



# The fame monster: Unintended consequence of fame for psychological science



Christopher J. Ferguson

Department of Psychology, Stetson University, Deland, FL, 32723, USA

## ABSTRACT

Scientific fields often assign fame to certain individuals and studies to delineate a narrative about the field's importance, success, and contribution to knowledge. However, such assignments of fame can result in a situation wherein assumptions of scientific merit may follow from fame, rather than fame following from clear scientific merit. This can result in several problems for the field including conflicts of interest in peer review, resistance to disconfirmatory results, politicization of some research areas, and may contribute to psychology's replication crisis. It is suggested that principles of replicability, transparency and open science can, ultimately, help psychological science identify those empirical results which are most verifiable and, as such, deserving of "fame."

"Reality must take precedence over public relations, for nature cannot be fooled."

~ Richard Feynman

## 1. Introduction

Seeking fame is a common and not unreasonable goal even for scientists. *Famous* scholars can often act as representatives (or perhaps mascots) for their fields and famous studies can help define social narratives about that field's contribution to knowledge. For instance, evolutionary biology continues to be represented by the figurehead of Charles Darwin in large part because his *On the Origin of Species* has survived over 150 years of scientific scrutiny. However, fame, when assigned to studies that are narratively appealing but scientifically flawed, can do more harm than good both in regards to scientific progress and the reputation of the field. At present, I argue psychology has emphasized narrative appeal rather than empirical veracity and rigorous replicability in assigning fame.

Defining what is meant by fame (or more euphemistically *eminence*) can be tricky as fame can be assigned by different individuals including news media, professional guilds such as the American Psychological Association (APA), journal or university press releases, etc. under differing circumstances. Sternberg (2016) in an introduction to a special journal issue on fame suggests multiple criterion for fame including quantity of publications controlling for impact factor, awards and honorary doctorates, grant funding, nominations by distinguished referees, etc. Diener, Oishi, and Park (2014) rely on a combination of awards and recognitions by professional guilds (e.g. APA), citation

metrics such as the h-index and the degree to which they were cited in introductory psychology textbooks.<sup>1</sup> Both of these approaches are very inward looking; that is to say they consider fame within the field as opposed to public recognition. Presumably this is intended to distinguish academic psychology from pop psychology figures who may often have easier access to public fame and, thus, compete for attention with academic psychology (Lilienfeld, 2012a). Yet, academic psychologists at least sometimes seek public fame as well. Feist (2016) provides a broader approach to understanding fame. Feist's approach is a graded one, with science moving from the mundane to the "normal" to creative and finally transformative. Transformative science is that which is most likely to achieve fame for both the scientists who create it and the knowledge generated. Although less quantifiable perhaps than counting citations or awards, this approach intuitively works best when considering both institutional fame within the field and with the public and news media. Public fame might also be quantifiable, albeit via different sources ranging from quantified newspaper coverage, books sold, number of media appearances, or through social media followers. None, naturally, are perfect nor entirely overlap with a consistent vision of fame.

The assignment of fame itself is complex. Fame may be sought by scholars, but as non-living entities, studies do not seek fame. However, fame can be assigned to both. As noted above, factors such as citation indexes, awards and grants all play a role. But there are likely soft, difficult-to-quantify factors as well. One might speculate that professional guild awards have as much to do with the utility of scholarly work for the advocacy, political and moral agendas of those guilds as they do any particular rigor in the science. A close empirical examination of this would be worthwhile. News media assign fame to

E-mail address: [CJFerguson1111@aol.com](mailto:CJFerguson1111@aol.com).

<sup>1</sup> Diener appears as #10 out of 100 on his own list of "extremely eminent" psychologists.

studies that are cool, counterintuitive, or morally/emotionally evocative not always those that are most rigorous (Selvaraj et al., 2014; Vettehen and Kleemans, 2018). These institutional and news media processes are not independent as press releases from professional guilds are likely to enhance the probability of news media attention. Although there is little data on this, it may be that textbook authors take particular note of studies appearing in both news media and professional guild newsletters. Their coverage may be further regulated by publisher peer-reviewers who may, themselves, have investment in particular narratives that have little to do with actual scientific rigor. The field of psychology does not have a kind of supernatural agency by which it assigns fame but rather the creation of fame is influenced by numerous actors including professional guilds, through press releases and awards, universities through their own press releases as well as news media attention. None of these assignments, however, are likely random but presumably occur to, at least in part, support the motives, missions and predominating narratives among such influence makers.

