Small effects: The indispensable foundation for a cumulative psychological science

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Accepted version, 13-Oct-2020 (in press, ***Perspectives on Psychological Science***).
This preprint may differ slightly from the final, copy-edited version of record.

Abstract

We draw on genetics research to argue that complex psychological phenomena are most likely determined by a multitude of causes, with any individual cause likely to have only a small effect. Building upon this, we highlight the dangers of a publication culture that continues to demand large effects: First, it rewards inflated effects that are unlikely to be real and encourages practices likely to yield such effects. Second, it overlooks the small effects that are most likely to be real, hindering attempts to identify and understand the actual determinants of complex psychological phenomena. We then explain the theoretical and practical relevance of small effects, which can have substantial consequences, especially when considered at scale and over time. Finally, we suggest ways in which scholars can harness these insights to advance research and practices in psychology (i.e., leveraging the power of big data, machine learning and crowdsourcing science; promoting rigorous pre-registration, including pre-specifying the smallest effect size of interest (SEOI); contextualizing effects; changing cultural norms to reward accurate and meaningful, rather than exaggerated and unreliable effects). It is only once we accept small effects as the norm, rather than exception, that we can build a reliable and reproducible cumulative psychological science.

Keywords: small effects; research culture; questionable research practices; scientific community

From cognitive functioning, memory, and sleep, to well-being, interpersonal perception, sexual attraction, and mental health, the more we learn about complex psychological phenomena, the more probable it appears that none of them are determined by a single cause. Instead, it is likely that many factors of varying degrees of influence are likely to cause such processes (Ahadi & Diener, 1989; Funder & Ozer, 2019; Gladstone, Matz, & Lemaire, 2019). Hence, with limited variance to explain, any individual cause should be expected to have only a small effect.

Our position draws on recent approaches in genetics. Here, researchers have recognized that complex human psychological phenomena such as personality (Smith-Woolley, Selzam, & Plomin, 2019) or cognitive ability (Plomin, 1999) can be understood only through the complex interplay of multiple genes (Plomin, Owen, & McGuffin, 1994). Consequently, in the early 2000s, geneticists abandoned reductionist one-gene-one-outcome approaches in favor of genome-wide associations studies (GWAS: Boyle, Li, & Pritchard, 2017; Visscher et al., 2017) that identify hundreds, or even thousands of genes associated with human phenotypes. This approach explicitly acknowledges that each individual gene is likely to have a very small effect that may account for only 1.0%, 0.1% or even less variance (Okbay et al., 2016; Smith-Woolley et al., 2019). Indeed, based on the results of this new generation of large-scale genetic studies, Chabris and colleagues (2015) even proposed small effects as the fourth law of behavioral genetics:

"A typical human behavioral trait is associated with very many genetic variants, each of which accounts for a very small percentage of behavioral variability." (p.304)

Rather than relegating genetics to irrelevance, this recognition has ushered in a new era of research in the field of genetics and paved the way for important discoveries (Donnelly, 2008; Mackay, Stone, & Ayroles, 2009). Specifically, in modern genome-wide associations studies, tens of thousands–and sometimes millions (Lee et al., 2018; Liu et al., 2019)–of individuals with varying phenotypes for a particular disease or trait provide DNA samples. Across these extremely large samples of genetic variants, geneticists then track the frequency with which specific genes and the trait or disease in question co-occur, thereby identifying complex systems of dozens and hundreds of candidate genes, which together influence the risk of a specific disease or likelihood of a specific trait. In recent years, this approach has resulted in a broad range of significant advances, from uncovering the genetic architecture of the human plasma proteome, which may crucially inform future drug development (Sun et al., 2018), to identifying etiologic pathways for diseases such as cancer, diabetes, hypertension, inflammatory bowel disease, obesity, and multiple sclerosis (Anderson et al., 2011; Altshuler, Daly & Lander, 2008; Hindorff et al., 2009; Son et al., 2017), to mapping loci that influence adult height (Weedon et al., 2008). In the present article we argue that the same basic logic—that complex phenomena are likely to have many causes—is also bound to be true for the causes of complex psychological phenomena and that similar progress can be made if the field adopts this insight.

