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Essays on the Benefits and Costs of Associating with a Fraudster

A Dissertation

Presented to

The Academic Faculty

by

Agnes Andor

In Partial Fulfilment

of the Requirements for the Degree of

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Essays on the Benefits and Costs of Associating with a Fraudster

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Abstract

This dissertation aims to provide a comprehensive understanding of the consequences to fraud. Its primary purpose is to estimate the costs and benefits that accrue to the collaborators of a fraudster over the entire lifecycle of a case of fraud. First, the current state of the literature examining the consequences to fraud is examined. This literature review highlights the fact that the current understanding of the consequences to fraud is not comprehensive, as it is heavily focused on the downside consequences, while largely leaving the upside consequences unexamined. The dissertation examines a single case of academic fraud. A unique dataset is built, which contains the performance data for the entire set of authors who collaborated with the fraudulent academic throughout the course of his career, as well as the entire set of authors who they collaborated with, but who themselves never worked with the focal fraudster. This allows for the construction of a matched panel dataset of collaborators, whose performance is tracked over time, both prior to and following the detection of the fraud. Several different matching strategies are utilized in order to construct groups of control authors, against whom the performance of the group of treated authors is compared. This allows both the pre- and post-detection effects of the fraud to be isolated, and ultimately, for a net effect to be measured. In addition, several individual-level characteristics are examined to determine whether there exist differences in the extent to which the costs and benefits to fraud accrue to different co-authors.

Across four samples, the study finds evidence of both a pre-discovery benefit and a post-discovery cost to the fraudster's co-authors. Ultimately, co-authors accrue a net performance benefit as a result of their collaboration with the fraudster. However, this benefit does not accrue to co-authors equally; rather, it is moderated by the gender and tenure of a collaborator, as well as by the intensity of their relationship with the fraudster. Ultimately, male co-authors, who begin to

collaborate with the fraudster early in their careers are found to stand to gain the most from the collaboration.

Through its empirical results, this dissertation advances theory in the fraud, gender, and status literatures in several ways. First, it extends the fraud literature, which has primarily focused on the downside consequences to fraud, by providing empirical evidence of its upside consequences. This leads to insights into potential motivating factors to both the decision to collaborate with a fraudster, and to remain silent when faced with evidence of fraudulent behavior. Second, it sheds further light on the differential perceptions of men's and women's contributions in mixed-gender teams, particularly in the academic context. Finally, it adds to the literature on the effects of status, as it provides evidence of a moderating effect of status on the degree to which co-authors accrue benefits when collaborating with a fraudster.

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I. INTRODUCTION

The question of whether crime pays has had a compelling life since Becker first published his rational choice theory of crime half a century ago (Becker 1968). Despite broad appeal and wide influence, empirical tests of whether crime pays are almost entirely absent from the literature. What exists is a preponderance of evidence for the costs of crime. This almost exclusive focus on the downside consequences of crime is likely driven by the fact that the upside consequences are difficult to study due to their hidden nature, as well as by a normative approach toward deterrence. We argue that the present state of the literature leads to a biased, one-sided view of the consequences of fraud. This paper aims to remedy the current, biased state by investigating both the upside and downside consequences to fraud. Our approach involves the thorough examination of the consequences of a single case of fraud over its entire lifecycle. This approach allows us to quantify both the pre- and post-discovery effects of fraud, as they accrue to the entire network of individuals who collaborated with a fraudster over the course of his career. The novel contribution of this paper is that it quantifies the net effect of fraud, including both its costs and benefits, to the entire set of a fraudster's collaborators.

We argue that it is important to examine the benefits to fraud in addition to its costs for several reasons. First, the current focus on the downside consequences keeps us from fully understanding the nature of fraud, and the resulting state of affairs; in fact, it provides us with a biased view of the outcomes of fraud. Second, without knowing its benefits, we cannot truly understand the motivation behind the decision to commit fraud. Finally, the current one-sided focus potentially misrepresents the effects fraud has on the fraudster's network of collaborators, as it provides us with a great deal of knowledge about the victims of fraud, but very little about its beneficiaries.

The primary goal of this study is to determine whether fraud pays. Anecdotal evidence from high-profile cases of fraud provides the intuition that it does. However, the existing body of literature examining fraud does not empirically test this intuition. One of the motivations behind the current study is purely metaphysical – in order to understand the concept of fraud, we must understand its consequences. The vast majority of our current understanding of the concept rests on the multitude of studies within the field of criminology, which examine the costs of fraud to the perpetrator (e.g. Karpoff 1993, Karpoff et al. 2008, Murphy et al. 2018), to the perpetrator's collaborators (e.g. Bonetta 2006, Mongeon and Lariviere 2013), to victims (e.g. Ganzini et al. 1990, Titus, Richard M., Heinzelmann Fred, Boyle 1995) and to society (e.g. Darby and Karni 1973). However, our understanding of the upside consequences is limited, as only a handful of studies quantify the benefits to fraud. A few examples include a study of economic crimes committed by investors, which found that their crimes can protect investors' social ties (Baker and Faulkner 2004), and a recent study of Chinese firms, which found that firms who choose to 'cook their books' are more likely to receive government funds than firms that engage in honest accounting practices (Stuart and Wang 2016). Our aim is to provide a more well-rounded understanding of fraud and its consequences by quantifying both its costs and benefits, and to ultimately determine its net effect. To the best of our knowledge, no previous studies have provided this type of comprehensive view of the consequences of fraud.