Consider the case of the famous Stanford Prison Experiment which has long been a darling of psychology textbooks.<sup>2</sup> The Stanford Prison Experiment's role in psychology has, arguably, been as a demonstration of the power of psychological principles on human behavior. Namely, that we are all potentially manipulated by context and assigned roles. Yet, in recent years, serious questions have emerged regarding, not only the ethics, but the validity of the Stanford Prison Experiment. For example, concerns have been raised regarding the potential for selection biases (Carnahan & McFarland, 2007) and demand characteristics (Gray, 2013). Replication efforts have not always replicated the original study (e.g. Reicher & Haslam, 2006) and news analyses of archival data have called the experiment a “sham” (Blum, 2018) although the study's main author, Phil Zimbardo has defended his study. The Stanford Prison Experiment has been famous for decades but, whether valid or not, this does raise the question of whether this study was famous for the right reasons. In other words, was it famous because it was rigorous or because it told an exciting narrative that made psychological science look important?

The same is true for some of the most famous studies in the field. For instance, the bobo doll studies of modeled aggression (e.g. Bandura, Ross, & Ross, 1961) are often reified as factual despite that significant criticisms of their methods have persisted over the years (e.g. Cumberbatch, 1992; Tedeschi and Quigley, 1996). Famous studies such as Little Albert (Griggs, 2014) or Milgram's obedience studies (Griggs, 2017) are commonly misrepresented in introductory psychology textbooks in ways that tend to exaggerate their consistency, rigor or generalizability to real life and downplay criticisms or controversies (Ferguson, Brown, & Torres, 2018; O'Donohue & Willis, 2018; Warne, Astle, & Hill, 2018). This is not to say that such studies are unworthy of discussion but that there is a notable tendency for them and similar ideas to be communicated as a kind of creation myth of psychological science, particularly in textbooks (Ferguson et al., 2018) and to the general public. Reification, whether in textbooks or press releases, can occur in multiple ways. These may include, failure to document controversies or failed replications of a famous research result, language used to discourage critical analysis of a famous research result, failure to note if results were weak or of limited practical significance, or overgeneralization of an esoteric lab result to real-life behaviors to which it may not apply. Reification, thus, inherently discourages critical analysis and critical thinking. Narrative appeal, whether in textbooks or in press releases and policy statements should never be promoted at the expense of empirical truth as this only serves to discredit the field when those untruths are revealed.

<sup>2</sup> Curiously, the famous creator of the Stanford Prison Experiment, Phil Zimbardo, is not included in Diener et al., (2014)'s list of extremely eminent psychologists.

## 2. The perils of fame

For a science, however, the assignment of fame based on convenient social narratives, institutional priorities, or excitability and ability of a finding to promote the importance of the larger field has the potential to do significant if unintended damage. The assignment of fame may bring with it an implication of trust or even conformance that may undermine critical thinking. Below I discuss some of the potential perils of fame.

### 2.1. Peer review

The practice of peer review helps ensure the scientific merit of published studies. Like all practices, peer review has limitations from difficulty in detecting hacking/fraud (Haug, 2015) to inefficiency and potential bias (Schwartz & Zamboanga, 2009).

The reification of fame can result in situations in which particular individuals have outsized control over peer review, particular of data that both supports and challenges their own personal beliefs. Famous scholars in a particular field may be oversampled to provide peer review. As a consequence, they become unusually powerful in regard to reject/accept decisions for new work in their field, including work that may challenge their own beliefs and findings. Given the anonymity of peer review, studying this issue can be difficult. However, some analyses suggest that peer review is generally susceptible to *herding* behavior around popular theories (Park, Peacey, & Munafò, 2014) and research supports that expert reviewers are hostile to data that conflicts with either their personal views or popular theories (Phillips, 2011). Given that *myside bias* is just as strong among the intelligent and educated (Stanovich, West, & Toplak, 2013) this can create a situation in which famous scholars act as gatekeepers regarding data that might challenge their own work. Fame then can become one source of publication bias as well as produce a culture of *obligatory replications* (Ioannidis, 2012) in which theory supportive research is preferred over disconfirmatory research. Obligatory replications are replications of results for which there is pressure to reproduce, faithfully, the findings of earlier studies rather than attempt to falsify them. Replications occurring within the same lab are the most obvious example, but obligatory replications can spread to the degree to which theory-confirming results are preferred for publication over null or falsifying results.