To illustrate the multi-determined nature of complex psychological phenomena, consider the case of personality. There is ample evidence that personality is affected by a multitude of diverse factors, ranging from proximal influences such as genetics (e.g., Bouchard, 2004; McCrae et al., 2000; Mõttus, Kandler, Bleidorn, Riemann, & McCrae, 2017; Polderman et al., 2015; Turkheimer, Pettersson & Horn, 2014), childhood experiences (e.g., Eisenberg, Duckworth, Spinrad, & Valiente, 2014; Furnham & Cheng, 2018; Rothbart, Ahadi, & Evans, 2000), family environments (e.g., Bleidorn et al., 2010; Hoffman, 1991; Sutin et al., 2017) and major life events (e.g., Bleidorn, Hopwood, & Lucas, 2018; Specht, Egloff, & Schmukle, 2011) to distal influences, such as neighborhood characteristics (e.g., Götz, Yoshino, & Oshio, 2020, Jokela et al., 2015; Jokela, 2020), climate (e.g., Fischer, Lee, & Verzijden, 2018; Van de Vliert & Van Lange, 2019; Wei et al., 2017), evolutionary presses (e.g., Buss, 2009; Revelle, 1995), and culture (e.g., Church, 2010; Kitayama, Conway, Pietromonaco, Park, & Plaut, 2010; Obschonka et al., 2018).

Similar cases can be made for virtually any complex psychological construct, which are all shaped, to varying degrees, by a broad range of proximal (e.g., Hufer, Konradt, Kandler, & Riemann, 2020; Hutteman, Nestler, Wagner, Egloff, & Back, 2015; Krapohl et al., 2014; Krauss, Orth, & Robins, 2019; Luhmann, Hofmann, Eid, & Lucas, 2012; Orth, 2018) and distal factors (e.g., , Ofosu, Chambers, Chen, & Hehman, 2019; Paluck, 2009; Talhelm et al., 2014; Tankard, & Paluck, 2017; Uskul, Kitayama, & Nisbett, 2008). Against this backdrop, it is not merely unjustified to expect large effects for any individual determinant of complex psychological phenomena, it is also dangerous.

The dangers of demanding large effects

Social scientific disciplines often cultivate publication cultures that favor or even demand large effects (Fanelli, Costas & Ioannidis, 2017). In an academic system where decisions about hiring, promotion, tenure, and funding are largely determined by publications (Nosek, Spies, & Motyl, 2012), the pressure to publish large effects is dangerous for at least two reasons. One reason is that it rewards lucky or exaggerated effects that are unlikely to be real (Lindsay, 2020; Shrout & Rodgers, 2018) and encourages practices that are likely to yield these inflated effects (Munafò et al., 2017; Nosek et al., 2012), such as *p*-hacking (Nelson, Simmons, & Simonsohn, 2018), optional stopping (Lakens, 2019), HARKing (Kerr, 1998), and other questionable research practices (Wicherts et al., 2016). In doing so the publication culture will contribute to the lack of replicability plaguing the social sciences in general (Camerer et al., 2016; 2018) and psychology in particular (Open Science Collaboration, 2015). The second reason is that an emphasis on large effect sizes increases the chances of overlooking the small effects that are most likely to be real (Funder & Ozer, 2019), thereby hindering attempts to identify and understand the actual determinants of complex psychological phenomena.

The importance and consequence of small effects

Does this new focus on many causes (or genes in the case of genetics) and small effects mean the effects are unimportant? Not at all. Understanding complex psychological phenomena remains as important as it ever was. The new focus merely tells us that to complete this important task we must focus on the interplay of many tiny causes working alone and in concert, with each individual cause playing a smaller individual role than we previously may have thought[1]. Thus a nuanced consideration, rather than categorical dismissal, of small effects can yield important theoretical advances that would otherwise be missed (Murray et al., 2020; Prentice & Miller, 1992).

In addition, some small effects may also have direct real-world consequences (Funder & Ozer, 2019; Gelman & Carlin, 2014). This phenomenon is especially true for effects that accumulate over time and at scale (Abelson, 1985; Bond et al., 2012; Funder & Ozer, 2019; Greenwald, Banaii, & Nosek, 2015; Matz, Gladstone, & Stillwell, 2017). A particularly compelling example of this phenomenon is personality, where effects accumulate over entire lifetimes (Noftle & Robins, 2007; Prentice & Miller, 1992) and across most major life domains, including occupational attainment, social success, personal relationships, financial security, and mortality (Ozer & Benet-Martínez, 2006; Roberts, Kuncel, Shiner, Caspi, & Goldberg, 2007;

[1] Prentice and Miller (1992) noted that in some cases researchers may deliberately seek out small effects under the assumption that if even minimal manipulations can have effects (e.g., Tajfel, 1970; Sawaoka & Monin, 2018), or if small effects replicate across very different situations and stimuli (e.g., Lu et al., 2017; Klein et al., 2018), then the basic phenomena underlying these studies must be robust, strong, and wide-reaching.