The majority of the literature examining fraud has had the ultimate normative goal of deterrence. We argue that, by focusing solely on its costs, we cannot accurately understand what motivates individuals to commit fraud. Half a century of work on the rational choice theory of crime provides extensive evidence of the notion that individuals weigh both the potential costs and the potential benefits of a criminal act, prior to making the decision to engage in that act (e.g.

Becker 1968, Bouffard 2007, Paternoster and Simpson 2016). The vast majority of existing studies focus on a single side of the cost-benefit equation. Quantifying the benefits to fraud can thus provide important insight into the other side of the cost-benefit calculus potential fraudsters engage in.

Finally, we argue that the current state of the literature leads to a biased understanding of the way fraud effects the individuals who collaborate with a fraudster. The costs incurred by these individuals seem to be driven by two different mechanisms. First, the fraudulent act itself can harm victims. Ponzi schemes, for instance, can cause immense financial losses to those who trusted their savings to a fraudster. Second, the costs facing the perpetrator of the fraud can spill over onto his or her collaborators. Once the perpetrator is caught, he or she typically faces immense reputational costs. Several studies have found that the costs facing perpetrators of fraud spill over to his or her network of collaborators (e.g. Janney and Gove 2017, Kang 2008). Do the benefits spill over as well? This question, to the best of our knowledge, has never been tested empirically.

I.1 Pre-Discovery Benefits

A wide array of anecdotal evidence from highly-publicized cases of fraud makes it impossible to dispute the fact that fraud can lead to great benefits, prior to its discovery. For instance, at the height of his crime, Charles Ponzi was bringing in $250,000 each day (Charles Ponzi, The Financial Idiot Who Drove Boston Money Mad in 1920), while Madoff's Ponzi scheme consistently delivered returns of 10-17% to his investors (Berenson 2008). Several studies examining the motivation behind the decision to commit criminal acts find that potential criminals engage in a cost-benefit analysis, in which they weigh both the potential costs and benefits of the act (e.g. Bouffard 2007, Hochstetler et al. 2007, Paternoster and Simpson 2016).

However, the literature quantifying the actual benefits resulting from criminal activity is even more limited. The few existing empirical studies find evidence of financial gains obtained by drug dealers and gang leaders (Levitt and Venkatesh 2000, McCarthy and Hagan 2011, Reuter et al. 1990), and white collar criminals committing fraud (Baker and Faulkner 2004, Stuart and Wang 2016).

The extremely limited nature of the existing literature quantifying the benefits to crime may be driven, at least in part, by its hidden nature. Our study tackles this difficulty by focusing on a single case of well-documented fraud in the context of academia. Given the collaborative nature of academia, the focal fraudster at the heart of our study worked with a wide network of collaborators throughout his academic career. The first purpose of our study is to measure the pre-discovery effects of the fraud on the performance of the entire set of the fraudster's collaborators. We examine these effects on a single, unique measure of academic performance – citation rates. We believe that citation rates are an accurate proxy for performance as they reflect the attitudes of the members of an academic's community. In fact, citations have been found to be a positive and significant determinant of academics' salaries (Diamond 2018). We predict that the fraudster's co-authors will experience a significant increase in their citation rates upon working with the fraudster, prior to the discovery of the fraud. Stated more formally, our first hypothesis is the following:

Hypothesis 1: Individuals who have co-authored at least one article with a fraudulent author will experience an increase in their citations upon starting to work with him or her, prior to the discovery of the fraud.

I.2 Post-Discovery Costs

A breadth of literature within the field of criminology has established the fact that those who commit fraud can incur severe costs upon its discovery. The case of Bernie Madoff clearly illustrates this phenomenon: Madoff is currently serving a 150-year sentence as a result of his crime. Fraudulent individuals and organizations can face severe reputational costs as well, which can ultimately prove fatal (e.g. Fich and Shivdasani 2007, Karpoff 1993). However, it is not only the direct perpetrators of fraud that incur costs –costs have been found to spill over onto collaborators as well. For instance, in cases of reporting fraud, firms associated with a fraudulent organization have been found to face severe reputational penalties (Kang 2008), firm linkages in cases of backdating scandals have been found to lead to costs for the associated firms (Janney and Gove 2017), and even competitors operating within the same industry as a firm facing a product recall have been found to incur performance costs (Zavyalova et al. 2012).

In the context of academia, misconduct often leads to the retraction of published work. When an article is retracted due to misconduct, not only do its citation rates drop, but so do the citations of the fraudulent author's previously published work (Azoulay et al. 2016, Lu et al.). The effect is 'immediate, severe, and long-lived: when controlling for age, year, and a fixed article citation effect, in the short term, citations decrease by 65%, by the 2nd year, by roughly 50%, and by the 10th year, by as much as 72% (J. Furman et al. 2012). This effect has been found to spread beyond the fraudulent author's work to other publications within the field in which a retraction occurs (Azoulay et al. 2015). Building on these findings, we propose that the retraction penalty will spill over onto the collaborators of a fraudulent author, and will cause a drop in their citation rates, compared to similar authors who did not collaborate with a fraudster. Stated more formally, our second hypothesis is the following:

Hypothesis 2: Individuals who have co-authored at least one article with a fraudulent author will experience a decline in their citation rates following the discovery of the fraud.

I.3 The Moderating Effects of Individual-Level Characteristics

While we estimate both the costs and benefits received by the entire population of a fraudster's collaborators, empirical evidence on the allocation of credit, status effects, and stigma transfer indicate that these costs and benefits will not accrue to all individuals equally. We explore how a collaborator's gender and career age, as well the intensity of their collaboration with the fraudster, moderate the costs and benefits they receive.

