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Relative Deprivation: A Theoretical and Meta-Analytic Review

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Abstract

Relative deprivation (RD) is the judgment that one is worse off compared to some standard accompanied by feelings of anger and resentment. Social scientists use RD to predict a wide range of significant outcome variables: collective action, individual achievement and deviance, intergroup attitudes, and physical and mental health. But the results are often weak and inconsistent. The authors draw on a theoretical and meta-analytic review (210 studies composing 293 independent samples, 421 tests, and 186,073 respondents) to present a model that integrates group and individual RD. RD measures that (a) include justice-related affect, (b) match the outcome level of analysis, and (c) use higher quality measures yield significantly stronger relationships. Future research should focus on appropriate RD measurement, angry resentment, and the inclusion of theoretically relevant situational appraisals. Such methodological improvements would revitalize RD as a useful social psychological predictor of a wide range of important individual and social processes.

Keywords

relative deprivation, meta-analysis, affect, protest participation, deviance, health, prejudice, social comparison, social justice

> A house may be large or small; as long as the neighboring houses are likewise small, it satisfies all social requirements for a residence. But let there arise next to the little house a palace, and the little house shrinks to a hut. The little house now makes it clear that its inmate has no social position at all to maintain.
>
> Marx, *Wage, Labour and Capital* (1847/1935)

Marx (1847/1935) captures the intuitive appeal of relative deprivation (RD) as an explanation for social behavior. If comparisons to other people, groups, or even themselves at different points in time lead people to believe that they do not have what they deserve, they will be angry and resentful. RD describes these subjective evaluations.

Thus, RD is a social psychological concept par excellence. It postulates a subjective state that shapes emotions, cognitions, and behavior. It links the individual with the interpersonal and intergroup levels of analysis. It melds easily with other social psychological processes to provide more integrative theory—a prime disciplinary need (Pettigrew, 1991). Moreover, RD challenges conventional wisdom about the importance of absolute deprivation for collective action, individual deviance, and physical health. It also has proven useful in a wide range of areas. Researchers have invoked RD to explain phenomena ranging from poor physical health (Adler, Epel, Castellazzo, & Ickovics, 2000) to participation in collective protest (Newton, Mann, & Geary, 1980) and even to susceptibility to terrorist recruitment (Moghaddam, 2005). Indeed, the concept has been used throughout the social sciences (Walker & Smith, 2002), from criminology (e.g., Lea & Young, 1984/1993) and economics (e.g., Yitzhaki, 1979) to political science (e.g., Lichbach, 1990) and history (e.g., Snyder & Tilly, 1972).

Some investigations support RD models (e.g., Abrams & Grant, in press; Leach, Iyer, & Pedersen, 2007; Pettigrew et al., 2008; Pettigrew & Meertens, 1995; Vanneman & Pettigrew, 1972; Walker & Mann, 1987), but others do not (e.g., Gaskell & Smith, 1984; Macleod, Smith, Metcalfe, & Hart, 2005; Schmitt, Maes, & Widaman, 2010; Snyder & Tilly, 1972; Thompson, 1989). In response to these inconsistencies, several previous literature reviews have sought to clarify the theoretical antecedents and components of the concept (Crosby, 1976; Martin, 1986a; Walker & Pettigrew, 1984). Other reviews, however, dismiss its value altogether (Brush, 1996; Finkel & Rule, 1986; Gurney & Tierney, 1982). We believe such dismissals of the concept are premature.

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The purpose of this review is twofold. First, we present the basic structure of RD theory. This outline allows us to assess systematically the degree of support for the hypotheses that lie at the heart of RD theory. We propose that RD measures that more closely match the theoretical conceptualization that we outline below more strongly predict attitudes and behavior. Second, to test this contention across an array of outcome variables, this article offers a quantitative literature review of the relevant social science research from 1949 to January 2010. Unlike traditional qualitative literature reviews, a meta-analytic integration of research results enables us to test the basic theory and determine whether the RD effects are as weak or nonexistent as some critics claim.[1]

History of the Concept

Samuel Stouffer (Stouffer, Suchman, DeVinney, Star, & Williams, 1949) coined RD to describe unexpected relationships that emerged from surveys of American soldiers in World War II. He found that U.S. Army Air corpsman reported more frustration over promotions in comparison to the military police even though they enjoyed a much faster rate of promotion. Stouffer maintained that the military police were not the relevant comparison for these airmen; within their Air Corps group, they knew many similar peers who had been promoted. He hypothesized that it was comparisons to these peers that produced RD. *The American Soldier* researchers did not measure RD directly; rather, they inferred it as a post hoc explanation. This failure to initiate a prototype measure has led to literally hundreds of diverse and sometimes conflicting measures that have bedeviled RD research ever since.

After Stouffer introduced RD, Merton (1957; Merton & Kitt, 1950) enlarged the idea within a reference group framework. This work led Pettigrew (1967) to point out that RD was one of a large family of concepts and theories that employed relative comparisons in sociological and psychological social psychology. Runciman (1966) further broadened the RD construct by distinguishing between egoistic (individual) and fraternal (group) RD. A person could believe that she or he is personally deprived (individual RD or IRD) or that a social group to which she or he belongs is deprived (group RD or GRD). Feelings of GRD should be associated with ingroup-serving attitudes and behavior such as collective action and outgroup prejudice, whereas IRD should be associated with individual-serving attitudes and behavior such as academic achievement and property crime.

During the following decades, scholars incorporated RD into larger models of social comparison, casual attribution, equity, and social identity theory (e.g., Crosby, 1982; Folger, 1987; Mummendey, Kessler, Klink, & Mielke, 1999; Walker & Pettigrew, 1984). For example, social identity research shows that people will experience GRD if the intergroup situation is viewed as illegitimate and unlikely to improve without collective challenge and group boundaries are seen as impermeable (Mummendey et al., 1999). Although social psychological RD research has focused on intergroup and interpersonal (upward and contrasting) social comparisons, political science RD research has focused on people's comparisons of their present situation with either their past, future, desired, or deserved selves (e.g., de la Sablonnière, Taylor, Perozzo, & Sadykova, 2000; de la Sablonnière, Tougas, & Lortie-Lussier, 2009; Gurr, 1970).

Defining RD

The intuitive explanatory appeal of RD has led to its use across numerous social science disciplines but with a bewildering variety of construct names and theoretical frameworks. Therefore, the first task is to establish a consistent and basic definition of what we mean by RD. In schematic terms, Figure 1 presents the model that guides our analysis.

We define RD in terms of three steps. First, there must be comparisons made by an individual. If one does not compare, there can be no RD. Second, there must be a cognitive appraisal that leads the individual to perceive that the individual or his/her ingroup is at a disadvantage. This perceived comparative disadvantage distinguishes RD from the earlier frustration-aggression hypothesis and other noncomparative models of social justice and discrimination. Third, the perceived disadvantage must be viewed as unfair. The perceiver thinks the perceiver or his/her ingroup deserves better, and this results in angry resentment. This component is an indispensable component of RD. If my better educated neighbor has greater income and a larger home, I still could think that it is not unfair, and thus no sense of RD results.

Note other salient features of Figure 1. First, RD theory concerns *individuals*—their comparisons, appraisals, and affect. This feature means that data to test the theory must come from individuals directly—a requirement overlooked by much of the research literature that purports to test RD. Second, three types of comparisons are delineated. Two are the types that Runciman (1966) usefully classified—interpersonal comparisons with ingroup members (IRD) and intergroup comparisons (GRD). These types also can include comparisons of oneself or one's ingroup to the past or future. We also list an additional type of comparison in which people compare their situation to an *outgroup member.* Third, each comparison type is linked to different types of outcomes. In other words, there should be a *fit* between the comparison level and the level of the outcomes. Thus, IRD is linked primarily to outcomes at the interpersonal level, and GRD is linked primarily to outcomes at the intergroup level. IRD can influence group-level outcomes, but—as noted in Figure 1—such influences are largely mediated through increased GRD. We return to this point later.

Repeated research demonstrates that it is that GRD promotes support for political protest (Pettigrew, 1967; Pettigrew et al., 2008; Walker & Mann, 1987) and outgroup prejudice (Pettigrew & Meertens, 1995; Vanneman &

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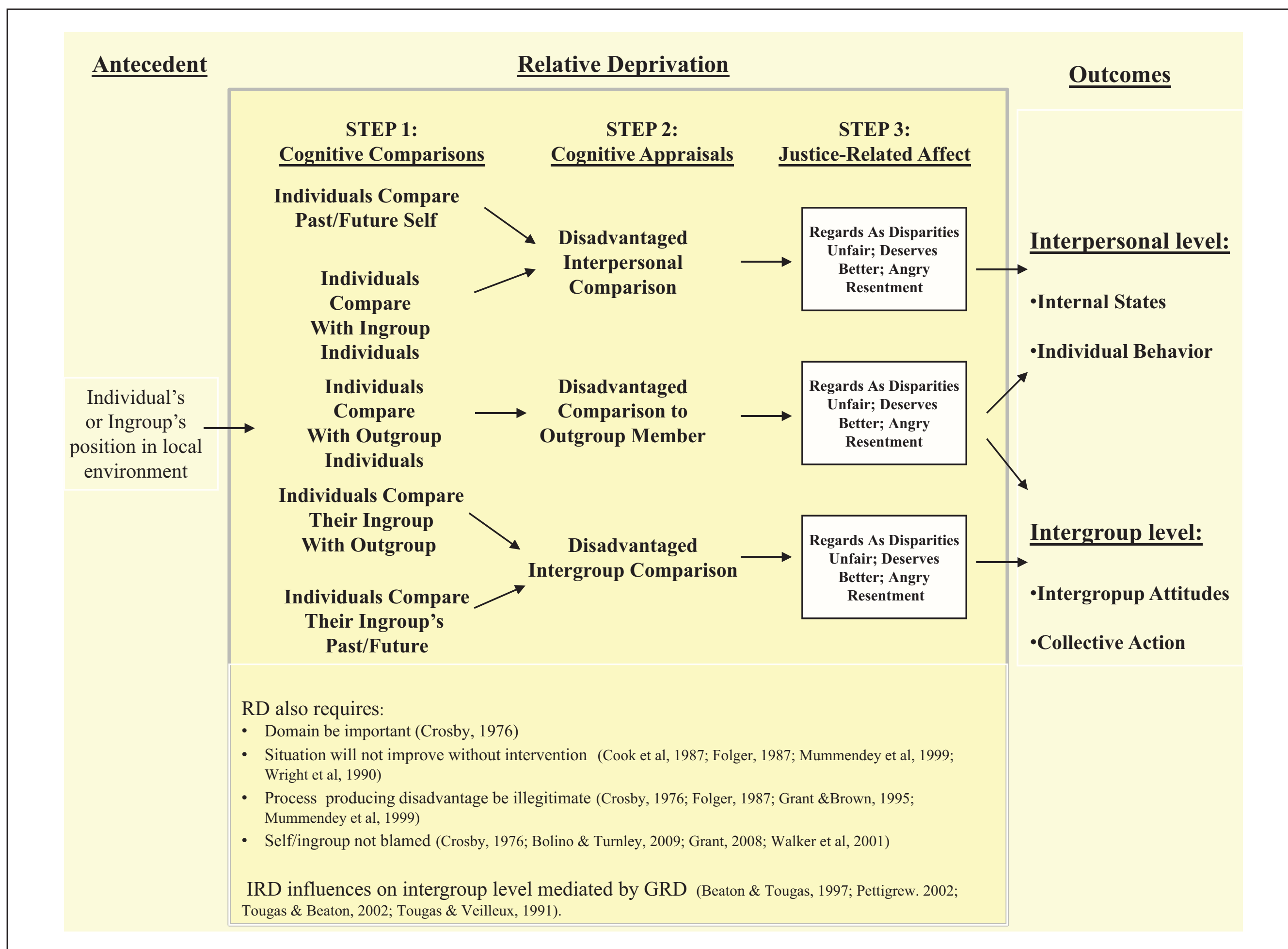

Figure 1. Outline of relative deprivation theory
Terms not in bold are outside the scope of our meta-analysis.