Soto, 2019). Thus, even comparatively removed predictors such as climate (Fischer et al., 2018; Van de Vliert & Van Lange, 2019; Wei et al, 2017) or physical topography (Götz, Stieger, Gosling, Rentfrow, & Potter, 2020) that may have only a small effect on personality may ultimately be quite consequential.

Similar processes can be observed in other fields of psychology (e.g., consumer spending: Matz, Gladstone, & Stillwell, 2016; Weston, Gladstone, Graham, Mroczek, & Condon, 2018; social influence: Bond et al., 2012; Kramer, Guillory, & Hancock, 2014; Ofosu et al., 2019) and other disciplines such as medicine and education. For instance, the correlations between aspirin and prevention of heart attacks (r = .03; Rosnow & Rosenthal, 2003; Rosenthal, 1990; Steering Committee of the Physician's Health Study Research Group, 1988); calcium intake and bone mass in premenopausal women (r = .08; Meyer et al., 2001), ibuprofen intake and pain alleviation (r = .14; Funder & Ozer, 2019; Meyer et al., 2001) or cardiac patient education and exercise (r = .09; Rosenthal & DiMatteo, 2001) are small to minimal, according to Cohen's classic guidelines (1988), but still highly consequential from a public health perspective. The same is true for the relationships between educational interventions such as growth mindset trainings (GPA increase of .05 standard deviations; Yeager et al., 2019; for a meta-analysis (r = .08) see Sisk, Burgoyne, Sun, Butler, & Macnamara, 2018) or universal free school breakfasts (math achievement increase .09 standard deviations; Frisvold, 2015) and academic performance. All of these effects can scale up to yield large impacts at national or global levels. For example, over the course of a year, the learning benefits of handing out free school breakfast may equate to approximately 1.6 months of schooling per child (Kraft, 2020), and in a group of 10,845 individuals taking aspirin, 85 heart attacks might be prevented (Funder & Ozer, 2019; Rosenthal, 1990).

Moving forward: Towards a cumulative psychological science built on small effects

So far, we have argued for: (1) the theoretical necessity of small effects, (2) the dangers of marginalizing them in favor of unrealistically large effects, and (3) the empirical relevance and practical significance of small effects. In this section we discuss research implications and outline specific steps that reinforce and leverage the potential of small effects to advance a robust and reproducible psychological science.

First, to combat the issue of inflated effect sizes, we re-affirm pre-registration (Nosek, Ebersole, DeHaven, & Mellor, 2018; Wagenmakers, Wetzels, Borsboom, van der Maas, & Kievit, 2012) and registered reports (Chambers, 2019; Hardwicke & Ioannidis, 2018; Nosek & Lakens, 2014) as potent means to gain a more realistic understanding of actual effect sizes in psychological science. Importantly, to be useful for our purposes, pre-registrations need to contain clear specifications of methods, study procedures, and statistical analyses, and researchers need to strictly adhere to their pre-registrations and justify deviations wherever they occur. Then and only then will pre-registrations and registered reports buffer against questionable research practices that likely yield inflated effect sizes (Lakens, 2019). Moreover, when pre-registering, we strongly encourage researchers to specify the smallest effect size of interest (SEOI; Funder et al., 2014; Anvari & Lakens, 2020) that would still be considered meaningful, and also use this estimate to inform power analyses. To be sure, just because all small effects could be relevant, this does not mean that all effects will be relevant, and it remains the task of the study investigators to make a compelling case for why their effects matter.

