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Guilt by association: How scientific misconduct harms prior collaborators

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ABSTRACT

Recent highly publicized cases of scientific misconduct have raised concerns about its consequences for academic careers. Previous and anecdotal evidence suggests that these reach far beyond the fraudulent scientist and (his or) her career, affecting coauthors and institutions. Here we show that the negative effects of scientific misconduct spill over to uninvolved prior collaborators: compared to a control group, prior collaborators of misconducting scientists, who have no connection to the misconduct case, are cited 8–9% less often afterwards. We suggest that the mechanism underlying this phenomenon is stigmatization by mere association. The result suggests that scientific misconduct generates large indirect costs in the form of mistrust towards a wider range of research findings than was previously assumed. The far-reaching fallout of misconduct implies that potential whistleblowers might be disinclined to make their concerns public in order to protect their own reputation and career.

1. Introduction

Scientific misconduct affects many more than those who commit it. Consider for instance the case of the Center for Developmental Biology in Kobe, Japan, where in 2014 two retractions due to scientific misconduct led to a change in directors, half of its laboratories closing, merging, or moving elsewhere, and a 40% budget cut (Cyranoski, 2015). In other cases, such as the one involving Dutch social psychologist Diederik Stapel, the work of graduate students was called into question due to the unethical actions of their supervisor.[1]

This raises the question of how far the ripple effect of scientific misconduct reaches. Prior research has shown that co-authors of scientists found guilty of misconduct experience significant drops in their publication flows (Mongeon and Larivière, 2015). While research on actual misconduct cases is scarce and, due to the limited amount of cases, rather descriptive (Lubalin and Matheson, 1999; Pozzi and David, 2007; Redman and Merz, 2008; Resnik and Dinse, 2012; Reynolds, 2004; Rhoades, 2004), we can refer to a related strand of the previous literature that has focused on retractions of journal articles. Though retractions can occur in response to scientific misconduct, they can also be due to honest mistakes (Azoulay et al., 2015a; Fang et al., 2012; Van Noorden, 2011). Despite the fact that a common cause of retraction is the honest reporting of a mistake by an author, retractions have been shown to have negative implications for the citations of the author's prior and future work (Azoulay et al., 2015a,b ; Jin et al., 2013; Lu et al., 2013), articles conceptually related to the research in question (Lu et al., 2013), and the narrowly defined research field as a whole (Azoulay et al., 2015a).

In this paper we show that the damage of scientific misconduct reaches further, affecting collaborators who previously worked with a scientist later found guilty of scientific misconduct, but who were not involved in the misconduct case. Our empirical analysis shows that prior collaborators face a citation penalty of 8–9% in the aftermath of a scientific misconduct case. We base this result on misconduct cases investigated by the U.S. Office of Research Integrity (ORI) between 1993 and 2008, which oversees misconduct investigations for research funded by the National Institute of Health (NIH) and the Public Health Service (PHS). Hence, our database is based on the complete list of investigated and well-documented scientific misconduct cases at the world's largest funder of medical research.

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[1] Cf. the report on Mr. Stapel's fraudulent research (Levelt Committee et al., 2012): the work of many of the doctoral dissertations overseen by him needed to be reassessed since they were based on fraudulent data.

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This observation fits a theory of stigma spreading through mere association (Pontikes et al., 2010), according to which scientists are punished by the scientific community for being associated with a scientist known to have engaged in misconduct – and hence deviant from the norms. Crucially, they are punished for associating with scientists who only afterwards are revealed to have engaged in misconduct, and did not choose to associate with a known fraudulent scientist. The effect we observe can be interpreted as an extended loss of trust in fellow scientists in the wake of misconduct. Trust plays an important role in science, considering its cumulative nature (Merton, 1973) and the lack of incentives and resources to replicate results (Hamermesh, 2007). While a loss of trust in the work of misconducting scientists can be seen as rational behaviour and as a means of self-regulation for the scientific community (Azoulay et al., 2015b), it turns into wasteful ignorance of public knowledge if it spills over to others.

To the best of our knowledge, there is no solution to this problem. Transparency about guilt, a presumably obvious measure, apparently is a necessary but not sufficient precondition as the U.S. ORI already publishes detailed information about the investigated cases, including the names of the scientists found guilty. This should help the community to distinguish fraudulent scientists from innocent bystanders, yet we do not find innocent bystanders emerging entirely unscathed from a misconduct scandal.

One important implication of our findings is that incentives for scientists to blow the whistle on unethical behaviour on the part of their collaborators are very low. Scientists are unlikely to draw attention to foul play if they can expect to be considered complicit by association. Such behaviour hinders the self-correction mechanism that science relies on. This implies that it is even more important to prevent misconduct at the bench, where it can be corrected without affecting the careers of others.

We contribute to the previous literature in several ways. First, by drawing on the theory of social stigmatization (Goffman, 1963; Pontikes et al., 2010), we provide a general explanation for previously documented negative implications of scientific misconduct and retractions on closely related parties (Mongeon and Larivière, 2015) and the afflicted research field (Azoulay et al., 2015a). These articles employ three different mechanisms to describe their findings. The first is Bayesian updating of beliefs (Azoulay et al., 2015b, Jin et al., 2013), which predicts that after retractions come to light, the scientists involved are considered to be of lower 'quality' than before. Second, a loss of trust may lead scientists to avoid citing work associated with misconduct – even indirectly – in order to protect their integrity (Fuchs and Westervelt, 1996). Lastly, the actual or feared invalidation of part of the scientific field by a retraction may incentivize scientists to refocus on different research topics (Azoulay et al., 2015a). The concept of stigmatization allows us to connect these different mechanisms. Caused by a *loss of trust*, social stigmatization can be the reason for a *Bayesian updating of beliefs* about the "quality" of prior collaborators and the means by which the members of the scientific community *protect their own scientific integrity*.

Second, our study focuses only on investigated cases of scientific misconduct in the field of medical research. Unlike most of the previous studies that are based on retractions (Azoulay et al., 2015a,b; Jin et al., 2013; Lu et al., 2013; Mongeon and Larivière, 2015), we base our findings on an analysis of the Findings of Research Misconduct published by the ORI, the central U.S. authority that is responsible for investigating scientific misconduct cases in biomedical research funded by the NIH throughout the country, between 1993 and 2008. This data constitutes a complete list of cases of scientific misconduct committed by NIH grant recipients and grant recipients at the PHS investigated by the ORI. We thus avoid the considerable uncertainty inherent to retraction-based analyses and instead base our analysis on confirmed cases of scientific misconduct (Azoulay et al., 2015a; Fang et al., 2012; Van Noorden, 2011). In doing so, we expand the literature analysing misconduct investigated by the ORI (Lubalin and Matheson, 1999; Pozzi and David, 2007; Redman and Merz, 2008; Resnik and Dinse, 2012; Reynolds, 2004; Rhoades, 2004; Wright et al., 2008).

Third, we contribute to the understanding of the consequences of misconduct on science by empirically showing that the implications of scientific misconduct reach beyond the scientists involved in the project and the fraudulent scientists' institutions. Specifically, our analysis shows that misconduct scandals affect prior collaborators. Hence, the potential fallout of a misconduct scandal is much wider than one might assume.

The remainder of the paper is organized as follows. In the next section, we develop our theoretical framework. Section three provides an overview of the institutional framework in the U.S. Section four details our methodology, while section five presents the data along with descriptive statistics. The results are presented in section six. Section seven concludes.

2. Conceptual framework

This section describes the key features of the scientific system that help our understanding of the incentives for scientific misconduct. The second part of this section draws on sociological research in order to introduce the concept of social stigmatization and apply it to the context of science and misconduct.

2.1. Science and misconduct

Trust is an important pillar of the scientific system (Dasgupta and David, 1994). This is due to distinctive features of the system itself, including the way in which scientists compete, the cumulative nature of science (Merton, 1973), and the freedom that scientists enjoy when choosing their projects (Stern, 2004; Aghion et al., 2008).

Scientists compete in a race for priority, advancing their reputation when they are the first to publish new discoveries in scientific journals. The rewards for publishing include access to further resources, prestigious jobs offers, contacts to peers in science, as well as other lucrative opportunities both within and outside of the academic sector (Merton, 1973). The race for priority is a winner-takes-all game, with the second to finish getting (almost) nothing, even though (his or) her investment might have been large. This structure is beneficial for society as it creates incentives for scientists to disclose their discoveries as soon as possible, thus allowing follow-up research to be conducted in a speedy manner and avoiding the duplication of research streams (Merton, 1957; Dasgupta and David, 1994; Stephan, 2012). The nature of scientific competition hence advances scientific progress. The absence of formal intellectual property rights in the usual scientific process further accelerates the dissemination and adaption of scientific results. The fact that third parties can freely access, exploit and modify a published idea is well in line with the scientists' pursuit of maximizing their reputation (Fleming and Sorenson, 2004; Murray and O'Mahony, 2007)

The downside of the winner-takes-all competition in science is that it creates incentives to cheat. As in sports, a someone might find it beneficial to use forbidden means in order to increase their chances of winning the race. Classic game theory predicts that it can be a rational strategy to cheat in these settings, at least when the chances of being caught are relatively low (Nalebuff and Stiglitz, 1983). This seems to be the case in science because incentives for replication and other forms of double checking published results are low (Dewald et al., 1986; Hamermesh, 2007). Kiri et al. (2015) as well as Lacetera and Zirulia (2011) investigate the peer review system in a game theoretical setting. The predictions of their models include a certain positive equilibrium level of misconduct, and the worrisome observation that more stringent verification procedures do not necessarily lead to fewer cases of misconduct.[2]

In order to establish the importance of trust in science, two more features of the system of scientific research need to be understood. The first one is the well-known cumulative nature of science (Merton, 1973). Science evolves along specific lines of research where scientists use the prior insights of their colleagues as a foundation and stepping stones for their own research (Azoulay et al., 2015a; Mokyr, 2002). The second distinct feature is that science also differs from alternative knowledge production systems insofar as scientists are able to enjoy substantial discretion in choosing their research projects (Aghion et al., 2008; Stern, 2004). As it is not possible to personally verify all prior research results related to a planned project, science depends to a great degree on trust. It is thus only rational that scientists shy away from the results of authors that have been involved in retractions in the past (Azoulay et al., 2015a,b 2014a; Lu et al., 2013; Mongeon and Larivière, 2015).