I.3.A Gender

It is risky for women to work in mixed-gender teams, as their efforts may go unrecognized when their team succeeds, or they may bear a large proportion of the blame in cases when their team fails. Under conditions of uncertainty, in which it is unclear how much each team member contributed to the team's ultimate failure, evaluators place more blame on female team members (Haynes and Lawrence 2012). However, when there exists a high degree of ambiguity regarding how individual contributions affect the joint *success* of a mixed-gender dyad, evaluators assume that women contributed less than their male teammates (Heilman and Haynes 2005). Women have even been found to assign themselves less credit than they assign to their male teammates, when they are uncertain about how much they have contributed to a successful joint work outcome (Haynes and Heilman 2013).

A recent stream of literature has found that women in academia are affected by the phenomena outlined above when co-authoring in mixed-gender teams. Women are less likely to receive tenure the more they co-author with men, while men are tenured at roughly the same rate regardless of whether they co-author or solo-author (Sarsons 2017). This is only the case in disciplines in which there is ambiguity regarding the amount of work each author put into the publication; in disciplines where co-authors are listed in order of contribution, men and women receive equal credit for co-authored work. However, women are much less likely to be listed in first- or last-authorship positions (West et al., 2013). Even when women are in the first-authorship position, more attention may be placed on the male co-author in the second position (Wolfers 2015). Additionally, publications on which women are listed as the first author are cited less than those in which men hold this coveted position (Sugimoto et al. 2013). As women are consistently given less credit when co-authoring with men, we hypothesize that women will incur both less of a pre-discovery benefit than their male colleagues when collaborating with a fraudster, as well as less of a post-discovery cost, as they are given less credit than the male fraudster, and other potential male collaborators on the publication.

Hypothesis 3a: Prior to the fraud's discovery, the decision to co-author with a fraudster will result in a less substantial benefit for female (compared to male) collaborators.

Hypothesis 3b: After the fraud's discovery, the decision to co-author with a fraudster will result in a more substantial cost for female (compared to male) collaborators.

I.3.B Career Age

Human capital - the educational, personal, and professional experiences an individual accumulates throughout his or her career (Becker, 1964) - is often used as a predictor of future career success (e.g. Judge et al. 1995). Several elements of human capital, including educational credentials, job experience, professional reputation, letters of recommendation, and references can all serve as signals of an individual's abilities (Bangerter et al. 2012). Within the context of academia, recognition and esteem accrue to individuals who make original contributions to a body of scientific knowledge (Merton, 1973). However, as early-career researchers typically have few publications, other elements of human capital must serve as signals of their future productivity. A recent study of early-career management scholars identified several key factors that predict a researcher's early-career success, including their advisor's research productivity, their own pre-appointment productivity, and the research output of both their graduate institution and the department in which they are hired (Williamson and Cable, 2003). We propose that collaboration decisions can also serve as information-rich signals of a researcher's future career performance. In addition, due to the limited number of early-career signals of future performance, we posit that each individual decision made early in one's career has more predictive power than each individual decision made during a later career stage. We hypothesize that these factors will cause those who decide to collaborate with a fraudster early in their career to experience both a greater pre-discovery benefit and post-discovery cost as compared to their more senior colleagues. The accumulated human capital of more senior researchers will weaken the signaling effect of their decision to collaborate with a fraudster, leaving them ultimately less effected by the collaboration.

Hypothesis 4a: A co-author's career age will be negatively related to the magnitude of the benefit he or she obtains prior to the discovery of the fraud.

Hypothesis 4b: A co-author's career age will be negatively related to the severity of the penalty he or she incurs upon the discovery of the fraud.

I.3.C Collaboration Intensity

The frequency with which two individuals interact can indicate the strength of the tie that exists between them (Granovetter 1973). Individuals that share strong ties are assumed to be similar to each other (e.g. Lee et al 2009), which can lead to both positive and negative associative effects. Being associated with a high-status individual can lead to heightened status (e.g. Cialdini 1989). However, sharing a strong tie with a stigmatized individual can cause the stigma to be transferred to the associated individual (e.g. Pryor et al. 2012). Being strongly associated with individuals who act in an unethical manner can cause observers to assume that one is more likely to act unethically himself (Coovert and Reeder 1990). Based on these findings, we predict that individuals who share a larger proportion of their publications with the fraudster, and are thus more strongly tied to him, experience both greater costs and benefits due to their association. Prior to the discovery of the fraud, the fraudster at the heart of our study had attained a high level of status. We predict that this high status transferred on to the individuals who collaborated with him more intensely. However, as the fraudster's status dropped dramatically once his misconduct was discovered, we predict that those who were strongly associated with him experienced courtesy stigma, while those who collaborated with him less intensely did not experience such strong costs.

Hypothesis 5a: The collaboration intensity between a co-author and the fraudster will be positively related to the magnitude of the co-author's pre-discovery benefit.

Hypothesis 5b: The collaboration intensity between a co-author and the fraudster will be positively related to the severity of the co-author's post-discovery penalty.

Our analysis focuses on the highly-publicized case of fraud committed by Diederik Stapel. In September 2011, Stapel, a well-known professor of social psychology and dean of Tilburg University's School of Social and Behavioral Sciences, was found guilty of research fraud. Later investigations found that close to half of the articles he had published throughout his highly successful career were based on fabricated data. These articles were retracted, and, ultimately, Stapel was relieved of his position, and returned his PhD title.