The most appropriate way of defining the SEOI depends on the study context and should thus be chosen on a case-by-case basis, but there are a number of existing approaches that might be useful starting points for researchers who are new to this exercise. For example, the concept of clinical significance posits that effects only matter if they make a difference that individuals notice (Kazdin, 1999). The thresholds for such minimally detectable differences can be extracted through so-called anchor-based methods (Anvari & Lakens, 2020), which can either be implemented as longitudinal within-person designs (i.e., global transition method) or as cross-sectional between-person designs (i.e., subjective comparison method). In the global transition method, within a suitable timeframe the same individuals are assessed twice on a psychological construct of interest and are asked to indicate whether they perceive a change. The mean change in scores from T1 to T2 among individuals who just about perceived a difference is then used as an estimate for the minimally detectable difference and hence serves as the SEOI (Button et al., 2015). The same principle is applied in the subjective comparison method, in which interaction partners are both assessed on a psychological construct and are then asked to indicate how strong, if at all, a difference they perceive between themselves and their interaction partner regarding the construct of interest (Anvari & Lakens, 2020). In more applied and intervention-focused settings, cost-benefit analyses can be helpful to assess when an effect is too small to claim practical importance (Bleidorn et al., 2019; Robertson, Grimes, & Rogers, 2001), while the sheer existence and robustness of an effect can be enough when the primary goal is to develop psychological theory (Murray et al., 2020; Prentice & Miller, 1992).

Overall, the emphasis on pre-registration is in line with a rising recognition that, in contrast to widespread underpowered studies in psychology, which likely report exaggerated effect sizes (Button et al., 2013; Schäfer & Schwarz, 2019; Szucs & Ioannidis, 2017), effect sizes obtained from well-powered pre-registered studies (Funder & Ozer, 2019; Miller, 2019; Schäfer & Schwarz, 2019; Schooler, 2011; Szucs & Ioannidis, 2017) accurately capture highly reliable effects. As in the case of genetics (Bycroft et al., 2018), such studies of small-yet-robust effects in psychology require large-scale research designs (De Boeck & Jeon, 2018) and computationally powerful analytic methods (Chen & Wojcik, 2016; Kosinski, Matz, Gosling, Popov, & Stillwell, 2015). Fortunately, the advent of big data (Adjerid & Kelley, 2018; Harari et al., 2016), novel machine learning methods (Bleidorn & Hopwood, 2019; Yarkoni & Westfall, 2017) and crowdsourcing science (Chartier, Riegelman, & McCarthy, 2019; Moshontz et al., 2018; Uhlmann et al., 2019) now afford opportunities to identify such small yet meaningful effects. Furthermore, special efforts should be made to eliminate confounding variables and improve measurement precision to further increase the reliability and reproducibility of psychological effects (De Boeck & Jeon, 2018; Funder & Ozer, 2019).

To be clear, we do not assert that large effects are flawed or unreliable *per se*. Indeed, under certain circumstances, such as in tightly controlled lab studies that explicitly seek to isolate an effect, large effect sizes might be very accurate, albeit limited in their external validity. Moreover, just as with any distribution, the fact that the majority of real effects are likely to be small does not rule out the possibility that some real effects are large. Rather, we believe that in evaluating research output, the reliability and precision of an effect should take primacy over its size, and that pre-registered, well-powered, and rigorously analyzed studies likely offer the best way to achieve such an outcome.

Second, to facilitate a better understanding of the meaning and relevance of effects, we advocate for more contextualization in the way in which effects are reported. One promising strategy is benchmarking (Funder & Ozer, 2019; Kraft, 2020). That is, an effect should be evaluated in light of typical effects sizes from the immediately relevant, specialized literature (Bosco, Aguinis, Singh, Field & Pierce, 2015; Gignac & Szodorai, 2016; Richard, Bond, & Stokes-Zoota, 2003) rather than generic one-size-fits-all thresholds such as those proposed by