Pettigrew, 1972). Yet researchers often ignore this distinction (Walker & Pettigrew, 1984)—for example, by using interpersonal comparisons to predict collective behavior (e.g., Long, 1975; Newton et al., 1980; Useem, 1975). Feeling deprived may inspire participation in collective behavior, but only if the person feels deprived on behalf of a relevant reference group. In contrast, IRD should predict individual behavior (e.g., "moonlighting" to earn extra money, stealing, using drugs). Unfortunately, previous RD reviews have either excluded (Bolino & Turnley, 2009; Cook, Crosby, & Hennigan, 1977; Crosby, 1976; Toh & Denisi, 2003) or ignored (Finkel & Rule, 1986; McPhail, 1971) research that distinguishes between IRD and GRD. For this and additional reasons, we suspect the conclusions of some earlier literature reviews are unduly pessimistic (e.g., Finkel & Rule, 1986; Gurney & Tierney, 1982; McPhail, 1971).

The most interesting, but rarely discussed, case involves a comparison to an outgroup member. Our understanding of RD theory holds that such comparisons, if regarded as unfair, can result in either interpersonal or intergroup outcomes depending on the context. If the perceiver is close to the outgroup comparison target and perhaps sees her or him as a friend, she or he should experience IRD. But if the perceiver thinks of the comparison target as an outgroup representative, she or he should experience GRD. Note our assumption that GRD requires people to view themselves as representative group members in comparison to an outgroup (also see Ellemers, 2002; Kawakami & Dion, 1993). Otherwise, it is unlikely they will adopt the psychological perspective that leads to ingroup-serving attitudes and behavior (see Turner, 1999).

Inclusion Guidelines for the RD Meta-Analysis

Disadvantaged comparisons. This delineation of RD theory establishes the guidelines for our meta-analytic review of the RD research literature. Figure 1 indicates two requirements for effective RD measures. The first requirement is a disadvantaged comparison. These comparisons can include questions in which people contrast their situation with another

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person, group, or even themselves at another point in time. Therefore, our data set includes any measure of relevant subjective comparisons that we could find even if the researchers did not identify the construct as an RD measure. For example, in an investigation of television viewing habits, Yang, Ramasubramanian, and Oliver (2008) measured dissatisfaction with one's personal life with comparisons between the participant and the accomplishments of similar others. In a series of important investigations of physical health, Adler and her colleagues measured subjective social status by asking adolescents and adults to place themselves on a ladder in which the top rung represents the most successful and respected members of various reference groups and the bottom rung represents the least successful and respected members of various reference groups (see Adler, 2009, for a review). To measure employee envy, Vecchio (2000) asked employees to indicate the degree to which "Most of my coworkers have it better than I do" and "My supervisor values the efforts of others more than he/she values my efforts." We believe that all these measures qualify as potential RD measures.

However, this requirement also means that studies that purport to use RD as a theoretical framework but do not explicitly measure disadvantaged comparisons are not included. For example, in one of the most often cited early RD articles, Davis (1966) used the item "I have a flair for biology" as a subjective measure of academic success to illustrate the relative difference between being a big fish in a little pond and a small fish in a larger, more competitive pond. Davis assumed that agreement with the item was more likely when feelings of RD were less frequent. In surveys of Afrikaners during the demise of apartheid, Appelgryn and Nieuwoudt (1988) asked White Afrikaners and Black South Africans to rate whether their personal and ingroup political, social, and economic situations were just, but they did not ask respondents to make these ratings in comparison to any particular standard or referent. Although direct questions about injustice can be significant predictors of collective action (see van Zomeren, Postmes, & Spears, 2008), they do not capture the essential RD argument—*people's reactions to their circumstance depend on the comparisons they do or do not make*.

This requirement also means that we do not include a long tradition of RD research in which researchers infer RD via aggregate measures of such variables as income inequality. As Figure 1 emphasizes, an *individual's* sense of RD is critical to RD theory. For example, Gurr measured economic RD with (a) short term trends in the amount of exports and imports, (b) cost of living increases, (c) GNP growth rates, and (d) summaries of news stories about crop failures, unemployment, and other adverse economic conditions (Gurr, 1970; Gurr & Duvall, 1973). Similarly, Feierabend, Feierabend, and Nesvold (1969) measured RD by combining a country's gross national product (GNP), the caloric intake per capita, the number of physicians, telephones, newspapers, and radios per unit of population, degree of literacy, and urbanization.

Such studies uncover many intriguing results, but they are not testing RD theory. From a RD perspective, these studies commit the ubiquitous ecological fallacy. This fallacy draws conclusions about individuals from macro-level data alone—a mistake often seen in statements made about individual voters based on aggregate voting results (Pettigrew, 1996, 2006). It is a fallacy because macro units are too broad to determine individual data and individuals have unique properties that cannot be safely inferred from macro-level data alone.

This RD requirement for data from individuals also excludes research in which RD measures are constructed from objective demographic characteristics. For example, Boyce, Brown, and Moore (2010) drew on a representative longitudinal sample of British households to estimate participants' life satisfaction. They compared participants' self-reported income to the income of reference groups constructed by averaging the incomes of participants in similar geographic regions, age groups, and education levels to show that one's relative rank within an income reference group predicts life satisfaction. Epidemiologists use similar measures to uncover relationships between relative income and (a) migration patterns (Stark & Taylor, 1999) and (b) mortality rates and physical health (e.g., D'Ambrosio & Frick, 2007, Eibner, Sturm, & Gresenz, 2004; Reagan, Salsberry, & Olsen, 2007; Siahpush et al., 2006). In one particularly interesting study, Bernburg, Thorlindsson, and Sigfusdottir (2009) report a hierarchical linear model interaction effect in which adolescents' assessments of their economic deprivation are more predictive of their individual attitudes and behavior the *less* economically deprived the local neighborhood. The authors argue that adolescents in this situation are more likely to make unfavorable upward social comparisons.

Fascinating as this work is, it does not directly test RD theory. These measures are constructed from individual demographic characteristics and move from measures of people's position in the local environment (an antecedent condition) to specific outcomes without measuring the intervening experience as diagramed in Figure 1. We cannot know from these data whether participants made or reacted to the same comparisons that researchers constructed from various geographic, gender, and occupational groups.

Still, it may seem obvious that local position should inform people's assessments of their situation. After all, as suggested by Marx (1847/1935), people appear to notice the small size of their house only when a larger house is built next door. However, one's place in the local environment does not straightforwardly predict comparison choices or interpretation (see Gartrell, 2002; Leach & Smith, 2006; Seaton et al., 2008). Even though sanitation workers in Cambridge, Massachusetts, regularly picked up garbage from homes in wealthy neighborhoods, they did not view the homeowners as relevant comparisons for their own incomes (Gartrell, 2002). Furthermore, even if people are aware of another person's better fortune, they may not interpret it as reason for envy

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or resentment. For example, poor villagers in Malawi reported that better-off friends and neighbors offer a form of social support that is valued and necessary and not the object of resentment or jealousy. A neighbor's increased resources can mean opportunities for work and protection against shared financial hardship and risk (Ravallion & Lokshin, 2010).

A rich set of qualitative and historical analyses illustrates how complex and nuanced the relationship between objective deprivation and the subjective experience of disadvantage can be. For example, Shedd (2008), in her analysis of the failure of the 1996 Russo-Chechen peace process, argues that the fall of the Soviet Union dramatically increased Chechens' expectations. When the post–Soviet Union political reality failed to deliver the anticipated political and economic benefits, Shedd argues, increased RD made it impossible for the peace process to succeed. In his ethnographic study of young people's criminal activity in a small town near London, Webber (2010) argues that it is the arrests and citations that British youth incur when they are young adolescents that lead to increased RD when these young people move closer to the labor market. Arrests and citations make it much more difficult for them to find the jobs to support the lifestyle they desire. Finally, Duncan (2010) uses the life story of Ingo Hasselbach**,** a former German neo-Nazi leader, to illustrate how White Germans who perceive themselves to be under attack, threatened, and (relatively) deprived of political power adopt extreme political ideologies, even if objective political reality suggests they have ample political power. Unfortunately, these intriguing analyses do not include an empirical RD measure, so they lie outside the scope of our analysis.

Justice-related affect. The second requirement is that people compare their situation to another possibility using principles about what "ought to be." It is this emphasis on entitlement or "deservingness," Step 3 in Figure 1, that distinguishes RD from other psychological theories that hold that people's hedonic reactions occur through the comparison of experiences to particular referent points (e.g., adaptation-level theory—Helson, 1964; anchoring and adjustment—Kahneman & Tversky, 1982; prospect theory—Kahneman, 1992).

Two different research traditions, both included in our meta-analyses, illustrate the role of deservingness, broadly defined, in RD. The first tradition represents largely political science research that focuses on people's comparisons with themselves at different points in time (e.g., Davies, 1962; Feierabend et al., 1969; Gurr, 1970). When the current situation violates expectations created by past experiences, people are held to feel politically alienated and are more likely to participate in collective protests (e.g., Gurr, 1970; Herring, 1989; Newton et al., 1980).

Yet researchers in this tradition often use measures of aspirations as a proxy for violated expectations (Gurney & Tierney, 1982). For example, a popular method for measuring RD, the Cantril–Kilpatrick Self-Anchoring Scale (Cantril, 1965), has respondents place themselves on a 10-step ladder with the top rung labeled as the best possible life and the bottom rung as the worst possible life. When defined in this way, this scale measures discrepancies between people's attainments and aspirations; but it does not measure discrepancies between their expectations as to what they *deserve* and their current situation (Finkel & Rule, 1986).

Even researchers who do not use the Cantril–Kilpatrick Scale often blur the difference between what is considered just and expected (e.g., Martin, 1986b). This is a critical distinction. Although researchers may not make these distinctions, research participants do. When employees at a manufacturing company were asked to create two pay plans, one based on what they expect and the other on what is just, they produced significantly different distributions (Martin, 1986b).