We focus on this particular case for several reasons. First, the academic context is ideal for the study of fraud, as public records regarding coauthored publications, and data on the performance of these publications over time are readily available. This availability of information allows us to obtain complete information regarding the entire set of individuals who worked with Stapel at any point in his career, as well the entire set of their work. Throughout his highly successful career, Stapel worked with 63 co-authors. His co-authors ranged from his 13 PhD students to a variety of assistant, associate, full, and emeritus professors. This completeness of information allows us to perform a comprehensive analysis of the effects of Stapel's fraud on the careers of his collaborators.

Second, due to the magnitude of the misconduct and the high level of publicity surrounding the scandal, the Stapel case was thoroughly investigated, and the results of these investigations were made public. The investigations presented a detailed history of the case and determined that Stapel alone was responsible for the misconduct (Levelt Committee et al. 2012). The investigations also reveal the fact that several of Stapel's co-authors ultimately uncovered the fraud and chose to go public with it. The identities of these whistleblowers remain unknown. For the purposes of this study, we remain agnostic about the level of knowledge Stapel's co-authors may have had in regard to his misconduct.

To ensure that our results are robust, we test each of our hypotheses using several different samples of treatment and control authors. We begin by comparing the pre- and post-discovery performance of the entire set of collaborators to all of the individuals they collaborated with throughout their careers, who themselves never worked with the focal fraudster. We then draw on this data to construct several different matched samples, in order to isolate the effect of collaboration, and exclude all potential differences in collaborators as potential drivers of the effects we find. Finally, we check the robustness of our results by utilizing a within-author matching strategy, in which we compare the pre-discovery performance of the collaborators to their pre-collaboration performance. Taken together, this set of empirical methods allow us to estimate the benefits, costs, and ultimately the net effect of collaboration with the focal fraudster on the pre- and post-discovery citation rates of the collaborators.

II. DATA AND METHODS

II.1 Empirical Setting

In September of 2010, one year before his fraud was uncovered, Diederik Stapel was a prominent, prolific, and much lauded social psychologist. Thirteen years into his career, he had just received the "Career Trajectory Award" from the Society of Experimental Social

Psychology, and had recently been appointed the Dean of Social and Behavioral Sciences at Tilburg University. He had published over 100 articles with 63 coauthors from universities all over the world, which together had been cited over 1,000 times. A social psychologist at Oxford University described Stapel as “one of the bright thrusting young stars of Dutch social psychology--highly published, highly cited, prize-winning, worked with lots of people, and very well thought of in the field" (Callaway 2011).

In the summer of 2011, however, Stapel’s career came to a screeching halt. For several years, some of his co-authors had been noticing discrepancies in his data. Yet when one of them initially brought up the possibility of fraud to another colleague, he was told: “Do you really believe that someone with Stapel’s status faked data?” (Bhattacharya and Marshall 2012). At first, when colleagues failed at attempts to replicate his findings, they believed that this was because they lacked his immense empirical skills. Finally, several years after initial doubts were raised, three of Stapel’s PhD students managed to collect sufficient evidence to prove that their supervisor was fabricating data.

By September 2011, Stapel had lost his position at Tilburg University. Fifty-five of his articles were retracted, and he returned his PhD to the University of Amsterdam, stating that his conduct “does not fit with the duties associated with a doctorate” (Verfaellie and McGwin 2011). Within a matter of months, Stapel went from rising star to the best-known fraudster within the field of social psychology, if not in all of academia. An extensive investigation by the Levelt Committee into his fraudulent research practices examined his activities at the three academic institutions he had worked at throughout his career. While they found him guilty of fraud, the committee also determined that Stapel alone was responsible for the misconduct – his coauthors were cleared of all allegations of misconduct (Levelt Committee et al. 2012).

The case of Stapel is far from rare. From 2000 to 2010, annual retractions grew by over 2000% (Van Noorden 2011). Compared to the 44% increase in publications per year over this time, we can see that the rate of retractions is growing much faster than is the publication rate (Marcus and Oransky 2014). Until fairly recently, the dominant belief about retractions was that they were due mostly to honest error (Steen 2011). However, a recent survey found that misconduct, including activities such as plagiarism and data fabrication, was responsible for roughly 66% of retractions (Fang et al. 2012). These figures tell us that fraud is rampant in academic work. Once uncovered, fraud has severe and long-lasting effects on the fraudulent researcher; these negative effects extent even to the field within which the retraction occurred (Azoulay et al. 2015). When an article is retracted due to misconduct, not only do its citation rates drop, but so do the citations of the fraudulent author's previously published work (Azoulay et al. 2016, Lu et al.). The effect is 'immediate, severe, and long-lived: when controlling for age, year, and a fixed article citation effect, in the short term, citations decrease by 65%, by the 2nd year, by roughly 50%, and by the 10th year, by as much as 72% (J. Furman et al. 2012).

Academic work is rarely the product of a sole individual. A shift to increasing teamwork can be seen in most academic fields. Within the social sciences, for example, in 1955 only 17.5% of articles were written by a team of coauthors, while in 2000, 51.5% of publications were written by co-author teams. In addition, the average size of co-authorship teams is increasing. Within the field of psychology, the average co-authorship team size has increased by 75% over the last 45 years (Wuchty et al.).