In line with this, it has been observed that scientists, eager to protect their reputation and wanting to avoid wasteful investments, avoid starting research projects in research areas characterized by a larger number of retractions (Azoulay et al., 2015a; Lu et al., 2013). Closely related to our study, Azoulay et al. (2015a) investigate the impact of retractions on cumulative science. They show that articles in related strands of literature receive fewer citations after a retraction occurs, with a stronger decline when the retraction is due to misconduct. Furthermore, scientists tend to avoid research fields in which retractions occur. While Azoulay et al. (2015a) are interested in the cumulativeness of research, employing retractions as a disruption to a research line, we are interested in the implications of scientific misconduct on prior collaborators of the fraudulent scientist.

2.2. Stigma and mere association

The fact that retractions spread beyond the scientists involved in the actual publication has been explained in the prior literature by rational Bayesian updating of beliefs (Azoulay et al., 2015b; Jin et al., 2013), and the fear of embarking on projects standing on "shaky shoulders" (Azoulay et al., 2015a). Since we are interested in the effect of misconduct on prior collaborators – who are more removed from the misconduct, both in time and in the sense that they are not directly involved in the fraud and their work is not necessarily thematically related to the fraudulent work – we draw on sociology where the notion that bystanders suffer from being unjustifiably associated with others' actions is well-established, and is expressed by the concept of stigmatization (Goffman, 1963).

A stigma is defined as a perceived undesirable, deeply discrediting attribute that disqualifies an individual from full social acceptance (Goffman, 1963). Attributes evoking a stigma can evolve from the attributes of an individual's social identity which include physical appearance, professional activities and the concept of self (Goffman, 1963). Stigmatization can hence be described generally as a flexible process involving labelling, stereotyping, separation, loss of status and discrimination (Link and Phelan, 2001). Put into the context of the scientific community, individuals who commit scientific misconduct are likely to be stigmatized because they disrespect the norms of the scientific community. They are labelled as cheaters, stereotyped as being generally untrustworthy, seen as deviating from the social norm of the scientist, and are rejected by the scientific community who ignore their findings and exclude them from common necessities such as scientific funding.

Stigma can spread by virtue of association; an individual can be stigmatized purely because of their association with something or someone else, rather than because of any of their own discrediting attributes (Goldstein and Johnson, 1997; Mehta and Farina, 1988). 'Stigma by association' (Goldstein and Johnson, 1997) or 'courtesy stigma' (Goffman, 1963) or 'associative stigma' (Mehta and Farina, 1988) has been well studied in laboratory settings (e.g. Goldstein and Johnson, 1997; Mehta and Farina, 1988; Neuberg et al., 1994) and has been applied to a variety of different sources for stigmatization such as mental illness (e.g. Lefley, 1987), homosexuality (e.g. Swim et al., 1999), and criminal behaviour (e.g. Levenson and Tewksbury, 2009). It has also been observed in corporate settings where firm-specific scandals can have industry-wide implications (Barnett and Hoffman, 2008; Barnett and King, 2008; Kostova and Zaheer, 1999; Smith et al., 2010) and executives associated with firm wrongdoing may experience wage losses (Groysberg et al., 2016).

Stigma transfers quickly. A single juxtaposition with a stigmatized individual can be enough to create further stereotyping (Risen et al., 2007). Stigma by association occurs for myriad reasons[3] and is persistent. At its core, stigmatization by association is found to exist because of the assumption that individuals choose to associate with the stigmatized individual, thus justifying stereotyping of the associated individual (Pontikes et al., 2010).

Essential to our setting is that stigmatization by association can occur ex post – an individual is stereotyped because he chooses to associate with the stigmatized group – or ex ante – an individual experiences stigmatization because of association with a group before it became stigmatized. The latter case is called stigma by mere association (Pontikes et al., 2010). Pontikes et al. (2010) show the existence of stigmatization by mere association among Hollywood actors during the Red Scare where being merely associated with a blacklisted individual significantly reduced actors' chances of being cast in feature films. The authors conclude that "*stigma by association can lead to false positives and harm many innocents*" (p. 456).

Our setting is one of mere association. Scientists do not expect their collaborators to engage in illicit behaviour. Yet, when a scandal surfaces they see part of their oeuvre tainted by the misconduct stigma. In the worst case, they are not complicit in producing a piece of false

[2] Lacetera and Zirulia (2011) consider four policy levers through which misconduct might be discouraged: facilitating replication studies, easing the "publish or perish" paradigm, increasing the punishment for being caught, and involving journal editorials boards in checking for fraud. Although commmon sense dictates that increasing either should result in less fraud, they find that none of these policies is able to eliminate fraudulent behaviour and that some may in fact lead to more cases of fraud. In their model, a reduction of costs for checking for fraud leads to a change in the degree of novelty of research that is conducted. Alleviating pressure to publish leads to more rather than less fraud. Higher punishment for fraud can deter an author from cheating, but might also reduce incentives to check for fraud. A more active role for the editorial board could crowd out incentives for the reader to check the published results. In a follow up paper, Kiri et al. (2015) focus explicitly on the interplay between investigating efforts in high-quality research and efforts spend on validating research results produced by others. More validation leads to a higher research quality level, and low-quality research can be eliminated if the incentives for verification and confirmatory research are high enough. However, a large number of scientists performing validation activities may reduce the overall level of validation activities as well as the overall quality level of research as the rewards need to be shared among many. Moreover, in research areas with a high level of collaboration, scientists may collude and not validate each others research findings if they have doubts. The current setting of investigated misconduct cases at the ORI and other agencies of the PHS contains elements from both studies. First, punishments are heavy, as they commonly include exclusion from important and prestigious NIH and PHS funding. As Lacetera and Zirulia (2011) showed, incentives to commit fraud should be lower in this context than in other settings, but it might also mean that incentives to verify results are lower. The difference in the misconduct rate between this and other contexts is therefore unclear. Second, the number of cases of scientific misconduct published by the ORI is small compared to the volume of retractions in medical sciences. It is, however, unclear to what extent these discrepancies are driven by low levels of validation efforts, or by other factors. Third, our study takes place in the context of biomedical research where teams are large and scientists collaborate with a wide range of people (cf. Section 5). This might indicate a higher likelihood of collusion to not validate results compared to other settings where coauthor teams and collaboration networks are smaller.

[3] An extreme example of arbitrary stigmatization is found in a study reporting interviewers' lower expectations of job candidates that sit next to an obese female (Hebl and Mannix, 2003).

science but find themselves stereotyped as frauds. Empirical evidence supports this idea, showing that co-authors of retracted studies see their citation rates drop (Jin et al., 2013; Mongeon and Larivière, 2015). In our case, the scientists of interest were not directly involved in false science. However, they can still be stigmatized because of stereotyping. The (original) misconducting author is classified as a fraud (even though evidence might exist for only one case of misconduct), which makes all of his collaborators, past and present associated with fraudulent activity by mere association. This forms the main hypothesis of the present paper: mere association with scientific misconduct through prior collaboration, leads innocent scientists to be caught up in misconduct scandals.

This raises the question of whether such behaviour can be seen as rational. In our view, the answer is yes. As scientists need to resort to heuristics, including reputation, to assess the quality of the work of their colleagues (Merton, 1968, p. 59), association to authors found guilty of misconduct can be interpreted as a negative quality indication. Rational herding behaviour (Banerjee, 1992) may add to this. If scientists see their peers shy away from a researcher's prior work, they may believe that these colleagues have more information regarding the integrity of the researcher in question and his or her involvement in the misconduct case.

The next question is whether such behaviour is welfare enhancing or destroying. Ignoring research results is welfare enhancing if they have been shown to be invalid or "shaky" (Azoulay et al., 2015a). Ignoring valid research results, however, is welfare destroying. We believe that disregarding the research of prior collaborators of a fraudulent scientist is to a large extent welfare destroying, as they are not directly connected to fraudulent research, only via indirect association through prior collaboration and incidental overlaps in topic. Neither of these, however, bear any significance for the quality of the ignored research.

3. Institutional context

The empirical analysis is based on Findings of Research Misconduct published by the Office of Research Integrity (ORI) in the U.S. Since 1993, the ORI has been responsible for overseeing and directing activities concerning matters of research integrity within the Public Health Services. This includes intramural and extramural scientific misconduct in research funded by the National Institutes of Health (NIH) and other agencies of the PHS, but does not include activities concerning regulatory research integrity by the Food and Drug Administration.