The second tradition embodies mostly social psychological research that focuses on people's comparisons to other people and groups (see Walker & Smith, 2002, for a review). RD theorists (Crosby, 1976; Folger, 1987; Martin, 1986a; Runciman, 1966; Walker & Pettigrew, 1984) specify anger and resentment as the essential affective correlates of RD. Yet many researchers use perceptions of the magnitude of differences between one's own situation and a relative standard to indicate RD and neglect emotional reactions altogether. However, as research within the system justification tradition makes clear (Jost, Kay, & Thorisdottir, 2009), people often recognize and accept their relative disadvantage as appropriate. Therefore, there is no guarantee that if people recognize a comparative disadvantage, they will feel angry and resentful.

For this review, we define Step 3's justice-related affect very broadly. One could characterize RD as a relational theme (Lazarus, 2001) or interconnected beliefs (Kessler & Mummendey, 2002) in which deserving, anger, and resentment are key characteristics of a larger narrative (see Folger, 1987). Cognitive appraisal research on emotions also shows that judgments of unfairness are closely tied to interpersonal accounts of anger (Averill, 1983; Frijda, Kuipers, & ter Schure, 1989). Although we recognize that anger does not always indicate injustice (O'Mara, Jackson, Batson, & Gaertner, 2011), it is the emotion most clearly associated with justice violations (see Feather, 2006). In particular, Feather (2006) reviews several experimental studies in which he documents (a) the close relationship among violations of deserving, anger, and resentment and (b) reciprocal causation between these constructs. Therefore, we are confident that affective RD measures (including measures of general negative affect, frustration, dissatisfaction, discontent, and disappointment) capture the notion of deservingness and entitlement that are at the core of RD.[2]

Our definition means that we exclude studies in which researchers define RD as the awareness of differences in a particular domain and the dependent measure as the resulting feelings about these differences. As illustrated in Figure 1, both measures represent aspects of the RD experience. For

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example, researchers used participants' ratings of the fairness of a particular comparison to predict their satisfaction with the comparison (e.g., Austin, McGinn, & Susmilch, 1980; Messe & Watts, 1983). Similarly, other researchers use relative discrepancies in material goods or income to predict income satisfaction (Crawford Solberg, Diener, Wirtz, Lucas, & Oishi, 2002; G. Johnson & Johnson, 1999). As one might expect from our model, the average correlation between subjective measures of relative income comparisons and income satisfaction is quite high ($r = +.67$).

Our review of the social psychological RD tradition also suggests four other key characteristics of the RD experience (see Figure 1). First, people obviously must care about what they lack. As Crosby (1982) writes, RD involves wanting as well as deserving. Second, people must believe that the current situation is unlikely to change without intervention (e.g., Folger's, 1987, likelihood of amelioration or Cook et al.'s, 1977, definition of feasibility). Otherwise, the possibility of improvement can temper anger and increase hope for the future (Crosby, 1976; Davies, 1962; Folger, 1987; Gurr, 1970). Third, people must *not* see themselves as responsible or to blame for the deprivation (Bolino & Turnley, 2009; Crosby, 1976; also see Grant, 2008; Walker, Wong, & Kretzschmar, 2002). Fourth, people must view the process that produced the deprivation as illegitimate (e.g., Ellemers, 2002; Folger, 1987; Mummendey et al., 1999). Although we list these additional characteristics in Figure 1, these appraisals were rarely independently measured in the research that we reviewed. Therefore, evaluation of measures with or without these additional appraisals is outside the scope of our meta-analysis.

Reactions to Relative Deprivation: The Outcome Variables

Although reviews of RD research center largely on whether RD offers an adequate explanation for participation in collective action (e.g., Brush, 1996; Finkel & Rule, 1986; Gurney & Tierney, 1982), collective action is just one of many possible responses to feeling deprived (see Figure 1). Previous theoretical analyses of RD reactions (Crosby, 1976; Kawakami & Dion, 1993; LaLonde & Silverman, 1994; Mark & Folger, 1984; Wright, 1997) make two critical distinctions. First, RD reactions can represent intentions and behaviors to improve one's personal situation, or they can represent intentions and behaviors to improve the situation for one's reference group (Ellemers, 2002; LaLonde & Silverman, 1994; Wright, 1997). Second, these frameworks distinguish among internal states (such as anxiety, depression, and attitudes toward the self), attitudes (toward the system and other groups), and actual behavior (Crosby, 1976; Mark & Folger, 1984).

We draw on these two distinctions to organize the RD literature into the four classes of outcome variables shown in Figure 1.[3] The first category of research includes *collective behavior*, the primary focus of early RD work and the recipient of the most severe criticisms. These outcome measures include indicators of self-reported rioting (e.g., Caplan & Paige, 1968), intentionally sabotaging job performance (e.g., Olson, Roese, Meen, & Robertson, 1995), readiness to block a road (e.g., Kelly & Breinlinger, 1996), readiness to block bulldozers or spike trees (e.g., Wong & Walker, 1994), readiness to approve violent politics or civil disobedience (e.g., Issac, Mutran, & Stryker, 1980), and willingness to sign petitions, join strikes, or shop selectively (e.g., Grant, 2008; Smith, Cronin, & Kessler, 2008).

The second category entails studies of *intergroup attitudes* that include attitudes toward political policies such as affirmative action and immigration, prejudice toward outgroups, and ingroup identification and bias. Attitudes toward outgroups include measures of blatant and subtle prejudice (e.g., Pettigrew & Meertens, 1995), outgroup stereotyping (e.g., Kelly & Kelly, 1994), and majority group members' attitudes toward affirmative action (e.g., Tougas & Veilleux, 1990). Attitudes toward the social system include belief in a just world (e.g., Corning, 2000), the trustworthiness and legitimacy of political institutions (e.g., Klandermans, Roefs, & Olivier, 2001), and organizational commitment (e.g., Feldman, Leana, & Bolino, 2002). Attitudes toward the ingroup include measures of ingroup identification (e.g., Pettigrew et al., 2008), nationalism (e.g., Moore, 2008), and ingroup bias (e.g., Boen & Vanbeselaere, 2002). In general, these first two categories of outcomes represent attitudes, intentions, and behaviors that focus on one's ingroup's situation.

The third category of outcomes involves *individually oriented behaviors*, both normative and nonnormative. Deviant behaviors include an official record of delinquency (Reiss & Rhodes, 1963), a self-report measure of the propensity to date rape (Boeringer, 1992), absence from work (Geurts, Buunk, & Schaufeli, 1994), bullying (Breivik & Olweus, 2006), and crime (Stiles, Liu, & Kaplan, 2000). Achievement behaviors include self-reported activities for church committees (Jun, 1991), moonlighting (Wilensky, 1963), engagement in academic activities (Wosinski, 1988), and participating in individual professional development activities (Zoogah, 2010). Escape behaviors include physical exercise (Kasimatis, 1993), smoking (Dijkstra & Borlan, 2003), drinking alcohol and other drug use (Baron, 2004), gambling (Callan, Ellard, Shead, & Hodgins, 2008), and watching television (Yang et al., 2008). In general, this category of outcomes represents efforts to cope with one's individual situation.

Finally, the fourth category entails work focused on such *internal responses* as psychological stress, depression, physical health, and altered self-evaluations. Mental health measures include the number of stress symptoms people check (e.g., B. P. Buunk & Janssen, 1992; Hafer & Olson, 1993) and depression and anxiety measures (e.g., Goodman, et al., 2001; Lemeshow et al., 2008). Self-concept measures include self-esteem (e.g., Crocker, Luhtanen, Blaine, & Broadnax, 1994), optimism (e.g., Chen & Paterson, 2006), and social

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competence (e.g., Francis, 1992). Physical health measures include self-reported physical health (e.g., Goodman et al., 2001) as well as blood pressure and medical history (e.g., Pham-Kanter, 2009).

Comparisons between oneself and an ingroup member or oneself at a different point in time should predict individual behavior (including deviance and achievement). Previous theoretical RD frameworks (Crosby, 1976; Mark & Folger, 1984) also propose that disadvantaged interpersonal comparisons (IRD) should predict mental and physical health. Certainly, traditional measures of personal self-esteem should be more closely related to IRD in comparison to GRD. More general symptom checklists may be more ambiguous, but questions that refer to a person's unique physical and mental state also should be more closely related to disadvantaged interpersonal comparisons.

Comparisons between one's ingroup and an outgroup should predict collective behavior and attitudes toward the outgroup. It is less clear to what extent more general attitudes toward the system might be more closely related to GRD, although historically the assumption has been that GRD is more closely related with these types of assessments (Walker & Smith, 2002).

The Scope of Our Meta-Analysis

For our review, we focus on the relationship between subjective experiences of RD and a range of possible outcomes. This decision means that we do not address how objective circumstances are translated into the set of cognitive appraisals that are associated with deserving, anger, and resentment (see Figure 1). However, there is a small set of experiments designed to investigate the antecedent conditions of RD (Bernstein & Crosby, 1980; Cooper & Brehm, 1971; Folger & Martin, 1986; Martin, 1986a; Olson & Ross, 1984; Spector, 1956). In these studies, situational variables are manipulated to create feelings of deprivation, then subsequent evaluations of fairness and satisfaction are measured.

In another small set of experiments, researchers manipulate RD as a between-subjects variable and measure subsequent attitudes and behavior (e.g., Markovsky, 1988; Ross & McMillen, 1973; D. M. Taylor, Moghaddam, Gamble, & Zellerer, 1987). For example, Grant and Brown (1995) operationalized GRD in two steps. First, an experimenter told all participants that most participants received $10.00 for their participation, even though the actual amount that they would receive depended on team evaluations of team position papers. Second, the outgroup unfairly recommended $4.00 for each participant. As predicted, relatively deprived participants were less likely to accept the outgroup's evaluation and more likely to support writing a protest letter compared to participants who were not deprived. More recently, Halevy, Chou, Cohen, and Bornstein (2010) developed an intergroup prisoner's dilemma-maximizing difference game in which players could contribute their monies to a within-group pool designed to benefit ingroup members or a between-group pool designed to harm outgroup members. If put at a monetary disadvantage compared to an outgroup, participants reported more anger and resentment, and they contributed more to the between-group pool.

The results from these experimental studies give us confidence that process legitimacy (Grant & Brown, 1995), the likelihood that the situation will not improve (Folger, 1987), and lack of self-blame (Bernstein & Crosby, 1980) are key characteristics of RD. But unfortunately, if the experimental design did not include a subjective RD measure as we define it, we could not include the study in our data set.

Drawing on our conceptual definition, we make two predictions that derive directly from Figure 1. First, we propose *an affective hypothesis*: *RD measures that tap either justice-related affective judgments or both justice-related affective and cognitive judgments will be more strongly related to key outcome variables in comparison to purely cognitive measures that ask respondents to estimate differences between their present situation and a referent comparison.* For the meta-analysis, we define justice related affect very broadly including measures of deserving, unfairness, negative affect and mood, frustration, discontent, dissatisfaction, anger, and resentment.

Second, we propose *a fit hypothesis*: *The relationship between RD and various dependent measures will be stronger when the level of reference for both the RD and outcome measures is the same.* Because of the ambiguity of comparisons between the self and an outgroup member, we limit this test to clear interpersonal and intergroup comparisons. GRD should predict collective action and attitudes toward outgroups more strongly than IRD, whereas IRD should predict individual behavior and self-evaluations more strongly than GRD.