### 2.2. Absence of neutral, independent replication and the Harry Potter Hypothesis

Obligatory replications (Ioannidis, 2012) occur when a culture develops in a scientific field where there is pressure to produce more data to support a preexisting theory and hostility toward data that would challenge it. Under such a cultural milieu, theories that become *truthy* (a concept in which the truth of a belief is held certain or dear, despite absence of clear, quality empirical support) may become resistant to falsification. Scholars, particularly less famous scholars, may disbelieve their own disconfirmatory findings, concerned they are less qualified or worthy that the famous scholars advocating the theory in question.

Such a culture of obligatory replications can be enforced via the *Harry Potter Hypothesis* (HPH).<sup>3</sup> Variations of the HPH imply that the research world is made up of wizards and muggles (i.e. those incapable of doing magic.) Thus, if a team of less famous scholars fail to support a famous theory, this must be because they are muggles (i.e. incompetent, less talented researchers) lacking the *flair* (e.g. Baumeister, 2016) necessary to find evidence for the theory. Of course, researchers do differ in skill but the HPH is typically used to disparage less famous scholars

<sup>3</sup> I cannot claim to be the originator of this term, though it has existed previously as an informal meme in scientific conversations about replicability.

and to defend famous theories against null findings. As an example, speaking of his own research laboratory Baumeister (2016) has said “My laboratory has been working on self-regulation and ego depletion for a couple decades. Most of my advisees have been able to produce such effects, though not always on the first try. A few of them have not been able to replicate the basic effect after several tries. These failures are not evenly distributed across the group. Rather, some people simply seem to lack whatever skills and talents are needed. Their failures do not mean that the theory is wrong.” Though undoubtedly in good faith, such thinking by a famous psychologist illustrates the pressure to produce a particular result under the assumption it is true, and those who do not find it are flawed researchers.

Another infamous example took direct aim on study replicators. In an early version of a column for the *APS Observer* Susan Fiske referred to individuals critiquing published studies as “online vigilantes”, “data police”, “destructo-critics” engaged in “methodological terrorism”. (Fiske, 2016). Although some of this harsh language was removed from the final published version, this came only after the draft was leaked online and a furor ensued. Although Fiske presumably intended to speak to the tone of some methodological critiques taking place on line, her own article arguably came across as bullying and enforcing of a status quo discouraging failed replications, even if that was not her intent.

Researchers do, of course, vary in skill. However, I argue that scholars have a duty not to merely assume that skill can be inferred from experimental results. Rather if a replication failure is suspect, detailed documentation of that study's failures must be elucidated rather than assumed. Further, that process of critical analysis should be applied to the original results as well which also may have involved critical errors. Such critical analysis is aided by open data and methods and obscured by the non-transparency more typical in psychological research. Lastly, I observe that defenders of a theory are likely to prove adept in tearing down any failed replication of it. Thus, critical analyses conducted by neutral observers in an open and transparent process are most convincing.

Several caveats are worth mentioning. First, though robustness is certainly a factor that could play more into eminence, this does not mean that gravity of findings is unworthy of consideration. Findings that are both robust and change the field as we know it are particularly worthy of eminence. Though at present, the focus has been more on the latter without careful consideration of robustness. Second, the field does need to be cautious not to disincentive all risk taking among researchers. Studies that are conducted in good faith, preregistered, etc., but which ultimately fail to replicate should not result in their authors experiencing shame. However, promotion of novel findings (i.e. those that are startling or unexpected) would best proceed after robustness is confirmed.

### 2.3. Politicization of science

The development of rigid ideological theories promoted by their fame can occur in any research field no matter how dull to the general public. That is to say, the rigidity of a theory does not necessarily relate to its renown in the general public as both public and institutional communities can be sources of fame. But both psychological theories and individual psychologists can attract fame for the perceived moral value of their work. This can amplify the resistance of such theories to falsification given that such theories are perceived as contributing to the public good or protecting vulnerable individuals (children, racial or sexual minorities, women, disabled individuals, etc., see Ferguson, 2013) or, conversely, protecting social narratives, liberal or conservative, to which academic psychology has become aligned. In other words, scholarly work is often judged for its moralistic value rather than rigor.