When the ORI receives allegations of scientific misconduct, either directly or through funding institutions, the ORI decides whether or not to launch an inquiry into the allegation. Note that institutions can also pursue an inquiry independently. If sufficient grounds are found to continue, the inquiry becomes an ORI investigation. If evidence of scientific misconduct is found, the ORI publishes the results in its Findings of Research Misconduct, as well as in its annual report.[4] These reports describe the misconduct cases transparently and in great detail, recording the persons involved, where and when the reported misconduct took place, the nature of the misconduct, any publications that are affected by it, and any administrative sanctions taken such as institutional oversight, exclusion from referee or advisory boards, exclusion from government contracting, and/or exclusion from further NIH or PHS funding. This level of transparency makes it possible to identify scientists found guilty of scientific misconduct.[5] The ORI uses the following definition of research misconduct: "*Research misconduct means fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. Fabrication is making up data or results and recording or reporting them. Falsification is manipulating research materials, equipment, or processes, or changing or omitting data or results such that the research is not accurately represented in the research record. Plagiarism is the appropriation of another person's ideas, processes, results or words without giving appropriate credit. Research misconduct does not include honest error or differences of opinion*" (Office of Research Integrity, 2011, p.9).

There are previous studies focusing on the ORI cases of scientific misconduct (Lubalin and Matheson, 1999; Pozzi and David, 2007; Redman and Merz, 2008; Resnik and Dinse, 2012; Reynolds, 2004; Rhoades, 2004; Wright et al., 2008). Earlier studies (Rhoades, 2004; Reynolds, 2004; Pozzi and David, 2007) provide detailed descriptive evidence on the ORI misconduct cases, looking into the different types of accusations and outcomes, sources of funding, trends over time, etc. Resnik and Dinse (2012) investigate the correlation between information provided by the retraction note from the journal and the outcome of the ORI investigation. Redman and Merz (2008) consider the career consequences of being found guilty of misconduct by the ORI and find that being found guilty is associated with severe drops in publication rates (with approximately a third of fraudulent scientists dropping out of publishing completely), and a high incidence of the guilty party leaving university employment. Lubalin and Matheson (1999) survey whistle-blowers and individuals accused of having been involved in scientific misconduct but who were exonerated and investigate the long-run and short-run professional and personal consequences for both groups. They find that in the short-run, during the period of investigation, whistleblowers fare worse than those accused of misconduct. While both groups report limited long-term impacts, those accused of misconduct report worse consequences in terms of mental and physical health as well as self-esteem and self-identity. Lastly, Wright et al. (2008) point out that the majority of misconduct cases are associated with a lack of oversight from mentors. In more than half of the investigated cases, stress was reported as a main or contributing factor to misconduct.

4. Method

We aim at investigating how scientific misconduct affects the reputation of prior collaborators, as proxied through the accumulation of citations. Citations to publications are a widely used indicator for the importance of scientists and their scientific findings, reflecting the extent to which results and insights are used as building blocks for future research. They have been shown to correlate with other measures of a scientist's influence such as awards, honours and Nobel laureateships (Cole and Cole, 1967; Inhaber and Przednowek, 2007; Myers, 1970) as well as with peer judgements (Aksnes and Taxt, 2004). Keeping the quality of the scientist and other factors constant, a drop in citations reflects a loss of trust in the scientists' work by the scientific community. We start the analysis with a descriptive exercise to detect a change in the time trend of the citations received by an innocent prior collaborator. This shows whether a drop in citations occurs around the point in time when the misconduct case was published. Since such a dip in the trend could be driven by time-varying macro effects, we use a control group of comparable scientists that were not associated with a misconduct case. These scientists serve as a benchmark, a control group, and allow us to abstract from influences other than the publication of the misconduct case that could impact the number of citations that the publications of a researcher who previously collaborated with a misconducting scientist (the treatment group) receive.

To evaluate whether there is a causal effect of the publication of a misconduct case on the citations to prior collaborators, we employ a difference-in-difference method that allows us to compare the evolution of citations to prior collaborators of misconducting scientists relative to citations to the control group of scientists. We control for the length of the scientists' careers and unobservable ability in a multivariate setting.

[4] See http://ori.hhs.gov/historical-background. The first ORI annual report was published in 1994.

[5] A more detailed description of the ORI's course of action in misconduct investigations and statistics on previous investigations can be found in Rhoades (2004).

More specifically, we estimate an equation of the form:

$$\text{Citations}_{it} = \psi_1 T_t + \psi_2 Pt_i + \psi_3 T_i{}^*Pt_t + \beta\Gamma_i + \varphi_t + \xi_i + \ln(Publications_{it}) + \varepsilon_{it}$$

Citations_{it} represents the total number of citations accumulated by articles published by author *i* in year *t* at the time the data were collected. In the remainder of the analysis, we refer to this measure as 'aggregate citations'.[6] As citation counts follow a count distribution, we estimate a Poisson model. We offset aggregate citations by publication output by including the natural logarithm of the number of publications issued by the author in the year, with the coefficient constrained to one to account for differences in publication output.

T_t and Pt_i represent the core of the model. The first variable is an indicator variable that takes the value one if the author is in the treatment group, and zero otherwise. The second variable takes value zero in pre-treatment years, and one after treatment. The estimated coefficients of these variables, ψ_1 and ψ_2, thus capture any systematic differences in aggregate citations between the treatment and control group, and between any shared differences pre- and post-treatment. The main result of the model is provided by ψ_3, which captures the average difference in change of aggregate citations between control and treatment observations after the misconduct was discovered. If scientists in the treatment group experience a drop in aggregate citation after being associated with scientific misconduct, while scientists in the control group do not, ψ_3 takes a negative and significant coefficient.

Γ_i represents a vector of individual-specific factors that affect citations. One such factor is career age, as measured by the time since the authors' first publication in a scientific journal. This accounts for the fact that authors have changing levels of commitment to publishing as their career progresses (e.g. Stephan and Levin, 1992). We include career age in linear and squared terms to account for the possible life-time effect of scientific productivity of researchers.

Another factor is talent, or ability. As this is usually impossible for econometricians to observe directly, we control for inherent differences in citation rates either through unobserved (fixed) effects models or by applying a pre-sample average estimator (Blundell et al., 1999).[7] Finally, φ_t captures common time trends through a set of year dummies, and ξ_i captures that misconduct can have a heterogeneous impact through a set of case dummies.

5. Data

5.1. Data sources and construction of the database

The analysis is based on the *Findings of Research Misconduct* as published by the Office of Research Integrity (ORI). The *Findings* are concerned with misconduct cases of National Institute of Health (NIH) grant recipients and grant recipients at the Public Health Service (PHS). The analysis makes use of the 36 cases published between 1993 and 2008 in which at least one scientific publication with a Pubmed[8] identifier was affected. We focus on misconduct cases involving retractions or corrections only[9] in order to restrict the analysis to cases of misconduct with real scientific impact.

We should note that research funded by the NIH or PHS is not representative of all research performed in biomedicine: given the eminence associated with being NIH-funded, we expect misconduct cases investigated by the ORI to be more high-profile than the average misconduct case. This is corroborated by the fact that these misconduct cases involve significant sanctions, such as barring researchers temporarily or permanently from NIH funding, contracting with the PHS, or subjecting them to institutional oversight (Redman and Merz, 2008). Given that the NIH accounts for a substantial share of biomedical research funding in the U.S. – 28% in 2008 (Dorsey et al., 2010) – and that grant recipients constitute top researchers, it is a relevant setting to study nevertheless.

We retrieved all of the misconducting author's publications and citations from the Scopus publication database in late 2014. Through these publications we identified prior collaborators of the misconducting authors in the five years prior to the publication of the case in the *Findings*. Throughout the analysis, we took articles, conference papers, notes, reviews, and short surveys into account. Letters, books, and other document types were not taken into consideration.

Our dependent variable is the aggregate number of citations that the articles published in the focal year receive over the years until late 2014 when the data was collected. As we only consider misconduct cases up to 2008, this leaves reasonable time for citations to accumulate, even three years after the *Findings* have been published. In the robustness check section, we also use the annual citation flow as an alternative dependent variable. This data is available from Scopus from 1996 onwards only. The results are reported in the robustness checks section.

Authors who also collaborated on work mentioned in the *Findings* were excluded from the set of collaborators to ensure that the effects found only stem from association, and not direct implication. We also removed all articles co-authored with the fraudulent scientist, as citations to these articles could be affected by negative attention directed to the author found guilty of misconduct. Furthermore, we removed authors who had co-authored papers with more than one scientist found guilty of misconduct, as these are subject to multiple treatments. After data cleaning we arrived at 929 unique co-authors who had published jointly with the scientists later found guilty of misconduct in the five years prior to the publication of the *Findings*. Our final sample is somewhat smaller, consisting of 856 treated co-authors as we needed to observe publication outputs at least four years before the misconduct was published by ORI in order to be able to apply a pre-sample correction for unobserved ability.

5.2. Construction of control group

To construct a control group we needed to identify prior collaborators comparable to the treated prior collaborators, i.e. those that collaborated with a fraudulent NIH or PSH grant recipient in the past. We achieved this by selecting prior collaborators of a scientist funded by a similar grant from the same agency as the scientist guilty of misconduct. Selection based on having received a grant is essential since we observe a positive selection when focusing on scientists that receive grants. To do this, we used the NIH grant database made available by Pierre Azoulay, which is referred to as Grant2pmid database.[10] This database contains information on NIH grants from 1971 onwards, and lists grant numbers, general information about the grant, and any publications (indexed by Pubmed identification numbers) which list the grant as a source of funding.