Cohen (1988). In conjunction with rigorous pre-registration as advocated above, benchmarking can create a mutually reinforcing and self-correcting cycle in which carefully pre-registered studies lead to the publication of more realistic effect sizes and null findings. Such a system would simultaneously decrease publication bias and increase the accuracy of meta-analyses (Grand et al., 2018), which would help provide more precise calibrations of empirical benchmarks for specifying meaningful SEOIs in future pre-registrations. For practical applications and interventions, such as those commonly encountered in clinical and educational psychology, implementation costs (Duncan & Magnuson, 2007; Harris, 2009; Levin & Belfield, 2015), scalability (Kraft, 2020), and expected growth or change in the absence of an intervention (Hill et al., 2008) might be useful additional criteria to assess the relevance of an effect. To illustrate, while individualized tutoring (0.23 SD; Cook et al., 2015) produces substantially bigger improvements in academic achievement than universal free school breakfast (0.09 SD; Frisvold, 2015) or a one-hour online growth mindset intervention (0.05 SD; Yeager et al., 2019) – the latter strategies are much cheaper and more feasible to implement at scale. More broadly, in contextualizing effects, evaluating relevance and specifying SEOIs researchers should also consider how consequential their outcomes are. Indeed, for some extremely important and consequential outcomes (e.g., suicide prevalence, adherence to social distancing during a pandemic) any effect can matter. Put differently, whereas identifying policy-relevant psychological forces that explain 1% of variation in people's propensity to shelter-at-home during the Covid-19 pandemic would likely justify extensive research efforts and funding, accounting for 1% of variation in Stroop task performances may not. Crucially, it is conceivable that some of these extremely important outcomes are largely or even entirely determined by factors that each exert only a very small effect; in such contexts, declaring effects below a certain magnitude to be too small to matter may mean that we will never understand the phenomenon at hand, just as geneticists would have sentenced themselves to never understanding various important phenomena if they had held on to the position that some effects (e.g., explaining less than 1% of the variance) are in principle too small to be important.

Relatedly, contextualization should also refer to the way that effects are presented. Rather than casting effects in terms of standardized but abstract and difficult-to-interpret effect size metrics, researchers should strive to make the meaning of effects understood by highlighting how they translate into real-world outcomes. Promising examples include cases, where in addition to reporting betas, *r*s, or Cohen's *d*s, researchers explicitly stated the corresponding changes in prevented heart attacks (Rosenthal, 1990; Rosnow & Rosenthal, 2003), money spent (Matz, Kosinski, Nave, & Stillwell, 2017), class percentile rank (Kraft, 2020) and vote gains during the 2016 UK Brexit referendum (Garretsen, Stoker, Soudis, Martin & Rentfrow, 2018).

Of note, this approach is more challenging if researchers report certain psychological outcomes that may not have a natural metric and are often assessed on more arbitrary metrics such as Likert scales (Blanton & Jaccard, 2006); however, even under such circumstances a better understanding can be achieved if researchers undertake efforts to contextualize their effects. For example, to contextualize the effect of neighborhood poverty on subjective wellbeing (SWB), a core psychological construct without a natural metric, Ludwig and colleagues (2012) explained that a 1-SD decrease in neighborhood poverty (approximately 13 percentage points) corresponded to an increase in SWB that is equivalent to 1) two thirds of the gap in SWB between U.S. blacks and whites or 2) the SWB gap between families that differ in their annual incomes by $13,000.

We hope that together these steps enable researchers to gain a better understanding of when and how small effects matter. This being said, we do not wish to replace thoughtless adherence to universal effect size thresholds such as those proposed by Cohen (1988) with an equally thoughtless, universal claim that all effects matter. Rather, we contend that on a general level, most real effects in psychology will be small, and that many of these small effects may be of theoretical and practical importance. However, this claim does not obviate the need for researchers to show that their effects – however big or small – matter. In other words, we encourage psychologists to think differently about their effects, but no less hard.

Conclusion

We argue here that just as in the field of genetics, research on the causes of complex psychological phenomena needs to stop searching for implausibly large effects and invest more effort in identifying and contextualizing robust, albeit small, effects (Funder & Ozer, 2019; Miller, 2019). Such research will provide the foundation for future work that can seek to understand how exactly these many small influences combine to influence consequential outcomes. We call on researchers, reviewers, editors, institutions, societies, publishers, and funding bodies to cease expecting or demanding large effects. If we are to progress as a science, we must adjust our expectations and align our incentive structures to reward accurate and meaningful, rather than exaggerated and unreliable effects (De Boeck & Jeon, 2018; Lindsay, 2020; Munafò et al., 2017; Spellman, 2015). It is only once psychological science accepts that small effects are to be expected—as the norm, rather than the exception—that we have any realistic hope of understanding causal processes in our field. Only then can we start building a cumulative psychological science – on the foundation of small effects.

Acknowledgement

We thank Paige Harden for her thoughtful comments on an earlier draft of this manuscript

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