Third, we also propose that *the relationship between RD and various outcome measures will be stronger when higher quality measures are used for the RD and outcome measures*. If there is a solid relationship between RD and an outcome variable, then studies with well-measured RD and outcome variables should record stronger effect sizes then studies with less rigorously measured variables (see Pettigrew & Tropp, 2006). Although not strictly a test of RD theory, this methodological test is designed to test the possibility that poorly designed studies are providing the major support for the theory. Some meta-analyses in the past have found their largest effects supplied by the most rigorous research (e.g., Pettigrew & Tropp, 2006), whereas others have found their largest effects supplied by the least rigorous research (e.g., Cuijpers, van Straten, Bohlmeijer, Hollon, & Andersson, 2010). In the latter case, theoretical validity is called into serious question.

Method

Inclusion Criteria

Our theoretical discussion leads to six inclusion criteria for the RD measures and two more for the outcome measures.

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Below we make explicit what types of studies are being excluded by these criteria, as recommended by Rojahn and Pettigrew (1992).

Criterion 1. Because we focus on the relationship between RD and behavior, we consider only those empirical studies in which researchers treat subjective RD measures as an independent, predictive variable. Therefore, we exclude 148 studies (including 26 purely qualitative case studies) that treat RD as a dependent variable because they are beyond the scope of this meta-analysis.

Of the 149 studies, 3 use measures in which researchers ask participants to indicate the frequency with which they made a particular type of comparison but do not ask about the consequences of those comparisons (e.g., Brown, Ferris, Heller, & Keeping, 2007). Without clear evidence as to how people react to a specific comparison, we cannot be sure that they experienced RD (also see Stephan et al., 2002). Although comparison choice and frequency are important RD antecedents, this research is not pertinent to our focus on RD's effects.

Criterion 2. RD or a close proxy variable must be asked of individual respondents directly. Therefore, we had to remove 99 investigations in which researchers inferred RD from societal or national levels of income inequality or similar indices (e.g., Gurr, 1970). We also excluded an additional 202 studies in which RD is a state only inferred from such individual respondents' demographic variables as income, age, or gender (e.g., Geschwender & Geschwender, 1973; Pinard, Kirk, & Von Eschen, 1969; Reagan et al., 2007; Siahpush et al., 2006).

Criterion 3. For two reasons, we eliminated 94 studies in which investigators created difference scores from respondents' answers to different survey questions to represent participants' subjective experience (e.g., B. P. Buunk, Zurriaga, Gonzalez-Roma, & Subirats, 2003; M. C. Taylor, 2002; van Dierendonck, Schaufeli, & Buunk, 2001). First, difference scores are inherently difficult to analyze and interpret (Pedhazur & Schmelkin, 1991, pp. 291-295). Second, difference scores do not ensure that the respondent made the same comparison used by the investigator.

Criterion 4. Researchers must have operationalized RD as a comparative construct (excluding 25 studies that we reviewed). This requirement excludes studies in which researchers defined RD as respondents' feelings of injustice or resentment about their general personal or group situation, but without an explicit comparison to some standard (e.g., Dube & Guimond, 1986, Study 3; van Kyk & Nieuwoudt, 1990). The comparative requirement also excludes simple counts of the number of incidents in which a person experiences discrimination (used as a proxy for RD as in Dion, 1986). However, if participants report their experience of discrimination in contrast to other people or groups, we include the data.

Criterion 5. We include RD measures only if they were negative discrepancies that created feelings of deprivation. This eliminates six further studies. This criterion excludes measures that tap what Beaton and Deveau (2005) and others define as RD on the behalf of others or ideological deprivation. For example, Tougas and Veilleux (1990) measured men's perceptions of differences in salary, hiring, and promotions between men and women multiplied by their dissatisfaction with these discrepancies. Unlike the traditional conceptualization of GRD, this measure assumed that the greater the (illegitimate) discrepancies *favoring* their ingroup, the more dissatisfaction and resentment men felt. It is important to note that the authors also distinguished this measure from a measure of GRD that captured the extent to which men felt men were deprived in comparison to women. Interesting as this phenomenon is, altruistic or ideological RD is not within the scope of this meta-analysis.

Criterion 6. We include RD measures only if the relationship between the respondent and the comparison target is clear. For example, if a RD measure represents a comparison between two outgroups, we omit the measure. Seven studies were excluded based on this criterion. Thus, we eliminated African American respondents' comparisons between well-educated Blacks and blue- or white-collar workers (Abeles, 1976). This is necessary because it is unclear whether these African American respondents viewed well-educated Blacks as part of their ingroup or as an outgroup. If a RD measure is a comparison between a subgroup and the whole group, we exclude the measure for the same reason. So we could not use unemployed Australians' comparisons between their immediate peer group and all unemployed Australians (Walker & Mann, 1987). Unemployed respondents may feel equally identified with both their unemployed peers and all unemployed Australians or may see both groups as essentially the same. We have no way of knowing. Finally, we exclude the ingenious measures of comparative mistreatment used by Guyll, Matthews, and Bromberger (2001) to uncover differences in cardiovascular reactivity between African American and European American women. It was unclear in this study just who the respondents had in mind when they reported that other people (in comparison to themselves) had been treated with more respect, courtesy, and better service (e.g., Did these other people represent an ingroup or outgroup?).

We also set aside eight studies in which RD measures were the product of several different comparisons that could not be disentangled (Ashton, 1978; Dibble, 1981; M. C. Taylor, 1980; Vanneman & Pettigrew, 1972). For example, Ashton (1978) compares respondents who report feeling deprived compared to the outgroup and the ingroup ("doubly deprived" respondents) to respondents who report feeling deprived compared to the outgroup but gratified compared to the ingroup ("group deprived" respondents). Unfortunately, there is no way to disentangle these comparisons to test our fit hypothesis. However, if separate effect sizes were able to be calculated for different comparison targets, as when authors reported a correlation for a GRD measure and a different correlation for an IRD measure, these data are included.

In addition, we require each outcome measure to meet two inclusion criteria.

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Criterion 1. We use data only if the relationship between the outcome measure and the respondents' attitudes and behavior is clear. This inclusion rule removes four further studies. Sears and McConahay (1970) included a measure of the number of riot acts that respondents witnessed. Because it cannot be determined how the observation of collective behavior relates to feeling motivated to act from feelings of deprivation (one could just be in the "wrong" place at the "wrong" time), we exclude these data. We also exclude opinions about whether a nonmembership group should use violence to accomplish political goals (e.g., whether European Americans thought African Americans should use violence). Finally, we eliminated measures in which respondents reported what they thought other peers or family might think of them (Breivik & Olweus, 2006; Brunsting & Postmes, 2002). Although RD might be related to perceptions of one's neighborhood as less friendly, it is unclear how RD would directly cause or predict neighborhood climate.

Criterion 2. We include data only if the outcome measure is not part of the RD experience as we define it. This criterion removes 24 studies. As shown in Figure 1, we define RD as composing both comparison and justice-related affect. Therefore, we rejected studies in which researchers defined RD as the awareness of differences in a particular domain and the dependent measure as the resulting feelings about these differences.

Finally, we could not include 36 additional studies because we were unable to obtain the information that we needed to calculate an estimate of the effect size—a common problem in meta-analytic studies that is being solved by the recent insistence of journal editors that such data always be included.

Locating Relevant Studies

The selected studies were part of more than 860 studies located by using a variety of methods: (a) a computer search through psychological, sociological, economic, political, and dissertation abstracts through January 2010; (b) 112 personal letters and emails to researchers who have published relevant studies; (c) a review of reference lists from previously located studies and conference presentations; and (d) Listserv requests to members of the International Society for Justice Research, Society for Personality and Social Psychology, Society for the Study of Social Issues, International Society for Political Psychology, European Association of Social Psychologists, and Society of Australasian Social Psychologists. Keywords for the database search included *(employment) over-qualification*, *subjective social status*, *upward (contrasting) social comparisons*, *injustice gap*, *referential comparisons*, *perceived socio-economic differences*, *relative deprivation*, *relative disadvantage*, and *relative discrimination*. We also examined the reference sections of all located primary studies and relevant literature reviews. We reviewed any study that we could locate that indicated either by title or abstract that RD might have been measured. If we reviewed the study, we included it in the 860 count above even if RD was referenced only in the introduction as a theoretical framework or in the discussion as a post hoc explanation for an unexpected pattern of results.

The search yielded 210 studies (summarizing 293 independent samples, 421 tests and data from 186,073 participants) written between 1961 and January 2010 that met our inclusion criteria (median year of publication = 2000). Note the sharp decline from the 860 studies initially obtained to 210 final studies. This large 76% drop-off represents the loss from our necessary criteria for inclusion. This massive clearing of the underbrush demonstrates that most of the research literature labeled as testing RD does not actually test RD theory directly. Indeed, this has been a major problem for previous qualitative reviews that did not use strict inclusion criteria.

Most of the included studies were published journal articles (168), but the data set also includes 8 book chapters and 34 unpublished studies (including 19 dissertations). Samples from the United States represent 79 (39%) of the studies, but 29 other countries also are represented. Similarly, social psychology represents the most frequent author and journal affiliation (45%). However, the data set also includes work published in other subdisciplines of psychology (e.g., personality, political, education, industrial/organizational, clinical/counseling, economic, and health psychology) and disciplines outside of psychology (e.g., criminology, textile arts, economics, public health, political science, sociology, and behavioral medicine). Although most of the studies were written in English, the final data set also includes studies written in French, German, and Afrikaans. Samples ranged from probability population surveys to single occupations (e.g., university faculty, female police officers, funeral directors, and concrete construction workers) and ethnic, religious, national and political minority and majority groups.

It is important to show that these inclusion criteria do not inflate our estimates of average effect sizes. For example, the average mean effect size for the 20 independent samples that we excluded because the outcome domain was too similar to the RD measure domain was +.32 (ranging from +.23 to +.40). For the 32 excluded tests of the relationship between RD, manipulated as a between-subjects variable, and subsequent attitudes and behaviors, the average effect size was +.21 (ranging from +.17 to +.25). For 23 excluded tests of the relationship between aggregate societal level characteristics and outcome measures, the average effect size was +.51 (ranging from .40 to .61). That these subsets of studies yield such significant effects is certainly of interest, but they are not directly relevant to RD theory as we define it.

As we describe below, the average effect size for the 210 separate studies included in the final data set was +.11. This result strongly indicates that our exclusion rules constitute a conservative test of the relationship between RD and various dependent measures. For the 33 excluded studies that used difference scores to measure RD, the average effect size

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was +.09 (ranging from +.03 to +.16). Although this average effect size is close to the final data set effect size, this small effect also is consistent with our contention that researcher-created difference scores do not capture RD adequately.

Variables Coded From Each Study

If available, we recorded the following general information from each study: (a) date of publication, (b) publication form, (c) author and publication discipline, (d) sample nationality, (e) number of respondents, (f) respondents' age, (g) respondents' gender, (h) type of sampling strategy, and (i) response rate. For each study, if the sample could be considered homogeneous, we also coded the relevant group membership and occupation. Two coders independently read and coded each sample, RD, and outcome measure. If at least one item in a scale met our inclusion criteria, we included it as part of the database. All coding was done independently of effect size calculations, and any disagreements were solved through discussion between the two coders. The coding reliabilities were consistently high—with all kappas greater than .90.