The control group was built as follows. We selected a NIH grant

[6] The data were collected in late 2014. As we only consider misconduct cases up to 2008, this leaves reasonable time for citations to accumulate, even three years after the *Findings* have been published. We control for any remaining time-driven variation in citations through a set of publication year dummies. We additionally show the robustness of our results to employing a citation flow indicator, which represents the number of citations an author received in a year regardless of the year in which the cited article was published.

[7] The pre-sample mean estimator suggested by Blundell et al. (1999) accounts for unaccounted heterogeneity due to unobservables by including an additional parameter in the model which contains the pre-sample average outcome. This restrains our models to prior collaborators whose publication outputs has been observed for at least four years at the time the *Findings* were published.

[8] Pubmed is a search engine for the bibliometric database MEDLINE (Medical Literature Ananlysis and Retrieval System Online). This database contains more than 5500 biomedical journals.

[9] Others include faking of credentials or affiliations, and submitting fraudulent applications, often occuring in the application phase for an NIH grant.

[10] http://pazoulay.scripts.mit.edu/Data.html.

Table 1
Summary of Data Gathering Protocol.

1. For each misconduct case, select a NIH grant acknowledging funding of one of the papers listed as affected in the *Findings*
   - Pick the grant on the earliest paper listed in the *Findings* which acknowledges funding
   - Pick the earliest grant in case the paper acknowledges more than one NIH grant
2. Match these 'treatment' NIH grants to NIH 'control' grants with similar characteristics to the treatment grants, considering granting institute, grant type, grant duration, and grant year as matching parameters.
3. For each control grant, select one publication funded by the grant
   - In cases where more than one paper was supported by the control grant, the publication published as close as possible to the date of the treatment publication was chosen.
4. Select a random author of these publications to form the control group of non-fraudulent authors
5. Collect the collaborators of the control authors in the five years before the relevant *Findings* were published.

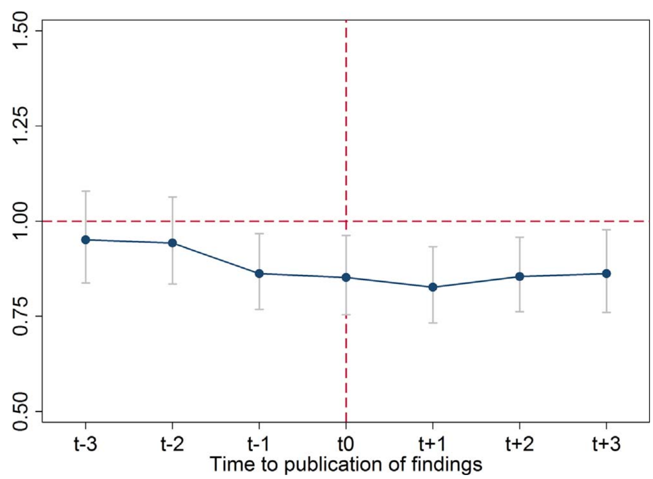

Fig. 1. Average aggregate citations to prior collaborators' publications relative to the control group.
Notes: This figure represents a plot of coefficients stemming from a Poisson model where prior collaborator's aggregate citations are regressed onto year and case effects, an exposure term capturing publication output in the year, and 7 interaction terms which represent the treatment effect for each year before or after the *Findings* were published. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case.

which was listed as a source of funding in the corrected or retracted publication mentioned in the *Findings* ('treatment grant'). We then matched this treatment grant to a control grant (using the NIH grant database) with similar characteristics in terms of medical research area, type of grant, grant year, and grant duration ('control grant'). We matched on research area by selecting grants issued by the same NIH institute. The National Institutes of Health consist of 27 institutes and

Table 3
Mean comparison of treated and control samples.

	Treated		Control		Diff
	Mean	St. Dev.	Mean	St. Dev.	
Publications	4.75	7.08	4.11	5.23	***
Aggregate citations	274.31	473.09	288.37	506.48	.
Years since first publication	16.62	9.48	17.18	9.48	***
Pre-sample aggregate citations per publication average	41.92	65.17	50.78	59.46	***
After treatment indicator	0.56	0.50	0.54	0.50	**
Authors	856		1149		
Observations	5769		7437		

Notes: Sample restricted to author-years with at least one publication to match baseline estimation sample. Diff: two-sample *t*-test. Stars indicate significance level of difference: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

centers, which are divided thematically. Thus, grants issued by the same center should be in comparable fields. Examples of institutes are the National Cancer Institute (NCI), the National Human Genome Research Institute (NHGRI), and the National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK). Selecting grants of a similar type ensures that the grant recipients are in comparable career positions. While the database does not include information on the size of the grant, we proxy for this through the duration of the grant.

We then selected a random author of a random publication supported by the control grant to serve as a match for the grant recipient found guilty of misconduct and used Scopus to identify this author's prior collaborators in the five years before the *Findings* associated with the corrected or retracted publication were published. This results in a final control group of 1149 co-authors. The process is summarized in Table 1.

6. Results

6.1. Visual inspection

To start with a visual description of the effect of the publication of the *Findings* on prior collaborators compared to control authors, we estimate the evolution of the aggregate citations over time. To do so, we estimate a Poisson model on the author-year level, interacting the treatment indicator with each year relative to the publication of the *Findings* while controlling for calendar year and case effects. We further control for the number of articles published that year. The resulting coefficients of the interaction terms thus represent the difference between treatment and control collaborators over time.

Fig. 1 plots the effects and 95% confidence intervals. Three and two years before the misconduct is revealed, there is no significant difference in aggregate citations between the articles published by treatment and control authors, confirming that the control authors are similar to the treatment authors in that time period. In the year before the

Table 2
Summary statistics (2005 prior collaborators, 13,206 observations).

	Summary Statistics					Correlation Matrix					
	Mean	St. Dev	Min	Median	Max	1	2	3	4	5	6
1. Publications	4.39	6.12	0	2	121	1					
2. Aggregate Citations	282.23	492.21	0	91	4257	0.69	1				
3. Years since first publication	16.94	9.43	1	16	56	0.22	0.17	1			
4. Pre-sample aggregate citations per publication average	46.91	62.17	0	30.71	1046.5	0.03	0.15	−0.13	1		
5. Treatment dummy	0.44	0.50	0	0	1	0.05	−0.01	−0.03	−0.07	1	
6. After treatment indicator	0.55	0.50	0	1	1	0.02	0.01	0.18	−0.01	0.02	1

Notes: 42 prior collaborators that did not publish between $t-3$ and $t+3$ have been dropped from the sample used for the descriptive statistics. Aggregate citations: total number of citations received by papers published in year *t* at time of data extraction.

Table 4
Poisson regression estimates of aggregate citations of prior collaborators of authors guilty of misconduct and control group.

Dependent: Prior collaborator's Aggregate citations Model	(1) Baseline	(2) Offset by Publications	(3) Incl. controls	(4) Relax offset	(5) Lead treatment	(6) Drop t-1 and t0	(7) QML F.E.
Treatment group	0.13	−0.09*	−0.03	−0.04	0.005	0.01	
	(0.08)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	
After-treatment period	0.08*	0.08**	0.10***	0.10***	0.04	0.02	0.08**
	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.08)	(0.04)
Treatment group*	−0.12**	−0.08**	−0.09**	−0.09**	−0.12***	−0.13***	−0.09**
After-treatment period	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Years since first publication			0.02***	0.02***	0.02***	0.02***	
			(0.01)	(0.01)	(0.01)	(0.01)	
Years since first publication^2/100			−0.03***	−0.03**	−0.03***	−0.04***	
			(0.01)	(0.01)	(0.01)	(0.01)	
Pre-sample aggregate citations per publication average/10			0.03***	0.03***	0.03***	0.03***	
			(0.003)	(0.003)	(0.003)	(0.003)	
Ln(number of publications)				1.02***			0.91***
				(0.02)			(0.02)
Intercept	4.761***	3.95***	3.71***	3.70***	3.70***	3.75***	
	(9.96)	(0.37)	(0.37)	(0.37)	(0.37)	(0.39)	
Year effects	YES	YES	YES	YES	YES	YES	YES
Case effects	YES	YES	YES	YES	YES	YES	NO
Individual fixed effect	NO	NO	NO	NO	NO	NO	YES
Number of observations	13,206	10,756	10,756	10,756	10,756	7526	10,679

Notes: Poisson estimation of aggregate citations received by articles published in year. Models 2,3,5,6: Estimates offset by publications in year by including ln(number of publications) with coefficient fixed at 1. Model 4 and 7: ln (number of publications) included as control without restriction. Observations without publications not included in columns 2–7. Years since first publication^2 and pre-sample aggregate citations per publication scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

misconduct is revealed, aggregate citations drop to 86% compared to those of control authors, and hover around this proportion until $t + 3$.

The fact that the aggregate citations start decreasing in $t - 1$, i.e. before the scientific misconduct case is published, is likely explained by the presence of rumours of misconduct during the investigation: we take the end of the investigation, the publication of the *Findings*, as treatment, but damage to the scientists' reputation could occur much earlier. The ORI's investigation takes on average approximately half a year to complete, and rumours of potential misconduct could spread throughout the scientific community even before allegations reach the ORI (Rhoades, 2004). The dip in $t - 1$ is likely to be further deepened by the fact that we plot aggregate citations and not the flow of citations in *t*: articles published just before the misconduct case came to light could still be affected, as a large part of their citations would only emerge after the *Findings* have been published.

