saguy et al., 2011; Tropp et al., 2012; Wright & Lubensky, 2009). Such findings have led a number of scholars to conclude that – in our efforts to improve intergroup attitudes and pursue social harmony through intergroup contact – we may have underemphasised (and understudied) the likely downstream consequences of contact for achieving intergroup equality (see, e.g., J. Dixon et al., 2012).

Not only are these new developments valuable and important, but they have served as the springboard for further theoretical growth and insight. For instance, researchers are now investigating the types of intergroup contact that may keep intergroup contact from inhibiting motivations for collective action and social change. Promising lines of work in this area show that when members of minoritized groups engage in contact with members of dominant groups who clearly support intergroup equality and see existing inequalities as illegitimate, intergroup contact no longer weakens their motivation for social change (e.g., Becker et al., 2014; Droogendyk et al., 2016).

In light of these new developments, I have found that some of my own research and grown to examine how we might simultaneously work towards the dual goals of improving relations between groups *and* promoting greater support for intergroup equality, rather than necessarily choosing only one of these goals to the relative neglect of the other (see, e.g., Schreiber et al., 2024; Tropp & Dehron, 2023). As I present on this work at research conferences, I have been struck by the number of times I've been asked some variant of the following question: *“how do you reconcile your beliefs in the merits of intergroup contact with research showing that it could potentially inhibit support for social change?”* My response to questions like these is typically two-fold. Part of my response involves simply acknowledging that there is broad empirical support for both trends and that these divergent patterns suggest meaningfully distinct implications for members of dominant and minoritised groups. A second part of my response reflects a feeling that, in many ways, the question itself is missing the point. In my view, the ultimate goal of

research is not necessarily to seek absolute confirmation for any particular theory, but rather to enhance our understanding of how core processes manifest and function in the real world (see also Pettigrew, 2018). In this light, then, any new discoveries that provide useful insights are worth heeding, and anyone who truly cares about their relevance to social issues would do well to learn from them.

Societal inequality and conflict as moderators of contact effects

Relatedly, greater attention to existing societal conditions and prevailing intergroup divisions can inform whether intergroup contact may (or may not) be usefully pursued as a strategy for reducing prejudice and improving relations between groups. For instance, until recently, most of the contact literature had focused on studying relations between groups in fairly peaceful contexts, where relations between groups have been relatively stable and without protracted conflict, or long after violent intergroup conflict has dissipated and groups appeared more willing to begin processes of reconciliation (see Tropp, 2015; Wagner & Hewstone, 2012). Yet it is possible that such a limited sampling of cases could mask boundary conditions for some of contact's effects; indeed, countervailing forces such as protracted conflict and structural inequality would understandably render contact less effective in yielding and sustaining desired contact outcomes, as compared to contexts where such obstacles do not exist (see Tropp et al., 2022 for a related argument).

Some emerging lines of work have extended the bounds of the contact research literature to examine these very issues. For instance, Mousa (2020) examined the effects of contact between Iraqi Christians and Muslims in Iraq, a context in which relations between groups have been volatile and fragile (see Bobseine, 2019). Iraqi Christians who had been displaced by the Islamic State of Iraq and Syria (ISIS) were assigned either to an all-Christian soccer team or to a soccer team where they would have contact with both Christians and Muslims. Results showed that having contact with Muslims enhanced Iraqi Christians' tolerance towards their Muslim peers within the context of the intervention, yet contact was much less effective in building fostering positive attitudes towards Muslims outside of the intervention context. Similarly, Israeli and Palestinian youth have been shown to develop positive attitudes towards and friendships with each other through contact as part of coexistence camp programmes; yet, these encouraging outcomes began to wane and became harder to sustain after they returned to their home communities (Schroeder & Risen, 2016). These examples clearly illustrate how contexts of prolonged, violent conflicts between groups are likely to challenge or undermine any latent potential for contact to foster positive intergroup attitudes (see also Paluck et al., 2019; Tropp & Dehron, 2023).

Associations between contact and intergroup attitudes have also been studied across contexts that vary in degrees of structural inequality. Spearheading this work, Kende et al. (2018) investigated contact effect sizes from our 2006 meta-analytic dataset in relation to societal indicators of inequality. Results indicated that links between contact and prejudice reduction were more pronounced in studies conducted in more egalitarian societies, as compared to effects observed for studies conducted in more unequal societies. Kende et al. (2025) have continued to focus on how context-level factors shape contact outcomes, by analysing how host society members' perceptions of immigrants are shaped by both their contact experiences and local immigration policies. Drawing on data from nine countries across Europe and North America, these authors show that more inclusive immigrant integration policies predicted greater numbers of friendships with immigrants; and, in turn, these greater friendships predicted more positive perceptions of immigrants among members of the host society. These findings nicely complement those of other researchers who incorporate contextual moderators into their analyses of intergroup contact (Christ et al., 2014; Pettigrew, 2021), revealing ways in which prevailing social norms and societal structures can contribute either to strengthening or weakening contact effects (e.g., Christ et al., 2010; De Tezanos-Pinto et al., 2010; Wagner et al., 2006).