Two recent papers examine the effect of retraction events on the citations received by the prior work of the entire set of coauthors on a retracted work. Azoulay, Bonatti, and Krieger (2015) found that the pre-retraction work of retracted authors suffers a 10% citation penalty following a retraction event, in comparison to the work of non-retracted control authors. They examined both retraction events due to honest error, and to clear cases of misconduct. Their findings indicate that retractions due to misconduct cause a more pronounced citation penalty to the author's prior work. In addition, they found that more eminent authors within the co-authorship team were punished more severely in cases of misconduct. Jin, Jones, Lu, and Uzzi (2013), examined the differential penalty experienced by members of the coauthor team of a retracted work. They found that less senior members of the co-authorship team suffered a more severe citation penalty, particularly when they worked with an eminent coauthor. Taken together, these findings indicate that authors guilty of misconduct, or who coauthor with a fraudster, experience a post-discovery cost in terms of citations. However, the benefits of research fraud prior to its discovery remain largely unknown. Articles with higher pre-retraction citation rates, published by authors from high-status institutions, are more likely to be retracted than articles with lower citation rates (J. L. Furman et al. 2012). This finding may indicate that fraudulent articles perform better than honest articles prior to the discovery of the fraud. However, it may simply be the case that highly-cited articles are scrutinized more than are articles with lower citation rates, so fraud in lower-status articles may simply be less likely to be discovered. Ultimately, pre-discovery benefits to fraud have remained unexamined in the academic misconduct literature, and discussions of post-discovery costs have not taken these benefits into account.

II.2 Econometric Estimation

The empirical goal of this study is to quantify the pre-discovery benefits and the post-discovery costs of fraud to those who collaborate with the fraudster. We use several empirical approaches to insure that any result we find is not a function of the method we choose. In all of our approaches, our treatment group is comprised of the set of individuals who co-authored with Stapel at least one time. We refer to this set of authors as 'focal co-authors.' The variety of empirical methods we utilize vary in terms of the treatment group against whom we compare the performance of the focal co-authors. First, we create a control group comprised of the entire set of authors with whom the focal co-authors shared publications, but who themselves never co-authored with Stapel. We refer to this set of authors as 'level 2 co-authors'. Next, we utilize coarsened-exact matching to limit the sample of both treatment and control authors. Finally, we test our first hypothesis by utilizing a second empirical strategy, which we refer to as the 'within-author control method'. This approach allows us to use each focal co-author's pre-co-authorship with Stapel citation trajectory as a control against which we estimate the effects of collaboration. We explain both empirical strategies in further detail below.

Matching Strategy

Our aim is to identify authors as similar as possible to the focal co-authors in our treatment group, in order to isolate the effect of co-authoring with Stapel on their citation rates. First, we identify authors similar to the 63 focal co-authors by co-authorship. Namely, we create a group of control authors comprised of the entire set of individuals with whom the focal co-authors worked throughout their careers, but who themselves never co-authored any work with Stapel. This leaves us with 2,427 control authors. We refer to this control group as the 'full sample'. Second, we limit both the treatment and control authors to those who work within the field of psychology, as this is the field within which Stapel worked. This leaves us with 52 focal co-authors in the treatment group, and 796 in the control group, which we refer to as the 'psychology only' sample. Next, we utilize the coarsened exact matching (CEM) approach developed by Iacus et al. (2010) to carefully match focal co-authors with level 2 co-authors, in order to isolate authors who are as similar as possible to each other, with one exception - whether or not they shared a publication with Stapel (Iacus et al. 2012). We match authors based on their career age and their publication and citation rates. We create two different matched sets of authors, in order to answer two different empirical question. First, for each of the 52 focal co-authors working in psychology, we construct discrete bins at year t, where t is the year before the author started working with Stapel, for the following variables: year of first publication, gender, and annual citation and publication rates for the three years preceding t. This strategy allows us to match authors prior to the point at which they began working with Stapel, in order to measure the benefit authors gained upon starting to work with him, prior to the discovery of the fraud. We find matches for 33 of the 52 treatment authors. Under our second matching strategy, we construct discrete bins for the 52 treatment authors at year t, where t is the year before the fraud was discovered (2010), and find matches for 31 of the 52 focal co-authors. This strategy allows us to find matches for treatment authors at the height of their co-authorship with Stapel, just prior to the discovery of the fraud. Comparing treatment authors to this matched sample allows us to isolate the change in citation rates faced by focal co-authors following the discovery of the fraud. Table 1a shows the pre-matched t-tables for the 52 treatment co-authors in psychology, and the set of all possible co-authors in psychology. Table 1b shows the t-tables comparing the treatment group to the control group created through the first matching strategy, while table 1c compares the treatment group to the control group created through the second matching strategy.

Insert Tables 1a – 1c about here

We test hypothesis 1by comparing the pre- and post-co-authorship with Stapel citation rates of treatment authors to three different groups of control authors – the full sample, the psychology sample, and to the sample of level 2 authors matched at the year preceding the year of first co-authorship with Stapel. In order to test this hypothesis, we examine if, compared to control authors, treatment authors experience an increase in their citation rates following the year of first publication with Stapel. As our unit of analysis is at the author-year level, we utilize an ordinary least squares (OLS) estimation with multiple fixed effects. The first empirical model, which allows us to test hypothesis 1 is:

(1) $$Y_{it} = \beta_1 Co\text{-}authorshipStarted_i \ x \ Pre\text{-}discovery_t + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it}$$

The dependent variable, *Yit*, measures the log of the number of annual citations coauthor i receives in year t. The independent variable, $\beta_1 Co\text{-}authorshipStarted_i \ x \ Pre\text{-}discovery_t$ switches to one in the year co-author i first publishes an article with Stapel, and switches back to zero in 2012, once the fraud was discovered. The estimation is limited to the years prior to the year the fraud was discovered (2011). β_1 captures the change in annual citations of co-author i after the year of the first co-authored publication. We capture author fixed effects with μ_i, time effects with δ_t, and career age fixed effects with γ_{it}. In order to control for correlated standard errors within authors, we cluster standard errors at the author level. A positive coefficient on β_1 indicates that following the year in which an author began to work with Stapel, their citation rates increased compared to those of the control authors.