6.2. Descriptive statistics

Table 2 provides summary statistics and correlations. The average prior collaborator of a grant recipient in the sample publishes 4.39 papers per year, which received 282.23 aggregate citations. The average prior collaborator enters the sample relatively late in (his or) her career, 16.94 years after (his or) her first publication. It should be underscored here that the sample under observation does not form a representative sample of biomedical researchers, but rather of researchers that collaborate with recipients of prestigious NIH grants.

Table 3 compares the means for the treatment and control groups for the key variables. Treated prior collaborators publish slightly more than those in the control group (treated: 4.75, control 4.11, difference significant at $p < 0.01$), but the control scientists are cited equally often (treatment: 274 aggregate citations per year, control: 288, difference not significant at $p < 0.1$). In terms of control variables, treated authors' careers are approximately half a year younger than control authors (treatment: 16.62 years since first publication, control: 17.18, significant at $p < 0.01$) and tend to have lower measures of pre-sample citations on average per publication (treatment: 41.92, control: 50.78, significant at $p < 0.01$), which are defined as the average aggregate citations per publication the author received in each observed year before t-3.

6.3. Estimation results

Table 4 presents the results of the Poisson regression of prior collaborator's aggregate citations on the treatment effects and controls. In all specifications, we find that the aggregate citations per publication by treated prior collaborators drop compared to prior collaborators in the control group in the years after the *Findings* were published. The point estimates of all specifications are significant at least $p < 0.05$, and show an average drop between 8% and 9% in the main specifications and up to 12% in the alternate specifications.

The first column shows the model without taking into account any observed or unobserved scientist-specific controls, only controlling for case and common year effects. The former is justified by the idea that cases of misconduct bring about highly heterogeneous amounts of (media) attention in the scientific community and vary greatly in severity. Therefore, the effect on the careers of prior collaborators is also likely to be partly case-specific. The results show that treated prior collaborators' aggregate citations do not differ significantly from control collaborators. We also find a positive trend for both groups, which is captured by the dummy indicating the after-treatment. The treatment effect of being a treated prior collaborator (as inferred from the interaction between being in the treated group and the after-treatment indicator) is negative and highly significant, with a marginal effect of 11.3% at the mean.[11] In other words, prior collaborators suffer a loss of 11.3% in aggregate citations after details of a misconduct scandal are made public.

Model 1, however, does not control for publication volume. Therefore, we offset citation counts by publication counts in model 2.[12] This controls for the volume of scientific output and allows us to interpret the coefficients as a change in the citation rate. Offsetting changes the interpretation of the results: whereas treated prior collaborators did not gather significantly more or less citations than control

[11] Calculated as the difference in expected incidence rate ratio at the mean, i.e. 1-exp (-0.12).

[12] In practice, we include the natural logarithm of the number of publications in year *t* as an explanatory variable with its coefficient fixed at one. This thus means that we have to condition on the subsample of author-years that include at least one publication, which is the case for 80% of the sample.

prior collaborators, they now gather fewer citations per publication. Offsetting also lowers the treatment effect: the drop of 11.3% in citations at the mean translates to a drop of 7.7% in the citation to publication rate.

In model 3, we enrich the offset baseline model by controlling for the career age of the scientist through the time since first publication in linear and quadratic form. We additionally include a proxy for unobserved ability in the form of the pre-sample average aggregate citations per publications. We find an inverted-U shaped relation between career age and citation counts, and a positive and significant relationship between pre-sample citations per publications and current citation outputs. Including these factors slightly increases the treatment effect from 7.7% to 8.6%.

The results in columns 2 and 3 are based on the assumption that there is a one-to-one relationship between citations and publications. That is, the model assumes that the coefficient of the natural logarithm of publications is one. In model 4, we relax this assumption. We find that the estimated treatment effect is the same as before, and the coefficient of the publication offset is highly significant ($p < 0.01$) and estimated at 1.02, not statistically significantly different from the assumed coefficient of 1 ($\chi^2(1) = 0.96, p = 0.3284$).

Fig. 1 showed an initial drop in aggregate citations among prior collaborators in the treatment group, as compared to prior collaborators in the control group, at t_{-1}. We speculate that this might be the result of inaccuracy of the treatment effect, i.e. while we are certain that the misconduct has been published in the *Findings*, the misconduct could also have been made public through other channels before then. We test whether this concern explains our findings through model 5, where we move the treatment one year forward, to t_{-1}. While the results are qualitatively the same, the treatment effect is estimated slightly higher, at 11.3%, in this specification. As an alternative robustness test, we provide results when disregarding years t_{-1} and t_0 in column 6. We thus compare the aggregate citations three and two years before the findings of misconduct to one to three years after. This shows an even higher estimate of the treatment effect at 12.2%.

Lastly, we present in column 7 the results of a fixed effects Poisson estimation with robust standard errors as described in Wooldridge (1999). Using fixed effects instead of pre-sample estimators does not yield different conclusions. As in model 2 and 3, the treatment effect is estimated at 8.6%.

6.4. Further analysis and robustness checks

6.4.1. Effect on publication output

Table 5 presents estimation results for Poisson regression models using the volume of scientific output as the dependent variable. The models presented in columns 1–3 do not show a statistically or economically significant drop in the number of papers published by the prior collaborators after the *Findings* were published. This strengthens the interpretation of the observed drop in citations being the result of stigmatization −perceived lower quality or reliability of published science, and not of effort exercised by the authors, in which case a drop in publications should also be observed.

6.4.2. Effect on fraudulent authors

Table 6 shows the effects on the fraudulent authors themselves. The baseline regression (column 1) and the model including controls (column 2) show significant negative treatment effects of respectively 48% and 39%. Previous studies focusing on retractions which include scientific misconduct cases along with retractions due to honest mistakes identified a 'career effect' to the order of 10% (Azoulay et al., 2015b). In comparison, we are analysing a dataset of investigated and declared misconduct cases of highly eminent researchers. The higher visibility of this selected set of misconduct cases likely leads to a steeper decline in citations post publications of the *Findings*. The results of the QML Fixed Effects Poisson specification (column 3) show a statistically

Table 5
Poisson regression estimates of publications by prior collaborators of authors guilty of misconduct.

Dependent: Prior collaborator's publication count Model	(1) Baseline	(2) Incl. Controls	(3) QML FE
Treatment group	0.27***	0.18***	
	(0.07)	(0.05)	
After-treatment period	−0.02	0.003	−0.0005
	(0.03)	(0.03)	(0.02)
Treatment group* After-treatment period	−0.03	−0.02	−0.03
	(0.03)	(0.03)	(0.03)
Years since first publication		0.07***	
		(0.01)	
Years since first publication^2/100		−0.15***	
		(0.02)	
Pre-sample publication average		0.15***	
		(0.01)	
Intercept	0.80***	0.07	
	(0.14)	(0.18)	
Year effects	YES	YES	YES
Case effects	YES	YES	NO
Individual fixed effect	NO	NO	YES
Number of observations	13,206	13,206	13,204

Notes: Poisson estimation of yearly publication counts of prior collaborators. Years since first publication^2: coefficient scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

insignificant effect, which is however still strongly negative at a drop of 26%. The lack of statistical significance in the Fixed Effect model, compared to the significant effect estimated in the same model for prior collaborators, is likely due to by the much smaller estimation sample.

Columns 4–6 show estimates of publication output by authors who have engaged in misconduct. In line with previous findings (Redman and Merz, 2008), authors found guilty of scientific misconduct by the ORI have a high chance of dropping out of publishing, with 21% of authors not publishing in the three years after treatment, compared to 5% in the three years before. This is a stark increase compared to the control group, which only shows 3% non-publishing authors in the after-treatment period. This pattern is mirrored in the regression estimates, which report drops in publication output by fraudulent authors between 36 and 45% (columns 4–6). However, this effect is only statistically significant in the fixed effects specification where we control for author-specific unobserved effects, indicating a strong degree of remaining unobserved heterogeneity.

6.4.3. Effect heterogeneity: author reputation and scandal size

Finally, we explore two sources of potential heterogeneity in the effects documented above. We consider the standing of the prior collaborators before the misconduct case surfaces and the scope of the scandal. One might expect that scientists with a higher standing are more affected by stigmatization and that a scandal with a broader scope would have more significant negative effects. On the other hand, previous work has shown that the most eminent authors seem able to largely avoid the fallout associated with retractions, whereas less eminent authors suffer disproportionally (Jin et al., 2013)

Starting with the standing of scientists, we estimated differential effects by the citation stock of either the misconducting author or the prior collaborator, based on their location in the citation stock distribution of the sample in the year before treatment. Citations proxy the standing or the visibility of the scientist within the academic community. The results are shown in Fig. 2, for heterogeneity along the misconducting author's pre-treatment citation stock, and in Fig. 3 for that of prior collaborators. The regression output can be found in Table A1 in Appendix A. As Fig. 2 shows, the negative effects shown above are driven by the bottom half of the misconducting authors' citation distribution. For the third and fourth quartile, the point estimates turn

Table 6
Poisson regression estimates of citations rates and publication output of authors guilty of misconduct.