RD measures. For each different RD measure, we coded whether participants estimated a difference (a cognitive measure), reported how they felt about the difference (an affective measure), or indicated whether their relative disadvantage was undeserved or unfair (treated as a second form of an affective measure). If measures of mood or emotions were woven into the RD measure, we coded it as an affective measure.

We also coded whether the comparison was between (a) the respondent's personal situation and the situation for an ingroup member, (b) the respondent's personal situation and the situation for an outgroup member, (c) the respondent's ingroup's situation and an outgroup's situation, or (d) the respondent's or his/her ingroup's present situation with his/her past, future expectations, or theoretical possibilities (e.g., the best possible life). If a measure included comparisons with both an ingroup and an outgroup referent (e.g., questions including a female and male employee referent for female employees, as in Hafer and Olson, 1993), the measure was coded as representing an outgroup comparison.

We also coded whether the comparison dimension represented differences in (a) social position, or class, (b) housing, (c) income, standard of living or purchase ability, (d) wages or taxes, (e) academic or task performance, (f) work situation, (g) geographic location, (h) health or overall well-being, (i) general life situation, (j) political power or institutional treatment, (k) physical appearance or personality characteristics, or (l) one's relationships (e.g., from a single-parent family in comparison to a two-parent family, as in Breivik & Olweus, 2006).

Finally, we coded the quality of each RD measure. The first category (scored as one) included single-item measures. The second category (scored as two) consisted of multiple-item scales with unknown reliability or an alpha of less than .70. The third category (scored as three) consisted of multiple-item scales with an alpha of .70 or more. We also distinguished between measures that had been used in a previous research project (e.g., the subjective status measure from Goodman et al., 2001) or created for the current project.

Outcome measures. We classified each dependent measure represented in the larger data set into one of the four general categories shown in Figure 1. The first category, labeled as *internal states*, includes, (a) stress, anxiety, depression, hopelessness, mental illness, and pessimism, (b) (personal) self-esteem and self-efficacy, and (c) (poor) physical health (e.g., more obesity, heart disease, interrupted sleep). The second category, labeled as *individual behavior*, includes (a) forms of deviance (e.g., violence, stealing, and counterproductive work behavior), (b) forms of escape (e.g., smoking, drinking, drug use, absenteeism and social isolation), and (c) forms of achievement (e.g., moonlighting, academic performance). The third category, labeled as *intergroup attitudes*, includes (a) attitudes toward the ingroup (e.g., ingroup favoritism, nationalism, and identification), (b) attitudes toward the system (e.g., voting intentions, support for authorities), and (c) attitudes toward outgroups (including prejudice, majority group members' attitudes toward immigration and affirmative action). Finally, the fourth category, labeled as *collective behavior*, includes both unstructured and structured forms of collective action as well as approval of political violence.

We coded each outcome measure as representing an attitude, a behavioral intention, or an actual behavior. We also coded the quality of the outcome measure with the same three measurement quality categories listed above. There was an additional category for outcome measures defined as difference scores created by the researchers. We also distinguished between outcome measures that had been used in previous research or were created for the current project.

Computation and Analysis of Effect Sizes

All analyses were conducted using version two of the Comprehensive Meta-Analysis program (www.Meta-Analysis.com). We report Pearson's r as the principal indicator of effect size throughout the analysis (Rosenthal, 1995). All mean rs were computed with each effect size weighted by the reciprocal of its variance (which gives more weight to effect sizes that are more reliably estimated; see Borenstein, Hedges, Higgins, & Rothstein, 2009). Although the use of random effects analysis addresses the issue of sampling error, we cannot correct for measurement error because of the failure of most RD studies to supply the needed data. As Schmidt (2010) emphasizes, this means that our final effect sizes are likely to be smaller than if we were able to eliminate measurement error.

A positive mean effect size indicates that greater RD relates to more of the particular behavior or stronger attitudes. If no correlations were reported (as was the case for 11.5% of the included effects), the effect size was derived from the results

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Table 1. Summary of Effect Sizes for RD Measures and Outcomes

Sample	*r*	95% CI	*z*	*k*	*N*	Q_W	Tau
Independent studies	.106	.084–.127	9.69	210	143,188	3,018.60	.142
Independent samples	.144	.128–.161	17.00	293	186,073	3,264.47	.128
Separate RD tests	.134	.121–.148	19.35	421	243,733	4,090.63	.124
Internal states	.173	.152–.193	16.19	188	135,198	2,393.82	.129
Individual behavior	.118	.097–.140	10.68	126	81,474	988.20	.106
Intergroup attitudes	.115	.097–.134	12.00	299	152,366	3,619.76	.149
Collective behavior	.148	.115–.181	8.66	99	49,242	1,268.18	.157

RD = relative deprivation. The mean effects and confidence limits listed in this table have been transformed back to the *r* metric from the *z*-transformed estimates obtained in the original analyses. Table entries represent random effects model estimates.

of significance tests (χ^2, *t* or *F* ratios) by use of the conversion formulas provided by B. T. Johnson (1993). If a particular relationship was reported as nonsignificant or the result was completely omitted (but implied by the methods section as was the case for two effect sizes), we assigned a value of .00 for the effect size. This procedure, as Rosenthal (1995) points out, is a conservative one that reduces the mean effect size.

For three reasons, our primary unit of analysis is the individual *test* of the relationship between a single RD measure and an outcome measure. First, many studies include separate questions for different comparison targets. Given our expectation that different types of comparisons will be more or less closely related to different outcomes, these tests had to be treated separately. Similarly, researchers often include separate cognitive and affective RD measures within the same study. Finally, many studies include several different outcome measures. The mean number of separate effect sizes included from a single study was 8.51 (*Mdn* = 4, ranging from 1 to 89 separate reported effect sizes per study). However, studies that included larger numbers of tests also reported smaller average effect sizes, $r(208) = -.16$, $p < .05$, indicating that, if anything, a focus on individual tests is a conservative method for estimating the strength of RD effects. If authors reported multiple effect sizes, we averaged effect sizes by category (for example, if authors report correlations with each item of a multiple item measure of affirmative action, we would combine the effect sizes into a single estimate for the data set). All combined effect sizes were calculated using Rosenthal's (1995) suggested formulas.

For each category of effect sizes, we calculate the weighted mean effect size and the corresponding 95% confidence interval. Second, we examine the homogeneity of each set of effect sizes by calculating the homogeneity statistic *Q* that has an approximate chi-square distribution with $k - 1$ degrees of freedom, where *k* is the number of effect sizes (Borenstein et al., 2009). In the absence of homogeneity, we test a series of categorical models that relate the effect sizes to characteristics of the study (Borenstein et al., 2009). We use a random effects model for all our analyses because we assume that the variance around the mean effect size cannot be fully explained by potential moderators because of the heterogeneity of our sample (Borenstein et al., 2009). This approach is a more conservative test of our hypotheses in comparison to a fixed effects model analysis. The reliability for mean effect sizes was established though calculations of confidence intervals as well as Stouffer's *z*.

Results

Table 1 lists the mean effect size for each of the three units of analysis (studies, samples, and individual tests) and the four types of outcome variables. Each of the mean effects is highly reliable, but relatively small—ranging from $r = +.11$ to $+.17$. As shown in Table 1, the studies provide smaller effects on average in comparison to independent samples and individual tests. Similarly, tests for internal states yield significantly larger mean effects compared to either those for individual behavior ($Q_B = 11.40$, $p < .001$) or intergroup attitudes ($Q_B = 14.08$, $p < .01$). But they are not reliably larger in comparison to collective behavior ($Q_B = 1.41$, $p = .24$). Tests of collective behavior yield slightly larger mean effects in comparison to intergroup attitudes ($Q_B = 2.87$, $p = .09$) but not compared to individual behavior ($Q_B = 2.25$, $p = .13$). Finally, there is no reliable difference between individual behavior and intergroup attitude tests ($Q_B = 0.03$, $p = .84$). Note, however, the considerable heterogeneity within each category (Q_W ranged from 988.20 to 4,090.63) before the application of the corrections advanced by our hypotheses.

Publication Bias

Before we test our hypotheses, we must check for potential publication biases. First, we applied Rosenthal's (1995) fail-safe index. For 293 samples, it would require more than 8,111 missing samples that reported an effect size of zero to erase the statistical significance of the mean RD effect size at the 5% confidence level. This number is considerably larger than the 860 studies uncovered by our intensive literature search. Second, we determined that the relationship between sample sizes and effect sizes is not statistically significant, $r(292) = -.058$, $p = .47$. In other words, there is no evidence that smaller sample sizes are contributing larger effect sizes—

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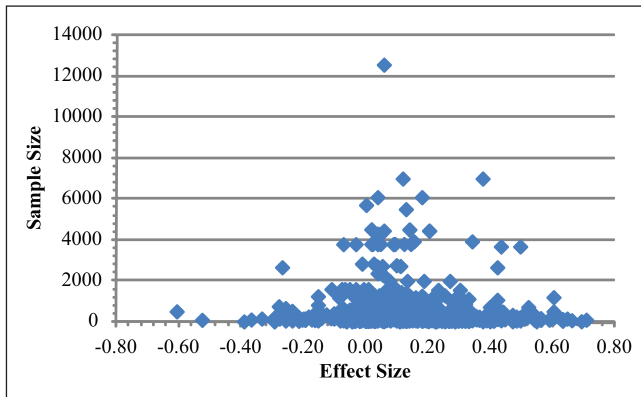

Figure 2. Funnel plot of sample size by Fisher's z

one potential determination of whether a study is published. Third, Figure 2 provides a scatter diagram comparing sample sizes with the effect sizes (represented by Fischer's z). The graph roughly resembles a funnel. The funnel is not sharply skewed, and the mean effect remains approximately the same regardless of the sample sizes.

Finally, as a direct test for publication bias, we compare the mean effect sizes from different publication sources. The nine book chapters yield the strongest average effect size (r = +.15, CI = –.02 to +.27, n = 4,589), the 167 published journal articles yield the next strongest average effect size (r = +.11, CI = +.09 to +.14, n = 123,772), the 19 dissertations yield a smaller average effect size (r = +.06, CI = –.01 to +.13, n = 4,305), and the 15 unpublished papers yield the smallest average effect size (r = +.05, CI = –.06 to +.12, n = 10,252). An unfocused test of a between-classes effect for the four types is not statistically significant, $Q_B(3) = 3.55$, p = .32. The difference between the two published sources (books and articles) and the two unpublished sources (theses and unpublished papers) is not conventionally significant, $Q_B(1)$ = 2.90, p = .09. Within each publication category, there is substantial heterogeneity among effect sizes (Q_W ranged from 79.85 to 2,177.60). All these tests suggest no important publication bias exists in our overall data file.