Examples of this phenomenon are likely legion. However, examples may include microaggressions (Lilienfeld, 2017a), psychological effects

of abortion (MacNair, Wiley, Meehan, & Rider, 2008), spanking (Larzelere & Cox, 2013) and media violence (Ferguson, 2013). Particularly of interest is the issue of microaggressions which are routinely discussed in public and taught to undergraduate students as if non-controversial and empirically based. But an interesting article by Lilienfeld (2017a) argued that the scientific basis for the construct of microaggressions is quite limited. This led to a fascinating debate between proponents and detractors of the microaggression concept (Haidt, 2017; Lilienfeld, 2017b; Ong & Burrow, 2017; Sue, 2017). By ignoring this debate, communications about microaggressions do more to misinform than inform, even if done for a good-faith advocacy cause. Theories of effects can become particularly famous to the extent they are embraced by advocates for a cause and promoted in general news media. This is not to say that such theories are inherently unworthy of discussion because they are famous, but that fame can be amplified by a theory's perceived public good and, as such, its resistance to null results strengthened.

### 2.4. Unhealthy incentives

Motivation to achieve eminence can result in unhealthy behaviors on the part of scholars. To achieve fame, scholars can become keyed into perverse incentive structures that promote attention seeking at the cost of high-quality science. Resulting behaviors include p-hacking (attempting to convert non-significant results into significant results), publishing numerous papers rather than a few high-quality studies, publishing papers with counter-intuitive shocking results likely to garner newspaper headlines, and refusal to incorporate disconfirmatory results into a theoretical world-view.

The result can create a field that is more histrionic than high-quality. Fame is privileged over high-quality, good science, which can often be slow, incremental, and tedious.

## 3. Reimagining fame

Like many things, fame is neither inherently good nor bad. Seeking fame and appreciation is a common, normal human motivation even among psychologists (Freund, 2017) and certainly many individual psychologists and their studies merit adulation. However, it may be valuable to reconsider how we delineate when fame has been warranted. Below, I offer several suggestions. These tend to focus on fame as it applies to studies, with the assumption that fame for individual psychologists would then follow from the value of their studies. All of these would require purposeful cultural shifts in the way that our field assigns fame to both individuals and the studies they produce.

### 3.1. Open science

The essence of fame for psychological studies ought have less to do with whether the study is inherently cool, or cleverly illustrates a psychological principle but whether the results of the study are robust. The open science movement (Nosek & Bar-Anan, 2012) has been gaining momentum in recent years and many scholars are considering ways to incorporate principles of openness in their research work. Open science includes several different elements from open sharing of data, open and post-publication peer review to open access for research publications. How open science might best fit with psychological research in some areas is still being debated (e.g. Lilienfeld, 2012b) and some unforeseen pitfalls may still emerge (e.g. open access to publications may place financial burden on study authors which may not be practical for authors without external funding.) Furthermore, researchers may have more confidence in publishing preregistered studies that contradict famous older studies, as they will be able to note the increased rigor of open science evening the playing field when challenging famous scholars. However, studies which have been conducted allowing for thorough pre and post publication vetting are better

candidates for fame than studies shielded from independent scrutiny. Journal publishers could prioritize open science practices for attention through badges and priority for press releases, thus linking fame concretely to open science. To the extent that badges themselves become a mark of reliability they may enhance the prestige of research articles and the authors who conduct them.

For instance, data from studies should more routinely be made available during the peer review process. And journals could serve as repositories for data in the event of a post-publication data request. In the electronic age, some journals provide forums for post-publication peer review and this could become more routine. Such innovations may help us to better delineate which studies deserve fame. Although open science principles were originally developed in the context of randomized controlled trials, preregistration and open science can be applied to all kinds of scientific data including, experimental, correlational and qualitative.

### 3.2. Replication

Replication and reproducibility are key elements of good science, yet psychological science has a history of eschewing these qualities (Nosek, Spies, & Motyl, 2012). In recent years, large replication efforts have questioned the validity of numerous famous experiments in psychology (Hagger & Chatzisarantis, 2016; Open Science Collaboration, 2015.) At present, fame tends to be reserved for studies that get the most buzz, both inside and outside of the field, even if those studies later prove to be difficult to replicate. As such, there are disincentives to conduct replication studies as these may have less chance to become famous. This cultural phenomenon can be worsened by *death by press release* wherein scholars exaggerate the consistency or importance of their findings in order to attract news media attention and public fame. Empirical data suggests that study press releases often exaggerate results and fail to note study limitations or conflicts of interest funding (Moynihan, 2003). Changing this culture is obviously going to require a deliberative process both by publishers, institutions and scholars.