Dependent	(1) Misconducting author's aggregate citations	(2)	(3)	(4) Misconducting author's publications	(5)	(6)
Model	Baseline	Incl. controls	QML FE	Baseline	Incl. controls	QML FE
Treatment group	−0.02	−0.01		−0.61**	−0.47*	
	(0.19)	(0.26)		(0.25)	(0.28)	
After-treatment period	−0.22**	−0.04	−0.08	0.24	0.11	−0.11
	(0.11)	(0.12)	(0.13)	(0.18)	(0.11)	(0.11)
Treatment group* After-treatment period	−0.65***	−0.49**	−0.30	−0.47	−0.44	−0.59**
	(0.23)	(0.25)	(0.19)	(0.33)	(0.32)	(0.27)
Years since first publication		−0.02			−0.02	
		(0.04)			(0.04)	
Years since first publication^2/100		0.03			0.02	
		(0.06)			(0.08)	
Pre-sample aggregate citations per publication average/10		0.12***				
		(0.04)				
Pre-sample publications average					0.27***	
					(0.04)	
Intercept	3.59***	3.63***		2.29***	0.73	
	(0.07)	(0.54)		(0.64)	(0.53)	
Year effects	YES	YES	YES	YES	YES	YES
Case effects	YES	YES	NO	YES	YES	NO
Individual fixed effect	NO	NO	YES	NO	NO	YES
Number of observations	255	255	253	331	331	331

Notes: Poisson estimation of yearly aggregate citation counts of authors found guilty of misconduct (columns 1–3) and annual citation output (columns 4–6). Aggregate citation count estimates offset by publications in year by including ln(number of publications) with coefficient fixed at 1 (QML F.E: not fixed). Prior author-years without publications not included in citation estimates sample. Years since first publication^2 and pre-sample aggregate citations per publication average scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

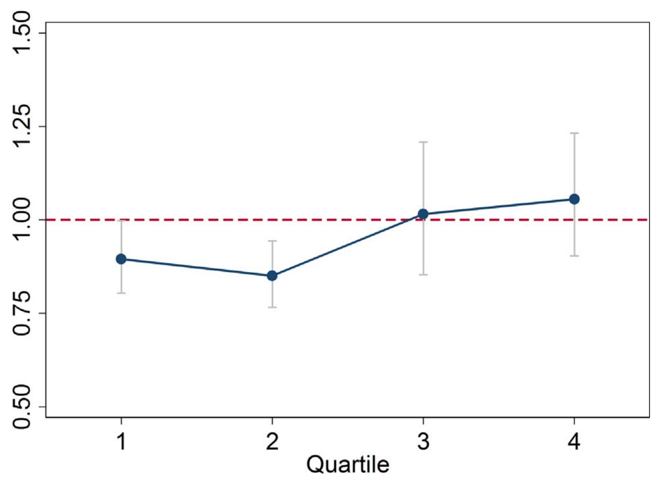

Fig. 2. Treatment effect on prior collaborator by pre-treatment citation quartile of author guilty of misconduct.
Notes: This figure represents a plot of coefficients stemming from a Poisson model where prior collaborator's aggregate citations are regressed onto year and case effects, years since first publication in linear and squared terms, pre-sample aggregate citations per publication average, and an exposure term capturing publication output in the year. The plot shows the treatment effect interacted with 4 quartile indicators of the fraudulent author's pre-treatment citation stock. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case. Full regression output is presented in Table A1 in Appendix A.

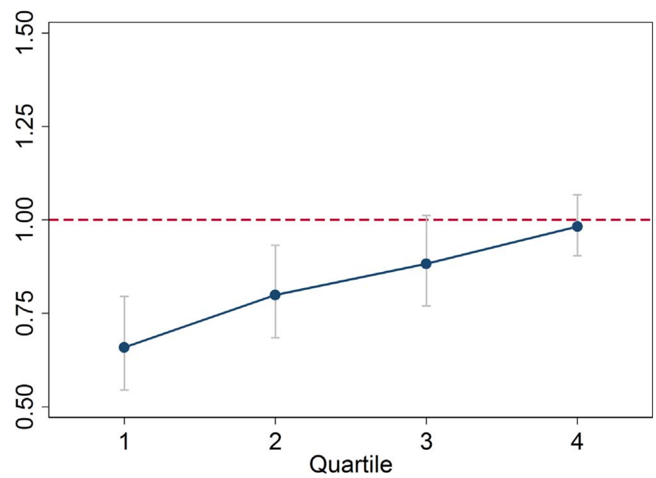

Fig. 3. Treatment effect on prior collaborator by pre-treatment citation quartile of prior collaborator.
Notes: This figure represents a plot of coefficient estimates stemming from Poisson model where prior collaborator's aggregate citations is regressed onto year and case effects, years since first publication in linear and squared terms, pre-sample aggregate citations per publication average, and an exposure term capturing publication output in the year. The plot shows the treatment effect interacted with 4 quartile indicators of prior collaborator's pre-treatment citation stock. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case. Full regression output is presented in Table A1 in Appendix A.

positive but statistically nonsignificant at $p < 0.10$. In other words, stigmatization of prior collaborators seems to occur mainly when the fraudulent author has a (comparatively) less established reputation himself.

Fig. 3 plots the results when differentiating by the prior citation stock of the prior collaborator. Here too, the results show that the effects are strongest for authors in the lower quartile, who experience a 34% drop in citations ($p < 0.01$). The second quartile is estimated at a drop of 20% ($p < 0.01$). For the third quartile the effect is estimated at 11% ($p < 0.10$), and the fourth quartile shows no significant drop at $p < 0.10$. These results confirm the “reverse Matthew effect” documented previously by Jin et al. (2013) for prior collaborators as well. The main effects thus seem to be strongly moderated by, on the one hand, the prior reputation of the author guilty of misconduct, and, on the other, the prior reputation of the potential victim of stigmatization. This is in line with the interpretation of the results presented here as social stigmatization, with those prior collaborators who lack a well-established reputation particularly suffering from the stereotyping inherent to stigmatization by mere association. The effects are at their worst when both are on the left tail of the distribution.

In a second set of estimations we compare the effects across the scope of the scandal. Ceteris paribus, it is reasonable to expect a larger misconduct scandal to have a stronger impact on the author's reputation. To conceptualize the scope of a scandal, we make use of the number of papers corrected or retracted, the number of citations received by papers which were corrected in the wake of the scandal, and the highest impact factor associated with a paper which was corrected or retracted in the wake of the scandal. One to ten papers were affected by the misconduct cases included in the study.[13]

Figs. 4–6 show the results of estimations differentiating between different scopes of the scandal. The full results are presented in Table A2 in Appendix A. Along all three measures, we cannot detect a substantial trend in effect size along the scope of the scandal. This result might be explained by the fact that our analysis is based on a positive selection in terms of scandal scope since all our cases have been formally investigated and found guilty by the ORI, which in all but a few cases in the sample led to disqualification from highly prestigious NIH grant funding. There might be a stronger link between scandal scope and consequences (for prior collaborators) among the broader range of misconduct cases outside of the ORI.

6.4.4. Robustness checks

6.4.4.1. Robustness check I: fraudulent authors leaving the scientific community as a driver of the citation drop. As a first robustness check, we explore the possibility that part of the drop in aggregate citations might be caused by the fact that authors found guilty of misconduct leave the scientific community (Redman and Merz, 2008). The results presented in the previous section indicated that fraudulent authors publish significantly less after their misconduct becomes public knowledge. Since the likelihood of citing a prior collaborator might be disproportionately high, the drop in fraudulent authors can be an alternative explanation for the patterns observed.

Whereas our dataset does not allow us to assess the origin of every individual citation, we perform a back-of-the-envelope calculation as a robustness check. We first estimate a publication trend for each author found guilty of misconduct, based on pre-treatment observations, as a proxy of the counterfactual publication pattern had the author not been caught committing scientific misconduct. We then make the most conservative assumption that all of these publications (observed in the pre-treatment and estimated counterfactual situation in the post-treatment period) cite all prior publications of their collaborators. We then subtract this maximum amount of potential citations that the collaborator could have received from the fraudulent scientist from the dependent variable and re-run our regressions.[14] If the citation drop were caused by the disgraced author leaving academia, our previously found treatment effect should disappear.

The results, shown in Table 7, however, show that the treatment effect is still significant and negative. This exercise should be seen as an upper limit estimate of the potential citation relation between fraudulent author and prior collaborator, it indicates that the effect is not driven by the author dropping out of scientific publishing after their misconduct is exposed.

6.4.4.2. Robustness check II: citation flows. We acknowledge the fact that a drop of aggregate citations does not necessarily coincide with a drop in the citation flow. Hence, we replicate the regressions for aggregate citations reported above for the citation flow in Table 8. We take a three-year fixed overlapping citation window (Rehn et al., 2014, p. 19) and offset for the volume of publications that might be cited through the number of articles published in the same time window. As Scopus only offers detailed information on citation flows from 1996 onwards (Rehn et al., 2014 p. 16) we restrict the estimation sample to this period. We do not include a pre-sample mean estimator in these models as it imposes a particularly strong constraint on the sample, and instead focus on the QML F.E. Poisson model to control for unobserved heterogeneity. While the models without controls or offset (Table 8, column 1), with offset but without controls (column 2), or

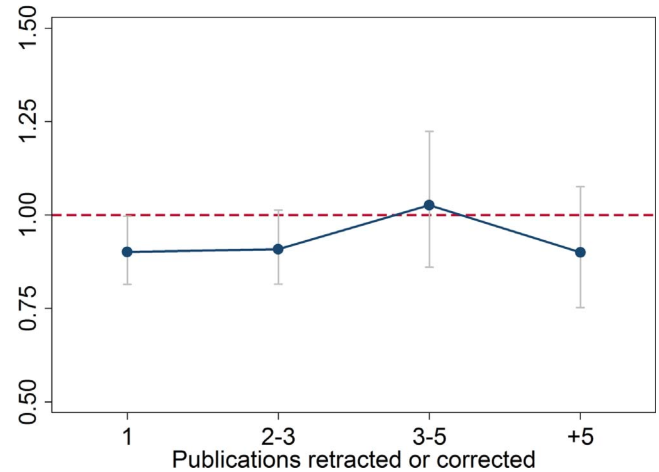

Fig. 4. Treatment effect on prior collaborator by size of scandal: number of articles affected.
Notes: This figure represents a plot of coefficients stemming from a Poisson model where prior collaborator's aggregate citations are regressed onto year and case effects, years since first publication in linear and squared terms, pre-sample aggregate citations per publication average, and an exposure term capturing publication output in the year. The plot shows the treatment effect interacted with 4 groups of the number of articles retracted or corrected in the wake of the scandal. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case. Full regression output is presented in Table A2 in Appendix A.