Testing the Justice-Related Affect Hypothesis

Table 2 combines all our data across the four categories of outcome variables to test the justice-related affect hypothesis. For all three units of analysis, our first hypothesis is supported. As shown in Table 2, the mean effects for studies, samples, and tests that employ justice-related affective RD measures are larger than those employing purely cognitive RD measures. This special strength of justice-related affective RD measures was largely found among measures of individual behavior, r(cognitive RD) = +.07 vs. r(affective RD) = +.16, $Q_B(1)$ = 13.61, $p < .05$, and intergroup attitudes, r(cognitive RD) = +.10 vs. r(affective RD) = +.15, $Q_B(1)$ = 4.24, $p < .05$. Justice-related affective measures (r = +.20) for internal states were stronger in comparison to cognitive measures (r = +.16), but the difference was not conventionally statistically reliable, $Q_B(1)$ = 3.21, p = .07. There was not a statistically significant difference between justice-related affective and cognitive RD measures for collective behavior, r(cognitive RD) = +.15 vs. r(affective RD) = +.14, $Q_B(1)$ = 0.05, p = .82. Within all outcome categories of justice-related affective RD measures, there was significant heterogeneity among effect sizes (Q_W ranged from 364.46 to 1,818.65).

Testing the Fit Hypothesis

The second hypothesis holds that RD measures will predict outcome measures more strongly if the level of analysis represented by the RD measure matches the outcome measure level. Before testing this possibility, we excluded RD measures in which the respondent compared themselves to an outgroup member. As discussed earlier, it is unclear whether the respondents view this type of comparison as interpersonal (between themselves and another unique person) or as intergroup (between themselves as a representative of their ingroup and the outgroup). We also excluded outcome measures that focused on attitudes toward one's ingroup because we could make no clear theoretical predictions. Depending on respondents' attributions about the cause or stability of RD, increased IRD or GRD could lead to more or less identification or commitment to one's ingroup. Alternatively, ingroup identification may best be viewed as a moderator of the relationship between RD and other outcomes (e.g., more identified respondents will react more strongly to group RD; see van Zomeren et al., 2008). If the comparison and outcome represent the same level of analysis (e.g., GRD comparisons associated with collective behavior), we coded it as a "match." If the comparison and outcome clearly represent different levels of analysis, we coded it as a "mismatch" (e.g., GRD associated with individual academic achievement).

As shown in Table 3, matched tests outperformed mismatched tests whether we consider all relevant RD comparisons or limit the analysis to justice-related affective RD measures. If we compare matched tests in which we screen for quality (by excluding single-item measures, outcomes measured with difference scores, and convenience samples), matched tests yield an effect size of +.23, CI = +.17 to +.29, k = 43, n = 27,064, in comparison to mismatched tests, r = +.11, CI = +.01 to +.21, k = 27, n = 16,453; $Q_B(1)$ = 4.22, $p < .05$. Finally, if we limit our analysis to the 40 tests of individual and collective behavior (as assumed by the original Runciman [1966] hypothesis), matched tests yield stronger effect sizes (r = +.25, CI = +.18 to .32, k = 26, n = 7,623) when compared with mismatched tests (r = +.10, CI = –.06 to +.25, k = 14, n = 10,333) although the difference only approaches statistical significance, $Q_B(1)$ = 3.14, p = .08.

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Table 2. Comparison of Cognitive and Justice-Related Affective RD Measures

Sample	r	95% CI	z	k	Q_W	Tau
Independent studies						
Cognitive RD	.077	.053–.100	6.34	124	1,494.95	.117
Affective RD	.174	.128–.220	7.33	60	679.62	.169
Between-classes effect			$Q_B(1) = 13.61, p < .05$			
Independent samples						
Cognitive RD	.128	.110–.145	7.94	173	1,465.87	.100
Affective RD	.186	.141–.231	14.17	87	1,469.21	.206
Between–classes effect			$Q_B(1) = 5.58, p < .05$			
Separate RD tests						
Cognitive RD	.116	.102–.130	16.28	264	1,962.59	.096
Affective RD	.165	.134–.195	10.38	157	1,931.78	.184
Between–classes effect			$Q_B(1) = 8.08, p < .05$			

RD = relative deprivation. The mean effects and confidence limits listed in this table have been transformed back to the *r* metric from the z-transformed estimates obtained in the original analyses. Table entries represent random effects model estimates.

Table 3. Comparison of "Matched" and "Mismatched" RD and Outcome Measures

Sample	r	95% CI	z	k	Q_W	Tau
All RD test comparisons						
Matched levels of analysis	.166	.151–.182	20.80	333	4,175.79	.130
Mismatched levels of analysis	.113	.089–.137	9.18	167	2,001.90	.142
Between-classes effect			$Q_B(1) = 13.59, p < .05$			
Justice-related affective RD tests						
Matched levels of analysis	.201	.167–.235	11.17	119	1,683.47	.183
Mismatched levels of analysis	.123	.076–.169	5.10	77	1,288.43	.200
Between-classes effect			$Q_B(1) = 7.06, p < .05$			
Justice-related affective RD tests with quality controls						
Matched levels of analysis	.230	.174–.285	7.89	43	1,098.35	.186
Mismatched levels of analysis	.111	.011–.210	2.164	27	1,080.65	.264
			$Q_B(1) = 4.22, p < .05$			
Multi-item justice-related affective measures as predictors of individual/collective behavior						
Matched levels of analysis	.253	.179–.324	6.53	26	268.43	.186
Mismatched levels of analysis	.099	–.058–.252	0.22	14	755.80	.297
Between-classes effect			$Q_B(1) = 3.14, p = .08$			

RD = relative deprivation. The mean effects and confidence limits listed in this table have been transformed back to the *r* metric from the z-transformed estimates obtained in the original analyses. Comparisons to outgroup members and attitudes toward the ingroup are not included. Quality controls exclude outcomes measured as difference scores, single-item RD measures, and convenience samples. Table entries represent random effects model estimates.

Comparisons to Outgroup Members

The choice to exclude 86 comparisons to outgroup members from the tests described above may mask the potential importance of these comparisons. For example, the six justice-related affective comparisons between the respondent and an outgroup member yielded strong effect sizes for personal self-esteem and efficacy (r = +.31, CI = +.24 to +.39), whereas the 18 justice-related affective comparisons between the respondent and an ingroup member yielded a much smaller effect size for personal self-evaluations (r = +.13, CI = +.04 to +.29), $Q_B(1) = 8.35$, $p < .05$. The six tests of outgroup member comparisons for mental and physical health yielded an effect size of +.28 (CI = +.19 to +.36) that was similar to the 13 tests of comparisons to ingroup members (r = +.29, CI = +.06 to +.48), $Q_B(1) = 0.01$, $p = .94$. Finally, the eight justice-related affective tests for comparisons to outgroup members predicted the desire to escape or exit the situation more strongly (r = +.23, CI = +.14 to +.32) in comparison to the effect sizes yielded by 13 tests for comparisons to ingroup members (r = +.11, CI = +.04 to +.17), $Q_B(1) = 4.44$, $p < .05$.

In contrast, the 10 justice-related affective tests of these comparisons to outgroup members yielded somewhat smaller effect sizes for collective behavior (r = +.16, CI = +.12 to

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+.21) in comparison to 19 justice-related affective intergroup comparisons (r = +.20, CI = +.14 to +.26), but the difference is not statistically significant, $Q_B(1) = 0.88$, $p = .35$. Finally, all three types of justice-related affective social comparisons—to an ingroup member (r = +.23, CI = +.15 to +.31, k = 7), to an outgroup member (r = +.23, CI = +.09 to +.37, k = 3), or to an intergroup comparison (r = +.23, CI = +.08 to +.39, k = 9)—were solidly associated with negative attitudes toward the larger system. These mixed effects for outgroup member comparisons (strong relationships to internal states and the desire to exit the situation but weaker relationships for collective behavior) support our suggestion that the interpretation of these comparisons is ambiguous and requires further information about how respondents perceive the outgroup member.

Testing the Research Quality Hypothesis

We compared 358 single-item RD measures to 233 multiple-items with unknown or poor reliability (Cronbach's alphas are less than .70) and 122 multiple-item RD measures with clear reliability (Cronbach's alphas are .70 or larger). Single-item measures predict outcomes (r = +.12, CI = +.10 to +.15) as strongly as multiple items with poor reliability (r = +.12, CI = +.09 to +.21), but they are significantly less strong in comparison to multiple-item measures with solid reliability (r = +.18, CI = +.15 to +.20), $Q_B(2) = 12.82$, $p < .05$. A direct comparison between single item and all multiple item measures showed a reliable difference, $Q_B(1) = 9.53$, $p < .05$. There remained, however, considerable heterogeneity within each category (Q_W ranged from 1,220 to 4,030).

Next, we compared 159 single-item *outcome* measures to 322 multiple-item outcome measures with solid reliability (Cronbach's alphas .70 or larger), 187 multiple outcome measures with unknown or poor reliability (Cronbach's alphas less than .70), and 23 difference score outcomes (e.g., researchers subtract trait ratings of the outgroup from trait ratings of the ingroup). Difference scores yielded the weakest effects (r = +.02, CI = –.02 to +.06), single items yielded somewhat the strongest effects (r = +.12, CI = +.10 to +.14), and multiple-item scales yielded the strongest effects (for solid reliability measures—r = +.14, CI = +.13 to +.16; and for those with unclear reliability measures—r = +.15, CI = +.13 to +.17), $Q_B(3) = 33.95$, $p < .05$. A direct comparison between all multiple item and single-item measures showed a marginally reliable difference, $Q_B(1) = 3.45$, $p = .06$.

Multicollinearity could be a problem here. Justice-related affect is closely related to RD measure quality, $r(712)$ = +.33, $p < .05$, outcome measure quality, $r(712)$ = +.11, $p < .05$, and matched levels of analysis, $r(495)$ = +.12, $p < .05$. Thus, the justice-related affect effect could reflect its relationships with RD quality and matching levels of analysis. To test this possibility, we treated the effect size as the outcome in a modified weighted least squares regression analysis (Lipsey & Wilson, 2001). Outcome quality ($\beta = .04$, $z = 4.50$, $p < .001$), RD quality ($\beta = .10$, $z = 11.49$, $p < .001$), justice-related affect ($\beta = .11$, $z = 7.54$, $p < .001$), and whether the levels of analysis were matched or mismatched ($\beta = .16$, $z = 10.73$, $p < .001$) all reliably predicted effect size. The regression model was statistically significant, $Q(4) = 31.59$, $p < .001$, with a random effects variance component $v = .02$ and an explained variance of 5.4%. As one would expect, more theoretically accurate RD measures represent better quality and are matched with better quality outcomes. But the important point is that theoretical accuracy (defined as the inclusion of both justice-related affect and matched levels of analysis) continues to predict larger effect sizes.

Figure 3 summarizes our hypotheses-testing results. Using individual tests, the histogram shows that the combined predictive effect of RD for all tests is +.134. The mean effect size for those tests that have none of our three hypothesized improvements—that is, they are the worst subset of tests because they used only low-quality and cognitive RD measures and related them to outcome variables at a different level of analysis—is only +.079. This effect rises to +.165 for those tests that employ some type of justice-related affect RD measure. The effect also increases to +.166 if we limit the analysis to RD measures that match the outcome level of analysis. The effect size rises once more to +.201 if both justice-related affect and matched levels of analysis are required. Finally, the mean effect reaches +.230 for the optimal subset of justice-related affective tests that boasted both a level fit and higher quality measures. In terms of variance explained, Figure 2 reveals that tests that employed all three of our methodological improvements were almost 3 times stronger than the mean effect for all the RD tests in our file and more than 8 times stronger than the worst subset of tests that featured none of our improvements.