Next, we employ a differences-in-differences framework to determine if, following the discovery of the fraud, the authors who worked with Stapel experienced a decrease in their citation counts, in comparison to authors in the control group. Our second empirical model is:

(2) $$Y_{it} = \beta_1 Co\text{-}author_i \times Post\text{-}discovery_t + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it}$$

As in all of our models, the dependent variable, Y_{it}, measures the log of the number of annual citations coauthor i receives in year t. The independent variable is an interaction composed of two dummy variables, *Co-author*$_i$ and *Post-discovery*$_t$. *Co-author*$_i$ is a dummy variable that equals 1 if an author is a focal co-author, and zero otherwise. *Post-discovery*$_t$ equals 1 if year *t* falls after 2011 (the year of the discovery of the fraud) and remains zero otherwise. A negative coefficient on β_1 indicates that authors in the treatment group experience a drop in their citation rates following the discovery of the fraud, compared to authors in the control group. As in the previous models, we capture author fixed effects with μ_i, time effects with δ_t, and career age fixed effects with γ_{it}, and cluster standard errors at the author level.

Next, we estimate the net effect accrued to collaborators, taking both the pre- and post-discovery effects of co-authorship into consideration.

(3) $$Y_{it} = \beta_1 AfterCoauthorshipStarted_{it} + \beta_2 Co\text{-}author_i \times Post\text{-}discovery_t + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it}$$

This allow us to estimate both the pre- and post-discovery effects of co-authorship in a single model, in order to determine if, compared to an author's performance prior to the period when they first began to work with Stapel, their decision to co-author with him ultimately harmed or benefitted their citation rates. The coefficient on β_1 measures the difference in citation rates of focal co-authors compared to control authors in the period after they began working with Stapel, but prior to the discovery of the fraud. The coefficient on β_2 measures the difference in citation rates of focal co-authors compared to control authors in the period following the discovery of the fraud.

Within-author control

In addition to the empirical approach outlined above, we test our first hypothesis by utilizing our second empirical strategy, the 'within-author control method'. This approach allows us to use each focal co-author's pre-co-authorship with Stapel citation rates as a control against which we estimate the effects of collaboration. We analyze the citation trajectories of all 63 focal co-authors and compare each author's citation trajectory before and after the first time they worked with Stapel, prior to the discovery of the fraud. We use the year of first co-authorship with Stapel to pinpoint the timing of the formation of the collaboration between each co-author and Stapel. This approach allows us to use the citation trajectory of a given author prior to the first year that a paper co-authored with Stapel came out as a within-person control. We utilize the annual citations prior to the year in which each author worked with Stapel for the first time as a baseline against which we compare their annual citation rates after they started working with him. In this way, we isolate the effects of the author's connection to Stapel, in order to determine if an author's citations receive a bump once they have become one of his co-authors. We utilize model (1), but limit our dataset to the 63 focal co-authors, and do not compare them against a set of treatment authors, as our treatment group is now comprised of the pre-co-authorship started author/year observations in our dataset. This second method allows us to perform a robustness test on our results for our first hypothesis. Unfortunately, we cannot use this method to test our additional hypotheses, due to a lack of within-author variation in co-authorship in the post-discovery period.

Moderation Tests

Models (1) - (3) outlined above test the main effects of our study. We create three additional models to examine the moderating effects of gender, career age, and collaboration intensity. We examine the effects of these moderators on the net effect to collaborators. We thus modify either model (2) or (3) in order to include each moderator. First, we estimate the moderating effect of gender by comparing the difference in both the pre- and post-discovery effects experienced by male co-authors compared to female co-authors.

(4) $Y_{it} = \beta_1 Co\text{-}authorshipStarted_{it} \ x \ Male_i + \beta_2 Co\text{-}authorshipStarted_{it} \ x \ Female_i + \beta_3 Co\text{-}author_i \times Post\text{-}discovery_t \ x \ Male_i + \beta_4 Co\text{-}author_i \times Post\text{-}discovery_t \ x \ Female_i + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it}$

We then perform post-estimation tests in order to determine if the difference in the coefficients on β_1 and β_2 is significant, as well as the difference in the coefficients on β_3 and β_4.

Next, we estimate the moderating effect of career age by comparing the difference in both the pre- and post-discovery effects experienced by co-authors are early points in their careers with those at mid or late periods.