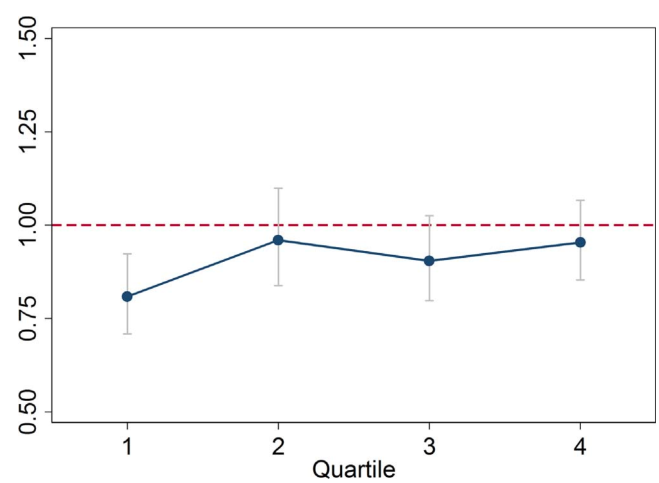

Fig. 5. Treatment effect on prior collaborator by size of scandal: total citation impact.
Notes: This figure represents a plot of coefficients stemming from a Poisson model where prior collaborator's aggregate citations are regressed onto year and case effects, years since first publication in linear and squared terms, pre-sample aggregate citations per publication average, and an exposure term capturing publication output in the year. The plot shows the treatment effect interacted with 4 quartile indicators of the importance of the affected work, as measured through the total number of citations received by the work. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case. Full regression output is presented in Table A2 in Appendix A.

[13] The median case involved the retraction or correction of 2 articles. Approximately 75% of the articles affected were retracted, the others corrected. The results presented in this section are similar when differentiating between retracted and corrected articles.

[14] Due to the fact that we use estimated citations for the counterfactual situation it is possible that the aggregate citations minus the estimated value may turn negative. In this case we set them to 0.

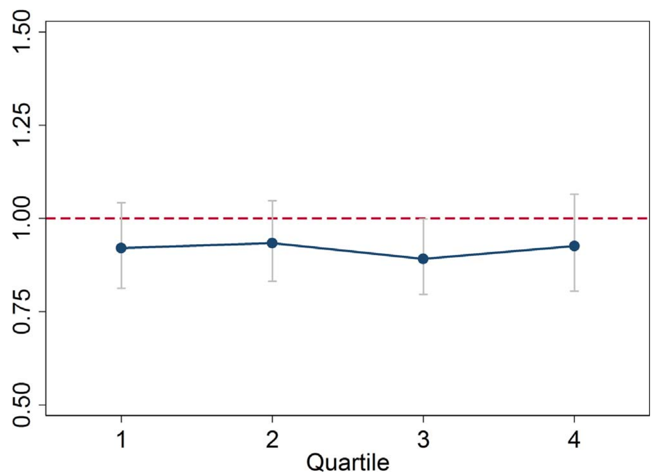

Fig. 6. Treatment effect on prior collaborator by size of scandal: highest impact factor.
Notes: This figure represents a plot of coefficient estimates stemming from a Poisson model where prior collaborator's aggregate citations are regressed onto year and case effects, years since first publication in linear and squared terms, pre-sample aggregate citations per publication average, and an exposure term capturing publication output in the year. The plot shows the treatment effect interacted with 4 quartile indicators of importance of the affected work, as measured through the highest impact factor among the articles retracted or corrected in the wake of the misconduct scandal. Coefficients are exponentiated and represent Incidence Rate Ratios. Confidence bands represent 95% confidence intervals, calculated based on robust standard errors clustered by misconduct case. Full regression output is presented in Table A2 in Appendix A.

Table 7
Robustness checks: excluding citations by authors found guilty of misconduct.

Dependent: Prior collaborator's aggregate citations	(1)	(2)	(3)
Model	Baseline	Incl. Controls	QML FE
Treatment group	0.33***	0.39***	
	(0.07)	(0.07)	
After-treatment period	0.24***	0.25***	0.23***
	(0.05)	(0.05)	(0.05)
Treatment group* After-treatment period	−0.24***	−0.25***	−0.27***
	(0.05)	(0.05)	(0.05)
Years since first publication		0.02**	
		(0.01)	
Years since first publication^2/100		−0.03*	
		(0.02)	
Pre-sample citations per publication average/10		0.03***	
		(0.004)	
Intercept	3.14***	2.88***	0.88***
	(0.60)	(0.61)	(0.03)
Year effects	YES	YES	YES
Case effects	YES	YES	NO
Individual fixed effect	NO	NO	YES
Number of observations	10,756	10,756	10,144

Notes: Poisson estimation of yearly aggregate citation counts, excluding citations by authors found guilty of misconduct. Citations by misconducting authors have been estimated post-treatment by extrapolating pre-treatment trend and assuming that every publication by the fraudulent author cites entire body of prior collaborator's work at that time. Aggregate citation count estimates offset by publications in that year by including ln (number of publications) with coefficient fixed at 1. Prior collaborator-years without publications not included in sample. Years since first publication^2 and pre-sample citations per publication scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

with offset and controls (column 3) show little effect, the results from a QML F.E. Poisson regression shows an estimated drop of 9% in citation flow after publication of the *Findings* (column 4), which is statistically significant at $p < 0.05$. The difference between the QML F.E. specification and the others highlights the importance of controlling for unobserved individual-specific effects in this specification.

7. Conclusions and discussion

The fact that we can detect the impact of misconduct on prior collaborators suggests that scientific misconduct affects many more individuals than previously imagined. By documenting the negative consequences of scientific misconduct for innocent prior collaborators, our results show that the implications of scientific misconduct reach beyond the fraudulent researcher, suggesting that articles by prior collaborators less often form part of the basis for future research. This is an unwarranted and wasteful disregard of valid research findings which slows down scientific progress and represents a cost to society. These findings are consistent with the theory of stigmatization through mere association (Goffman, 1963; Goldstein and Johnson, 1997; Mehta and Farina, 1988; Pontikes et al., 2010).

Why would stigmatization through mere association happen in the scientific setting? The answer, in our opinion, lies in the reliance on trust in the scientific enterprise. While misconduct should in principle be detected during the peer review process or through scrutiny by readers, it is prohibitively costly − in terms of money and time – to verify some results to a sufficient degree, and scientists need to assume a certain degree of honesty of their peers (Dewald et al., 1986). At the same time, scientific research is becoming ever more complex and multidisciplinary, meaning scientists need to rely more on the expertise of their collaborators (Jones, 2009; Wuchty et al., 2007). Not in the least, researchers do not have strong incentives to perform replication studies, as they receive relatively little attention and are unlikely to make an impact (Hamermesh, 2007). Yet these studies are important. One recent replication study reported that of 100 replication attempts in psychology, only 39 were successful (Baker, 2015). Another study found that 47 out of 53 'landmark' studies in cancer research could not be replicated (Begley and Ellis, 2012). Given that scientists need to assume the honesty of their colleagues and are hard pressed for testing it, it should come as no surprise that they fall back on heuristics and stereotyping for appraising the reliability of their colleagues.

Whereas some evidence exists that those accused but not found guilty of misconduct can at least mend their reputation through exoneration (Greitemeyer and Sagioglou, 2015), there seems to be no clear course of action for dealing with the indirect reputational effects documented here. The problem becomes even clearer when one notes that in this setup, misconducting scientists were clearly named, and the nature of their misdoings was transparently reported. Other, less transparent contexts might lead to even worse effects for prior collaborators.

Since means to counteract stigma at the institutional level are limited, we turn to options that the stigmatized collaborator themselves might have. Psychiatric research discusses three ways of responding to stigma[15]: protest, education, and contact (Corrigan and Watson, 2002). Evidence suggests those who know more about the "disease" – in our context the association with misconduct – tend to stigmatize less than others (Corrigan and Penn, 1999). Contact with stigmatized persons also reduces stigmatization (Kolodziej and Johnson, 1996). In the present context, this means that prior collaborators of colleagues found guilty of scientific misconduct should actively take part in the scientific community. Attempting to suppress the stigma can however also worsen stigmatization (Monteith et al., 1998).

Given these findings, we believe that it is important to create awareness within the scientific community about guilt by association. Stigmatized scientists may for instance proactively use scientific conferences as a forum to discuss their case with the scientific community. This is in line with the psychiatric recommendation to "transform the person from a patient to an advocate" (e.g. Byrne, 2000). Such efforts could also be supported by scientific journals by providing a forum for

[15] This research is mostly concerned with stigmatization associated with mental disease.