Broadening the scope of research questions

Another benefit of focusing on social relevance is that it can push us to broaden the scope of the research questions we ask, in light of the novel insights we gain from testing for variability in effects across social conditions and societal contexts. Rather than relying only on existing theory to inform our work, a focus on social relevance may help us to make new connections and see new implications for application, beyond what has already been studied or addressed in the research literature (see also Tropp & Mallett, 2011). A particularly compelling example of this approach is offered by Dixon, Durrheim, and their colleagues (J. A. Dixon et al., 2012, 2007) who noted that a key limitation of the contact literature has been its traditional focus on prejudice reduction, with little attention to support for policies that might curb inequality and the effects of inequality on historically disadvantaged groups. Convinced by their arguments and fuelled by a sense of informed curiosity (Rozin, 2001), I collaborated with these authors on a national survey in South Africa, through which we jointly examined interracial contact, racial prejudice, and racial threat as possible predictors of White South Africans' support for social policies that would benefit Black South Africans (Dixon, Tredoux, et al., 2010). Even after taking racial prejudice and racial threat into account, we found that White South Africans' positive contact experiences with Black South Africans uniquely predicted

lower levels of opposition to policies that would serve the educational, professional, and economic interests of Black South Africans.

Related programmes of research indicate that greater contact between groups not only improves intergroup attitudes but can also offer pathways towards societal transformation. For instance, greater contact has been shown to promote a greater willingness to compromise and make reparations to Palestinians among Jewish Israelis (Maoz & Ellis, 2008), greater willingness to forgive and participate in reconciliation efforts among Protestants and Catholics in Northern Ireland (Tam et al., 2008; Tropp et al., 2017), greater beliefs in the possibility of reconciliation and in the value of integration among ethnic Albanian and Serb youth in Kosovo (Morhayim et al., 2024), greater trust and willingness for integration among survivors and perpetrators of genocide in Rwanda (Dehron et al., 2022), and greater feelings of security in the midst of violent conflicts between farmers and herders in Nigeria (Grady et al., 2023). In the US, we have also found that the more White Americans experience positive contact with Black Americans, the more they support and participate in collective action for racial justice (Selvanathan et al., 2018; Tropp & Uluğ, 2019); these findings have since been replicated with data from members of historically advantaged and disadvantaged racial groups in a dozen countries, using many different indicators to assess intergroup contact and support for social change towards greater equality (Hässler et al., 2020).

Taken together, these branches of research suggest that, while prejudice reduction has long been the primary focus of the contact literature, the positive impacts of intergroup contact can potentially extend far beyond merely shifting intergroup attitudes. Such findings have led us to reflect in recent years about how contact may transform us beyond reducing our prejudices – like compelling us to care more about the experiences, perspectives, and interests of other groups whom we come to know (Tropp & Barlow, 2018). Increasingly, newer generations of contact research have granted growing attention to group members' lived experiences in settings where relations between groups are deeply shaped by inequality and conflict (see, e.g., Dixon, Tropp, et al., 2010; Tropp, 2006) while seeking further research extensions and exploring the potential for real-world application (see Tropp & Dehron, 2023).

Concluding thoughts

To conclude, as academic psychologists, many of us have experienced ambivalence regarding how we characterise our research – often feeling compelled to distinguish between doing basic versus applied work, between doing good science versus making a difference, or between conducting rigorous tests of extant theory versus understanding how

psychological processes operate in the real world. By viewing our work through such dichotomous, “either-or” frames, we tend to feel forced to make a choice – and likely a false one at that. Alternatively, both as individual researchers and as a field, we can choose to adopt more of a “both-and” perspective (see Jones & Dovidio, 2018; Tropp & Dehrone, 2023), recognising what we stand to gain by focusing on questions of social relevance and framing socially relevant research questions as extensions of existing theory and using rigorous scientific methods to test them. In the words of Berkman and Wilson (2021), “there is no logical inconsistency between theory development and problem solving – both can be done at once” (p. 5).

In doing so, we may find ourselves in a better position to link empirical research to real-world phenomena and make our work useful to policymakers, practitioners, and members of the broader public who might benefit from it. The next challenge, then, involves determining the most effective strategies and pathways through which we might have real-world impact (see Giner-Sorolla, 2019; Kaslow, 2015). For many academics, the prospect of “having real-world impact” may seem daunting, especially as we think about it in relation to all the other tasks that demand our time and attention in our multi-faceted careers as academics. But for many of us, the social relevance of the work we do is what brought us into this field in the first place; and, finding ways to make our work useful – by improving people’s lives, or somehow contributing to the public good – can give our work its own unique sense of meaning and offer its own rewards.

How we as academic psychologists choose to enact having real-world impact may look very different depending on who we are, what other professional goals we have, and the types of relationships we seek to have with the public audiences we aim to reach (see Pielke, 2007; Schalet et al., 2020). Although some would regard “translational efforts [as] a component of psychology’s social contract with society” (Kaslow, 2015, p. 368), ultimately, each of us must determine for ourselves if or how having public impact fits as part of our professional trajectory.

For those of us so inclined, there are many resources available to guide us, and many questions we might ask ourselves along the way, as we figure out what we have to say, how to convey what we know, and with whom relevant information should be shared (see, e.g., Badgett, 2016; Baron, 2010; Sucharov, 2019; Tropp, 2018).

In my view, the potential for academic psychologists to have public impact by conducting socially relevant research that can be used to address social issues and promote positive social change is less an issue of capacity than it is of will. Collectively, as a discipline, we have only begun to cultivate the presence and influence that we could conceivably

have in the public sphere. By focusing on all we stand to gain from highlighting the social relevance of our work, and envisioning the roles we might play in having real-world impact, my hope is that more of us will feel encouraged and emboldened to pursue research that can make a meaningful difference in the world.

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