(5) $Y_{it} = \beta_1 Co\text{-}authorshipStarted_{it} \ x \ Early_i + \beta_2 Co\text{-}authorshipStarted_{it} \ x \ Mid_i + \beta_3 Co\text{-}authorshipStarted_{it} \ x \ Late_i + \beta_4 Co\text{-}author_i \times Post\text{-}discovery_t \ x \ Early_i + \beta_5 Co\text{-}author_i \times Post\text{-}discovery_t \ x \ Mid_i + \beta_6 Co\text{-}author_i \times Post\text{-}discovery_t \ x \ Late_i + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it.}$

Once again, we perform post-estimation tests to determine if the coefficient on β_1 differs significantly from the coefficient on β_2 and β_3, and if the coefficient on β_4 differs significantly from that on β_5 and $\beta_{6.}$

Finally, we estimate the moderating effect of proportion of publications with Stapel on the post-discovery citation rates of the co-authors. We interact the *proportion of publications with Stapel* variable with the $Co\text{-}author_i \times Post\text{-}discovery_t$ variable, in order to determine if those with a larger percentage of publications with Stapel incurred a greater post-discovery cost than those who had a larger share of publications on which Stapel was not a co-author. We estimate the difference between co-authors with a mean proportion of publications with Stapel (*co-author x post-discovery)* and co-authors with proportions greater than the *mean (co-author x post-discovery x proportion of publications with Stapel)*.

(6) $Y_{it} = \beta_1 Co\text{-}author_i + \beta_2 Co\text{-}author_i \times Post\text{-}discovery_t + \beta_3 Co\text{-}author_i \times Post\text{-}discovery_t \times ProportionOfPublicationsWithStapel_{it} + \gamma_{it} + \mu_i + \delta_t + \varepsilon_{it}$

A negative coefficient on β_3 indicates that individuals who have a larger proportion of publications with Stapel than the average proportion of authors in our dataset experienced a greater drop in their citation rates following the discovery of the fraud than those who shared a smaller proportion of work with him.

II.3 Data

The costs and benefits of fraud are difficult to quantify for a number of reasons. An ideal empirical setting for our study should fulfil a set of criteria that would allow us to bypass the following problems. The hidden nature of fraud, combined with the propensity of its effects to spread, can pose serious roadblocks in its examination. Because of these factors, studies examining fraud tend to be limited by the lack of availability of empirical data. In particular, it can be difficult to obtain information regarding the involvement of actors in cases of fraud, as involved individuals, or individuals who are simply connected to the fraudster in some way, may hesitate to provide information related to the fraudulent act. This lack of information can make it difficult to compile complete information about the benefits or costs of those who collaborate with a fraudster. In order to fully capture the returns to fraud, we must obtain information regarding all parties involved with the fraudster throughout the entirety of his or her career. Thus, we need to examine a case in which we are able to access the full set of co-authors an individual has ever worked with, and to examine their performance over time. The case we study must satisfy this condition.

From an empirical standpoint, the Stapel case is ideal for our study as it allows us to gather information on the performance of Stapel's entire set of co-authors, as well as on the entire set of individuals these co-authors worked with. The performance measure on which we base our analysis is that of citation counts. One of the primary measures of an academic researcher's performance is the rate at which his or her articles are cited. As citations can be measured annually, we can isolate both the pre- and post-discovery effects of the fraud on co-author performance by limiting our dataset to include only citations prior to or following the year the fraud was discovered.

II.4 Measures

The goal of this study is to examine how fraud effects the careers of the fraudulent researcher's co-authors, both before and after its discovery. Many different factors could be used as a proxy to measure an author's performance, including the amount of time it takes them to obtain a tenure-track position, the ranking of the institution at which they obtain a position, their salary, their number of publications, or the number of times their work is cited. We chose to use citation rates as our measure of an author's performance for several reasons. First, citation data can be limited to different periods of time, which allows us to easily isolate pre- and post-discovery effects. Second, citation data is standardized, meaning that the way in which it is measured does not contain any degree of uncertainty, as many of the other potential measures do. In addition, citation data, unlike measures such as salaries, is easily accessible. We believe that citation rates can be utilized as a proxy for career performance, as citations provide evidence of the fact that the members of a researcher's community view their work as useful and relevant to furthering research in the area. In fact, citations have been found to be a positive and significant determinant of salaries (Diamond, 1986).

Our treatment group is comprised of Stapel's co-authors. We define a co-author as someone who has published at least one article with Stapel prior to 2011, when the fraud was uncovered. We consider treatment to be a time-invariant characteristic, as all authors who had at least one co-authored publication with Stapel are considered members of the treatment group.

II.5 Sample

Our sample is composed of the entire set of Stapel's co-authors (whom we refer to as 'focal co-authors), and all the articles they published throughout their careers, as well as the entire set of individuals the focal co-authors worked with, who themselves never worked with Stapel. We utilize Elsevier's Scopus database to obtain Stapel's full publication history, and to compile the entire set of focal co-authors with whom he worked over the course of his career, as well as the entire set of individuals each of the focal co-authors worked with throughout their careers. We chose the Scopus database due to its unique Author ID feature, which allowed us to accurately isolate each individual who co-authored with Stapel at least one time.

According to the Scopus database, Stapel published a total of 114 journal articles, 1 book, 1 book chapter, 1 editorial, 1 response letter, and 1 review article over the course of his career (Stapel wrote an additional book about his fraud in 2014). Scopus lists 66 individuals as Stapel's co-authors on these projects; however, we merged three of these observations, as they were found to be duplicates composed of mistyped co-author names. This left us with 63 co-authors, who published 114 articles with Stapel over the course of their careers. We pulled citation reports for all articles published by Stapel, and by his 63 co-authors, for the years ranging from 2001-2016. We chose this range as Stapel began his first tenure-track position in 2000. Citation reports include all publications by Stapel's 63 co-authors, regardless of whether Stapel was one of the authors on the article. As citations are heavily skewed to the right, we utilize the natural log of annual citation counts as our dependent variable, rather than pure citation counts. Figure 1a shows both the distribution of annual citation counts of the papers published by the authors in our dataset (leftmost graph), as well as the distribution of the natural log of citation counts (graph on the right). Figure 1b, meanwhile, shows the distribution of annual citation counts of the papers published by the authors in our dataset in which Stapel was not one of the co-authors (leftmost graph), as well as the distribution of the natural log of these citation counts (graph on the right). We include a histogram of publications by year for Stapel's 63 co-authors in Figure 1c. Publications are heavily skewed to the right as well, as most co-authors do not publish each year. The highest number of publications in a given year for a single author within our dataset was 92; the author works within the field of medicine, where publications with long lists of co-authors are fairly common.