Table 8
Robustness checks: Citation flow.

Dependent: Prior collaborator's citation flow	(1)	(2)	(3)	(4)
Model	Baseline	Offset	Controls	QML F.E.
Treatment group	0.09	−0.06	−0.06	
	(0.10)	(0.07)	(0.07)	
After-treatment period	−0.04	0.03	0.03	0.04
	(0.05)	(0.03)	(0.03)	(0.03)
Treatment group*	0.02	−0.02	−0.01	−0.09**
After-treatment period	(0.07)	(0.05)	(0.05)	(0.04)
Years since first publication			0.01	
			(0.01)	
Years since first publication^2/100			−0.02	
			(0.02)	
Ln(Publication stock)				1.00***
				(0.04)
Intercept	3.74***	1.24***	1.12***	
	(0.59)	(0.31)	(0.32)	
Year effects	YES	YES	YES	YES
Case effects	YES	YES	YES	NO
Individual fixed effect	NO	NO	NO	YES
Number of observations	10,235	9731	9731	9479

Notes: Poisson estimate of prior collaborator's yearly citation flow. Citation flow calculated in a three year citation window. Publication Stock: number of articles published in same window. Model 2–3: estimates offset by ln(Publication Stock) with coefficient fixed at 1. Model 4: ln(Publication Stock) included as control without restriction. Model 2–4: author-years with 0 publication stock not included. Years since first publication^2 scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

innocent scientists with indirect ties to fraudulent colleagues. In the case of stigma related to mental disease at least, education campaigns have proven to be successful (Wolff et al., 1996).

One alarming implication of our findings is that scientists might refrain from blowing the whistle on their colleagues if they expect a backlash through association. Such behaviour would stand in sharp contrast to the self-correction function that science relies on. A survey or 68 whistle-blowers at the ORI (Lubalin and Matheson, 1999) indicates that a majority of whistle-blowers experiences negative outcomes from their whistleblowing, affecting their career advancement, professional activities, mental health and personal life. This raises the question of whether self-correction is sufficient as a mechanism to ensure the validity of research results, or whether in times of increased pressure on scientists to publish paired with unpreceded levels of complexity and multidisciplinarity in research different quality control mechanisms are needed. Some have proposed that stigmatization might serve a function as a deterring factor for future misconduct. However, the effectiveness of stigma for deterrence has been debated in the broader criminal setting (Funk, 2004; Harel and Klement, 2007; Rasmusen, 1996), with some evidence indicating that, while inducing deterrence effects for non-offenders, stigmatization increases the crime rate for offenders (Funk, 2004). In our setting, stigmatization might make future misconduct more attractive by reducing its costs for the already stigmatized innocent collaborators. In this sense, modelling the effect of stigmatization and stigma by association and scientific misconduct, might make for an interesting avenue for future theoretical research.

Our study makes several contributions to the small but developing literature on the consequences of scientific misconduct. First of all, we contribute to prior studies providing possible explanations for citation penalties for scientists associated with fraudulent scientists. Mechanisms proposed previously include the Bayesian updating of beliefs (Azoulay et al., 2015b; Jin et al., 2013), a loss of trust (Lu et al., 2013) and the desire of scientists to protect their own scientific integrity (Azoulay et al., 2015a). We introduce the concept of social stigmatization which we borrowed from sociology as an overarching concept to explain the three mechanisms. Caused by a *loss of trust*, social stigmatization can be the reason for an *Bayesian updating of beliefs* about the "quality" of prior collaborators and the way for the members of the scientific community to *protect its own scientific integrity*.

Second, we show that the ripple effect of scientific misconduct reaches further than the fraudulent author's close collaborators or colleagues. Given the significance of the citation penalty that these distantly related scientists experience and the increasing trend of collaboration in science, our results show that the costs of scientific misconduct have thus far been underestimated. This seems to be especially the case for scholars who have not yet developed a strong reputation for themselves, as these individuals seem to be hit particularly hard by stigmatization through mere association.

Third, we present the first study to analyze a clean sample of scientific misconduct cases in this context. Prior studies often focus on retractions which are readily available from publication databases such as the Web of Science or Scopus and include scientific misconduct cases along with retractions due to honest mistakes (Azoulay et al., 2012; Azoulay et al. 2015a,b). The difference in magnitude of the effects on authors guilty of misconduct (Section 6.4.2) that we present as compared to prior studies should remind us of the important difference between the two.

From a more general perspective, our results are related to research into 'superstar extinction' in that it confirms that shocks to co-authorship networks seem to ripple out (Azoulay et al., 2014, 2010). Our results can also be interpreted as an extreme case of negative citations, which have been shown to precede a future decline in citations (Catalini et al., 2015).

Our study is not free of limitations. The downside of using a clean sample of misconduct cases for quantitative analysis is the smaller number of cases available for analysis. In our context, where we are interested in the effects of misconduct on prior collaborators, however, this is of minor importance because of the large number of collaborators NIH grant recipients have. A more important point to stress is that the NIH grant recipients are a positive selection and might not have a lot in common with average scientists. This implies that our results represent upper limit estimates and should be treated as such. An interesting avenue for follow-up studies could be to employ network methods for a network of co-authors in order to investigate how far the effects of scientific misconduct reach.

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Appendix A

Table A1
Poisson regression estimates of citations rates of fraudulent authors interacted with prior citation stock of fraudulent author or prior collaborator.

Dependent: Prior collaborators' aggregate citations Model	(1) By prior citation stock of misconducting authors	(2) By prior citation stock of collaborator
Treatment group	−0.02 (0.05)	−0.03 (0.05)
After-treatment period	0.09** (0.04)	0.10*** (0.04)
Treatment group* After-treatment period* quartile 1	−0.11** (0.06)	−0.42*** (0.10)
Treatment group * After-treatment period* quartile 2	−0.16*** (0.05)	−0.22*** (0.08)
Treatment group * After-treatment period* quartile 3	0.02 (0.09)	−0.12* (0.07)
Treatment group * After-treatment period* quartile 4	0.05 (0.08)	−0.02 (0.04)
Years since first publication	0.02*** (0.01)	0.02*** (0.01)
Years since first publication^2/100	−0.03** (0.01)	−0.03** (0.01)
Pre-sample citations per publication/10	0.03*** (0.00)	0.03*** (0.00)
Intercept	3.71*** (0.37)	3.76*** (0.36)
Year effects	YES	YES
Case effects	YES	YES
Individual fixed effect	NO	NO
Number of observations	10,756	10,756

Notes: Poisson estimation of prior collaborator's aggregate citations. Aggregate citation count in year offset by publications in year by including ln(number of publications) with coefficient fixed at 1. Quartile indicates quartile of citation stock at t-1 of author guilty of misconduct and prior collaborator respectively. Author-years without publications not included in citation estimates sample. Years since first publication^2 and pre-sample citations per publication scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Table A2
Poisson regression estimates of citations rates of fraudulent authors interacted with scope of scandal.

Dependent: Prior Collaborator's aggregate citations Model	(1) Number of publications affected	(2) Citation-weighed affected publications	(3) Highest IF of affected papers
Treatment group	−0.03 (0.05)	−0.03 (0.05)	−0.03 (0.05)
After-treatment period	0.10*** (0.04)	0.10*** (0.04)	0.10*** (0.04)
Treatment group* After-treatment period* quartile 1	−0.10** (0.05)	−0.21*** (0.07)	−0.08 (0.06)
Treatment group * After-treatment period* quartile 2	−0.10* (0.06)	−0.04 (0.07)	−0.07 (0.06)
Treatment group *	0.03	−0.10	−0.11**

(continued on next page)

Table A2 (*continued*)

Dependent: Prior Collaborator's aggregate citations Model	(1) Number of publications affected	(2) Citation-weighed affected publications	(3) Highest IF of affected papers
After-treatment period* quartile 3	(0.09)	(0.06)	(0.06)
Treatment group *	−0.11	−0.05	−0.08
After-treatment period* quartile 4	(0.09)	(0.06)	(0.07)
Years since first publication	0.02***	0.02***	0.02***
	(0.01)	(0.01)	(0.01)
Years since first publication^2/100	−0.03***	−0.03**	−0.03**
	(0.01)	(0.01)	(0.01)
Pre-sample citations per publication average/10	0.03***	0.03***	0.03***
	(0.00)	(0.00)	(0.00)
Intercept	3.67***	3.69***	3.72***
	(0.35)	(0.36)	(0.37)
Year effects	YES	YES	YES
Case effects	YES	YES	YES
Individual fixed effect	NO	NO	NO
Number of observations	10,756	10,756	10,756

Notes: Poisson estimation of prior collaborator's aggregate citations. Aggregate citation count in year offset by publication counts by including ln(number of publications) with coefficient fixed at one in the model. Column 1: quartile represents number of publications corrected or retracted in the misconduct case (1: 1 publication affected or corrected, 2: 2–3, 3: 3–5, 4: 5+). Column 2: quartile represents quartile of citation-weighted corrected or retracted publications in the misconduct case. Column 3: quartile represents quartile of highest impact factor among publications corrected or retracted in the context of the misconduct case. Author-years without publications not included. Years since first publication^2 and pre-sample citations per publication scaled down for readability. Cluster-robust standard errors in parentheses. Stars indicate significance level of coefficient: *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

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