It would seem reasonable to suggest that fame be reserved for studies that are replicable under rigorous independent evaluation. Obligatory replications (Ioannidis, 2012) from advocates of a theory are no longer sufficient. By contrast, replications that are successful under rigorous, open, preregistered conditions and include efforts by neutral and skeptical labs offer greater support for the underlying proposition. Experiments that survive such efforts are the best candidates for fame. Journals could provide incentives for replication studies just as some do for open science. This could take the form of badges for open science replication studies, as well as placing increased priority for press releases on findings that have been replicated rather than for new, novel findings.

### 3.3. Thinking creatively

The problem of obligatory replications highlights one other concern, namely that scholars may find themselves drilling down on a hypothesis, ultimately defending it rather than testing it with an eye to falsification. This can create problems for both science and the researcher, with the researcher ultimately becoming famous based on the truthiness of a belief associated with his/her name and ultimately jealous of that fame.

Creativity in science is inherently challenging to prior beliefs, to unconscious assumptions and to oneself (Sternberg, 2019 in press.) Creativity and novelty are, as such, similar sounding constructs. However, novelty can be thought of as the production of new but counter-intuitive and exciting results that attract attention including from news media. By contrast, creativity involves new ways of problem solving that may focus on falsification of old idea through innovation. Creativity as defined here is more thoughtful, deliberative, skeptical and purposeful than novelty. Kaufman and Beghetto (2009) note that

success in a field can come either from making incremental advances or from redirection or reinitiation in which entire fields are transformed. Put simply here, there is value in assigning virtue not only to those whom we move psychology forward, but also appreciating the accomplishments of those who challenge the assumptions of academic psychology. Particularly when such challenges from open, preregistered studies, their contributions should be recognized.

As such, my last thought is that scientists would benefit from occasionally doing something different or at very least reversing course and attempting to falsify one's own beliefs and theories. Scientists may start in on a new field, ready to challenge it as well as their own pre-existing beliefs. Scientists could also revolutionize their thinking by flipping their approach. For instance, instead of considering the negative outcomes of a thing (e.g. using marijuana, spanking, traditional masculinity etc.), attempt to discover some positive outcomes associated with that same thing. In the end there may be none, but by taking a creative approach, a scientist can be more assured that personal blinders have not corrupted their own research path. Under conditions of open, preregistered science and a willingness to seek independent replication, this may be a reasonable path to fame for any psychological researcher. Although it is worth looking at ways to incentivize replications, creative approaches to science are always going to particularly attract fame. It is worth considering how best to be assure that it is rigorous, creative work rather than novel but clumsy work that most attracts fame.

## 4. Concluding thoughts

Fame is not inherently a negative. My point is that, applied, assigned and used poorly, it can have unintended negative results. Used positively, fame can certainly have positive outcomes as well. Eminent psychologists can help focus public policy on important findings, particularly when those findings are produced by rigorous, open science principles. Fame can also reduce insecurities, at least in some researchers, giving them the freedom to pursue more challenging or even unconventional topics, or challenge the status quo. Fame obviously can feel good so long as one works to not let it get to their head! Fame can, in this sense, be thought of as any tool. When used positively, it can bring good outcomes, when used selfishly it can bring bad outcomes.

Fame or eminence is likely to continue to be a sought after goal for many psychologists<sup>4</sup> even if most do not achieve this (Freund, 2017). This is not necessarily a bad thing. However, fame can inadvertently result in the reification of dodgy theories and defensiveness toward the protection of old ideas. By reevaluating the culture of how fame is assigned by the academic community and, through press-releases, further disseminated to the general public, we can ensure that fame is focused on science that is rigorous, replicable and transparent rather than that which is narratively appealing, reifying of psychology as a field, or morally convenient. This will require non-trivial cultural shifts for the field, changes in the practices of scholars and journal publication and, potentially, new ways of understanding contributions to science aside from relying on accolades from professional guilds or citation counts.

## Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.newideapsych.2019.01.003>.

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<sup>4</sup> This author claims no immunity, for the record.

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