Comparing Objective With Subjective Deprivation

Finally, critics often raise a fundamental question about RD. Does the perceived deprivation tapped by RD measures largely reflect absolute deprivation? After all, we know that both economic IRD and GRD are found primarily among working-class respondents who are politically alienated (Pettigrew et al., 2008). Fortunately, our data set includes 26 studies that allow us to contrast objective with subjective deprivation effects in the same analysis. Outcomes include individual achievement and deviance, mental and physical health, personal self-esteem, and attitudes toward the larger system. Although the RD measures vary enormously, the objective RD measure for all 26 studies is income. The average effect size for subjective RD measures is +.18 (CI = +.14 to +.21, k = 26, n = 20,171). The effect size for objective RD measures is +.12 (CI = +.09 to +.15, k = 26, n = 20,171). As

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Table 4. Comparison of Objective and Subjective RD Measures

Sample	*r*	95% CI	*z*	*k*	Q_W	Tau
Objective RD	.119	.087–.151	7.34	26	103.22	.066
Subjective RD	.177	.141–.214	9.23	26	146.58	.082
Between-classes effect		$Q_B(1) = 5.36, p < .05$				

RD = relative deprivation. The mean effects and confidence limits listed in this table have been transformed back to the *r* metric from the z-transformed estimates obtained in the original analyses. Each study contributed a single subjective and objective RD test. Table entries represent random effects model estimates.

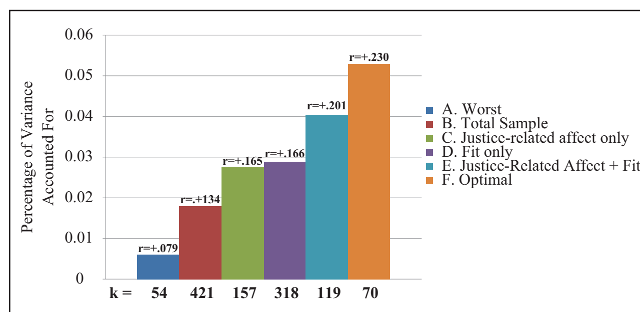

Figure 3. Mean effects of various test subsets by percentage of variance accounted for

Table 4 indicates, subjective effects not only are statistically significant but are also reliably larger than the objective deprivation effects, $Q_B(1) = 5.36$, $p < .05$.

One also could speculate that the relationships between RD and outcomes might be stronger for members of structurally disadvantaged groups who are more objectively deprived when compared to structurally advantaged groups. To test this possibility, we first limited our analysis to samples in which participants all shared the same social category (ostensibly making the relevant social category salient). The average effect size for best practices RD tests (defined as multi-item affective measures matched to the outcome level of analysis) for samples of ethnic, religious, or national minorities, citizens of occupied countries, the unemployed, women, and homosexuals is +.26 (CI = +.20 to +.31, $k = 26$, $n = 3{,}860$). The effect size for best practice tests for samples of ethnic majorities, men, and groups defined by their political affiliation is +.18 (CI = +.07 to +.29, $k = 16$, $n = 6{,}153$). However, this difference is not statistically significant, $Q_B(1) = 1.41$, $p = .23$.

Similarly, one might speculate that relationships between RD and outcomes would be stronger for economically based comparison dimensions than other comparison dimensions (e.g., how authorities treat members of different groups). The relative difference in wages could be objectively verified in ways that relative differences in supervisor treatment cannot. The average effect size for best practice RD tests for wages, income, and housing is +.17 (CI = +.17 to +.12, $k = 42$, $n = 24{,}830$). The effect size for best practice tests for other comparison dimensions is +.21 (CI = +.16 to +.26, $k = 94$, $n = 17{,}284$). The difference between the two groups is not statistically reliable, $Q_B(1) = 0.73$, $p = .39$.[4]

Discussion

Although the initial analysis suggests that the mean RD effect size might be as weak as some critics claim, if the RD measure includes justice-related affect and matches the level of analysis implied by the outcome variable, the predictive power improves sharply. The initial justice-related effect size of +.14 (for independent samples) improves to +.23 for high-quality affective RD measures matched to the outcome level of analysis. These patterns emerge despite our strict inclusion rules and our file's enormous variety of ages, nationalities, and group memberships. More importantly, appropriately measured RD predicts a wide variety of important outcomes—including behavior, behavioral intentions, and attitudes as well as measures of mental and physical health.

This mean effect size of +.23 is comparable to those of other meta-analyses of important social psychological processes. Pettigrew and Tropp (2006) obtained an effect size of +.21 between intergroup contact and reduced prejudice. Effect sizes are similar for tests of subjective well-being in relation to both internal locus of control (+.25) and low neuroticism test scores (+.27; DeNeve & Copper, 1998). Likewise, in an analysis of more than 120 meta-analyses dealing with psychological assessment, the median effect size was +.27 (Meyer et al., 2001). Finally, the effect size of +.23 represents a Cohen's *d* of +.47, which is recognized as a solid medium-sized effect in psychological research (Cohen, 1988).

Improving RD Predictability

But, clearly, there is still room for improvement. First, very few affective RD measures in our data set include the discrete emotions of anger and resentment that RD theorists propose to be associated with RD (see Smith et al., 2008). The nine projects in which investigators actually asked respondents whether they felt angry or resentful about the disadvantaged comparison yield a strong average effect size of +.34 (CI = +.26 to +.43, $n = 2{,}036$). Angry resentment is distinct from envy, jealousy, or even more generic forms of anger (see Leach, Snider, & Iyer, 2002). First, resentment should elicit a focus on the system that produces the inequity whereas envy should elicit a focus on the comparison targets and what they have. Second, resentment is often a publicly shared and supported emotion that evokes notions of justice, whereas envy is often a private and perhaps even shameful emotion (see Runciman, 1966). Finally, resentment represents a less ephemeral and more clearly moral emotion in contrast to simple anger (see Grant, 2008). If future researchers measure

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angry resentment directly, RD predictability should markedly improve.

Second, the fit hypothesis must be specified further. For example, we determined fit by whether a particular behavior serves the group or the individual. But for these analyses, we had to assume crudely that individual acts of deviance served the individual and collective protest served the group. However, the decision to "tag" a local business with graffiti could represent a desire to express one's individuality or a group norm (suggesting the proprietors are not welcome in the neighborhood). Similarly, the choice to attend a political protest could reflect one's commitment to the collective goal or a desire to join a friend at the rally. If future researchers measure respondents' *intention* to serve the group or their individual self, the fit between RD level and outcome could well be improved.

Third, researchers should clarify *why* they expect RD to be related to a particular outcome. It makes sense that the angry resentment generated by an undeserved ingroup disadvantage would be directed toward outgroups. Anger is an "attack" emotion associated with behaviors directed toward others (Mackie, Devos, & Smith, 2000). However, it is less clear how RD might be related to attitudes toward one's ingroup. On one hand, we could imagine that people who feel that they and those like themselves do not have what they deserve will identify more strongly with their group in contrast to other groups (perhaps leading to politicized identification; see Simon & Klandermans, 2001). On the other hand, people could feel that they personally do not have what they deserve *because* of their group membership. This attribution would lead them to identify less strongly with their group (in preparation, perhaps, to leave the group for another group). Of course, this analysis treats attitudes toward the ingroup as a consequence of RD. Other researchers (Mummendey et al., 1999; van Zomeren et al., 2008) treat ingroup identification as a cause or moderator of the relationship between (group) RD and collective action.

Similarly, it makes sense that anger and resentment would be related to individual "deviant" behavior but less clear why it would motivate individual achievement (particularly if achievement is identified within the system as the most legitimate avenue of behavior). If a person feels angry and resentful about a disadvantage, it seems unlikely that she or he will be motivated to work hard within the same system that produced her or his undeserved disadvantage. As Fine and Rosenberg (1983) argue based on qualitative interviews with young adults who chose to leave high school, leaving school emerged as an active response to a system that treated them unfairly, whereas staying appeared to be a more passive response to the same injustice.

The pathways between RD and physical and mental health outcomes (and associated attempts to "self-medicate" one's discomfort with drugs) also are unclear. Should the anger and resentment associated with RD lead to depression, anxiety, heart disease, obesity, or some combination? In part, this ambiguity reflects general questions about the relationship between specific emotional states and their psychological and physical consequences (Schnittker & McLeod, 2005). One difficulty for this area of research is the tendency to confuse anxiety about one's status and RD (Schnittker & McLeod, 2005). However, status anxiety—defined as concerns about where one might be situated in a particular status hierarchy—should be distinguished from RD in which one's (undeserved) *dis*advantaged position is a clear product of external circumstances. Indeed, if IRD is related to physical and mental health, it should be associated with diseases linked to anger, not fear and sadness. More importantly, it may well not be a single RD experience that leads to poor physical and mental health, but cumulative RD experiences over many years that lead to poor outcomes (see Adler et al., 2008). In fact, health researchers describe a "weathering effect" in which the effects of social inequality on health increase as people get older (Geronimus, Hicken, Keene, & Bound, 2006). It also might not be the experience of RD per se but whether people's attempts to address their disadvantages are successful that shape physical and mental health.

An important set of questions for future research will be to explore the relationships among the four categories of outcome variables that we identified. That is, if people actively challenge RD, does it protect or aggravate their physical health? Does the effectiveness of active RD responses on internal states depend on their success? Or is the key issue whether one's interpretation of the situation is socially shared or not?

Limitations

Before considering the broader implications of this review, we should recognize three limitations to our conclusions. First, even after controlling for measurement quality, matched level of analysis, and justice-related affect, the effect sizes remain remarkably heterogeneous. This result, of course, is typical of most meta-analyses. Given the enormous variety of research designs, participant populations, measures, and dependent variables, it would have been surprising if the results had approached homogeneity. This heterogeneity supports our decision to use the more conservative estimates provided by the random effects model. And it suggests that the true effect of correctly measured RD could well be stronger than we are able to document.

A second limitation is our inability to infer causal relationships. Although our data set contains 12 longitudinal studies and 3 experiments, most of these data come from cross-sectional surveys and questionnaires. It is reassuring that there is no reliable difference between the average effect size for the 32 longitudinal tests and the 691 cross-sectional tests (Q_B = +0.06, p = .81), but this result does not resolve the problem. Nonetheless, we adopt the common theoretical assumption that feeling deprived leads to various reactions even though nonrecursive causation is certainly possible. That is, one

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could argue that the various outcomes that we reviewed could lead to feeling deprived. For example, people's awareness of their (collectively) undeserved disadvantage could follow as well as precede their participation in collective protest (see Drury & Reicher, 2000). Or, perhaps, people who already feel depressed or anxious are more likely to notice and react to perceived undeserved disadvantages.

Finally, individual acts of deviance or escape could lead to IRD. For example, Webber (2010) argues that small indiscretions committed by young people lead others to identify them as criminals, and this identity, in turn, prevents these young people from achieving their aspirations. In other words, it is not (economic) IRD that leads to crime but crimes that lead to IRD. Yet Yang and his colleagues (2008) argue for the opposite relationship. They assert that greater exposure to material culture (measured as watching Western television shows) should increase IRD (and, perhaps, lead to crimes of acquisition). To create the most inclusive data set possible, we treated watching television as a form of escape from the RD experience in the same way that increased drug use could be considered as a form of escape. We also treated crime, delinquency, and counterproductive work behavior as reactions to RD.

Still, we think there is a rich opportunity for experimental tests of these proposed causal relationships. For example, Callan et al. (2008) experimentally manipulated IRD by asking undergraduate students to calculate their monthly discretionary income in the context of comparison information from other undergraduates to investigate the effect of IRD on undergraduates' intentions to gamble. And Smith and colleagues (1994) presented group members with distributions of participant payments that paid many more outgroup members in comparison to ingroup members as a manipulation of GRD. Experimental manipulations like these could enable researchers to test many of the basic casual relationships presumed by RD models (also see Folger, 1987; Grant & Brown, 1995; Guimond & Dambrun, 2002; Halevy et al., 2010; Martin, 1986a, 1986b).

Finally, our analysis suggests clear and continuing gaps within the RD literature. First, theoretically accurate multi-item measures are relatively rare, particularly for comparisons to oneself or one's group across time. Indeed, there is yet to be a proper individual level test of Gurr's (1970) original distinction among detrimental, aspirational, and progressive forms of deprivation—in which people's ability to attain resources and the resources to which they feel entitled vary in different ways across time.

Best Practice

Our review indicates that an adequate measure of RD must (a) include a clear comparison referent as part of the questions asked of respondents and (b) measure angry resentment. Researchers also must avoid the ambiguity implied by comparisons between individuals and potential outgroup members by determining whether respondents are thinking of themselves as group representatives or unique individuals. For example, Hafer and Olson (1993) asked working women the extent to which they felt resentful about women's working situation compared to men's working situation, and their answers strongly predicted women's self-reports of political action, $r(69) = +.45$. Similarly, Smith and colleagues (2008) asked faculty members the extent to which they felt that pay for faculty at their university was undeserved compared to the pay for faculty members at comparable universities and whether the difference made them angry and resentful, and their answers predicted their willingness to protest, $r(369) = +.33$ (Smith et al., 2008). Because both measures asked about the respondents' ingroup's relative position, they clearly capture intergroup comparisons.

In contrast, faculty members who reported feeling angry that their individual pay compared to other faculty members was less than they deserved reported more stress, $r(369) = +.38$ (Smith et al., 2008). Similarly, employees of two merged Korean telecommunications service companies who reported that, compared to other company employees, they felt unfairly treated, worse off, and more dissatisfied also reported more interest in leaving the company for another job, $r(274) = +.37$ (Cho, 2004). Because both measures indicate comparison targets within a relevant ingroup, they clearly capture interpersonal comparisons.

We also recommend that future measures include the additional appraisals listed in Figure 1: (a) the importance of the comparison domain, (b) the degree to which the process producing the comparison is viewed as legitimate, (c) the extent to which oneself or one's group is to blame, and (d) the extent to which the situation is expected to change for the better without interference. For researchers interested in measuring women's GRD in comparison to men, there is a 26-item measure of perceived social inequality created and validated by Corning (2000). Simply including a temporal and social comparison component also can lead to more effective RD measures (see de la Sablonnière et al., 2009; Guimond & Dambron, 2002; Pettigrew et al., 2008).

Once RD is measured effectively, researchers can determine its value compared to traditional alternative models. For example, one could compare RD to perceived control or access to resources as predictors of internal states (Adler, 2009; Schnittker & McLeod, 2005). One could compare IRD to social strain models as predictors of individual (deviant) behavior (Baron, 1994; Webber, 2010). One could compare GRD to integrated threat theory as predictors of intergroup attitudes (Stephan & Stephan, 2000). And one could compare GRD to traditional resource mobilization theories as predictors of collective behavior (van Stekelenburg & Klandermans, 2010). We anticipate that such comparisons will typically result in more complex and integrated theories that build on both RD and its supposed rival explanations (e.g., van Zomeren et al., 2008).

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Future Research

Our results also uncovered some unexpected but important patterns. First, comparisons to outgroup members strongly predict internal states and the desire to escape or exit the situation. These comparisons more clearly represent (upward) contrasts between the self and the comparison target, as opposed to comparisons to other ingroup members that could be either assimilation (because one feels similar to the comparison target, one feels inspired by the upward comparison) or contrast comparisons (because one feels dissimilar to the comparison target, one feels threatened by the upward comparison; see B. P. Buunk et al., 2003; Leach et al., 2002; R. H. Smith, 2000).

This possibility leads to a second set of important future research questions. How do characteristics of one's local individual or group situation shape people's comparison choices (the first arrow on the left of Figure 1)? Characteristics worth testing include (a) one's ideology (perhaps as shaped by union membership; Kelly & Kelly, 1994), (b) access to relevant comparison information (Card, Mas, Moretti, & Saez, 2010), (c) individual differences in locus of control (Martin, 1986b), and (d) identity salience—which could be defined by solo status (Pettigrew & Martin, 1987) or relative proportions of ingroup and outgroup members (Crosby, 1976; Turner, 1999).

It also may be that comparisons to outgroup members serve as an important developmental bridge between IRD and GRD—moving people from interpreting their disadvantage as a product of interpersonal circumstances to viewing their disadvantage as a product of intergroup relationships. Certainly, when IRD and GRD are considered as joint predictors of prejudice (Pettigrew, 2002; Pettigrew et al., 2008), we find a "spillover" effect such that GRD mediates the relationship between IRD and prejudice (also see Tougas & Beaton, 2002**;** Tougas, Rinfret, Beaton, & de la Sablonnière, 2005). In these studies, GRD is not a necessary condition for IRD to develop, but increased IRD appears to contribute to an increase in GRD. However, we must caution that there is little consistent evidence for double deprivation (the suggestion that experiences of IRD and GRD are independent and additive; see Dibble, 1981; Foster & Matheson, 1995; M. C. Taylor, 1980; Vanneman & Pettigrew, 1972). Often it is group members with more and not less resources who are most aware of GRD and motivated to challenge the injustice (Pettigrew, 2002).

Second, attitudes toward the system were strongly related to all three types of RD comparisons. This pattern supports our suggestion that all forms of RD are associated with angry resentment and an interest in the external agent responsible for the deprivation. But it is important for future research to validate this assumption. It will be equally important to investigate appraisals that might determine how people will respond (or not respond) to IRD or GRD. For example, knowing who people blame or hold responsible for their deprivation should predict the target of their action (whether it is a public protest or vandalism). As Glick (2002) argues, not only did German members of the Nazi party view Jews as more privileged, they also viewed Jews as responsible for the deprivation of non-Jewish Germans. Therefore, they targeted Jews for retaliation. However, in other contexts, people might view the larger system as responsible for the inequity (Simon & Klandermans, 2001). A second important appraisal could be feelings of efficacy or agency (at either the personal or the group level). Several IRD models (Bolino & Turnley, 2009; Crosby, 1976; Martin, 1986b) propose that if people feel more efficacious, they are more likely to pursue active responses to RD.

Conclusion

It is tempting to propose that researchers simply measure injustice, deserving, or anger directly without reference to particular comparisons. For example, in a meta-analysis of 65 studies of collective action, the average effect size between collective action and noncomparative group-based measures of injustice is +.34 (van Zomeren et al., 2008). Yet there are two reasons why direct measures of injustice could be misleading. First, as van Stekelenburg and Klandermans (2009) write in their review of social movement research, the grievances that prompt protest can represent perceptions of illegitimate inequality (RD), suddenly imposed grievances, or the violation of moral principles (also see Skitka & Mullen, 2002). Without more precise measurement, we cannot know what perceptions lead to feelings of injustice or deserving. Second, as RD research makes clear, two people in the same "local position" can perceive their situation quite differently. Without knowing the comparison standard that a person used, we can mistakenly assume that others in the same position will share the same feelings—but they may well be using an entirely different comparison standard and reaching very different conclusions. This fact helps to explain the apparent predictive superiority of subjective to objective measures of deprivation in our test that compared the two.

What makes RD so useful is the recognition that those who should feel deprived by objective standards often do not feel deprived and those who are not objectively deprived often feel that they are. It is the contextual and flexible nature of social comparisons that remind researchers that RD and injustice are not the property of a single person or group but rather the property of particular relationships. To paraphrase Marx, it is only after people notice that their neighbors have flat-screen televisions and new automobiles that they feel deprived. Similarly, it is when students realize that they do not have access to the educational training that leads to better paying jobs that they feel deprived. In fact, collective and individual challenges to disadvantage often come from people who have more rather than fewer resources—recall Stouffer's Army Airmen who revealed *more* RD while receiving far more promotions than the military police.

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Since Stouffer first proposed RD more than 60 years ago, it continues to be usefully employed throughout the social sciences. However, RD's usefulness as an explanatory concept has outpaced its theoretical specification. We believe that if RD is measured effectively (with a clear comparison, angry resentment and matched to the outcome level of analysis), its full potential as an tool for understanding people's subjective interpretation of objective disadvantage can be achieved. Measured properly, RD is a significant predictor of a wide range of important outcome variables spanning collective action, individual deviance, and physical and mental health.

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Notes

1. Smith and Ortiz (2002) provided a preliminary report on this project using only 35 studies and concentrating on collective protest behavior.
2. Many RD researchers treat affect as a mediator of the relationship between particular disadvantaged comparisons and specific attitudes and behaviors (e.g., Dube & Guimond, 1986). Others treat affect and comparisons as separate indicators of an underlying construct (e.g., de la Sablonnière, Taylor, Perozzo, & Sadykova, 2009), and still others include affective and cognitive items within the same measure (e.g., Corning, 2000). Because we view the specific comparison as a crucial and often overlooked part of the RD experience and we did not have adequate data to test affect as a mediator, we do not address this possibility in this article.
3. Previous typologies often distinguish between normative (or system-facilitating) attitudes and behaviors and nonnormative (or system-inhibiting) attitudes and behaviors. Although the distinction may seem obvious in theory, in practice such a distinction can be difficult to make. Is a faculty member's choice to cancel office hours in response to a pay cut a normative or nonnormative behavior? Is a protester's choice to remain at a rally after being told by police to leave a normative or nonnormative behavior? Often, the assessment of a particular behavior as normative depends on one's group membership and place in a status hierarchy. Although we later make choices that appear to support this distinction (e.g., the assumption that more RD should be associated with less support for the current political system, and the distinction between deviance and achievement behavior), we prefer to avoid labeling specific behaviors as violating or supporting particular norms.
4. We also explored a range of other possible predictors: publication year, respondent age, type of design, whether the RD or outcome measure had been used before, whether the sample represented a homogeneous group or not, and whether the outcome was self-reported or not. Details about these analyses are available from the first author.

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