Insert Figures 1a -1c about here

II.6 Unit of Analysis

Our study aims to quantify the costs and benefits that accrue from collaborating with a fraudster. We examine the changes to an author's performance before and after first working with Stapel. Although citations can be viewed at the article level, we utilize author-level citation counts, as we are interested in studying author-level effects. Our unit of analysis is thus the author-year dyad.

Insert Table 2a about here

II.7 Descriptive Statistics

Table 2a presents descriptive statistics for the 63 focal co-authors in our dataset. The gender breakdown of our sample is skewed slightly toward males, and heavily toward authors working within the field of psychology. The average focal co-author had already been publishing for close to nine years when he or she first published a paper with Stapel. However, 16 co-authors in our dataset did not have any publications at the time they started working with Stapel, while 2 co-authors had 34 years of publications at that point. The average co-author shared 21% of his publications with Stapel and had roughly 58 publications and 70 co-authors as of 2016.

Insert Tables 2b and 2c about here

Table 2b presents descriptive statistics broken out by field (divided into psychology, with N=52, and other, with N=11), while table 2c presents descriptive statistics by gender (divided into male, N=36, and female, N=27). We can see that authors with higher numbers of publications and co-authors tend to work in fields outside of psychology (predominantly in medicine), where co-authorship teams tend to be large, and publications more frequent than in fields such as psychology. On average, female co-authors had fewer publications and co-authors than males, but a larger percentage of their work was co-authored with Stapel.

Insert Table 3 about here

III. RESULTS

III.1 Pre-Discovery Benefits

Table 3 presents four specifications estimating of the effect of co-authorship on the annual citation rates of Stapel's co-authors, prior to the discovery of his fraud. The reference category for all four models presented in this table is *before co-authorship started*, meaning that each author's citation rates are compared to the citation rates they had prior to co-authoring with Stapel. The independent variable*, co-authorship started x pre-discovery,* indicates that at year t, author i has at least one publication with Stapel, and that the period of analysis is limited to the period prior to the discovery of the fraud. Specifications (1), (2), and (3) utilize level 2 co-authors as a control group against whom the performance of the treatment group is compared. Specification (1) estimates the pre-discovery difference in citation rates between the 63 focal co-authors in the treatment group and a control group consisting of 2,428 level 2 authors. Specification (2) limits the sample to authors within the field of psychology, and consists of 52 focal co-authors in the treatment group, and 796 level 2 authors in the control group. Specification (3) compares a matched sample of authors to each other, who have been matched at the period preceding each focal co-author's first publication with Stapel. Finally, specification (4) limits the sample to the 63 focal co-authors, and uses each author's pre-co-authorship started citation rates as a control against which post-co-authorship started citation rates are measured. We find evidence of a significant main effect of co-authorship on pre-discovery citation rates across all four models, as the coefficient on *co-authorship started x pre-discovery* is positive and significant in all four specifications. We find that, compared to the control group, Stapel's co-authors experienced an increase in their citation rates that was between 23% (specification 3) and

37% (specification 2). When compared against their own pre-co-authorship started citation rates, focal co-authors experienced an increase of roughly 42% upon starting to work with Stapel.

Insert Table 4 about here

III.2 Post-Discovery Costs

Table 4 presents three specifications estimating the effect of co-authorship on the annual citation rates of Stapel's co-authors, following the discovery of his fraud. The reference category for all four models presented in this table is *after co-authorship started*, meaning that each author's citation rates are compared to their citation rates during the period in which they were co-authoring with Stapel, but his fraud had not been discovered yet. The independent variable, *co-author × post-discovery,* indicates that author i is one of Stapel's focal co-authors, and that, at year t, Stapel's fraud has been discovered. Specification (1) compares the difference in pre- and post-discovery citation rates of focal co-authors to the full sample of level 2 co-authors, specification (2) limits the sample to authors within the field of psychology, and finally, specification (3) compares the citation rates of a sample of authors matched as of the year preceding the discovery of the fraud. We find evidence of a significant negative main effect of co-authorship with Stapel on post-discovery citation rates across all three models. Compared to the full sample of level 2 co-authors, those who worked with Stapel experienced a 30% drop in citations upon the discovery of the fraud, relative to their citation rates during the period in which they were working with Stapel, but he was not yet known to be a fraudster. Authors in the field of psychology experienced a slightly larger drop, of roughly 34%, while the matched sample of authors in specification (3a) experienced the largest drop, of 62%.

Insert Table 5 about here

III.3 Net Effect

Table 5 presents the net effect of co-authorship with Stapel, by estimating both the pre- and post-discovery effects of co-authorship on citation rates. The reference category for all four models presented in this table is *before co-authorship started*. In this way, we estimate the change in an author's citation rates from the time before he or she started working with Stapel, both prior to the discovery of the fraud and following it. The independent variable, *after co-authorship started,* indicates that at year *t*, author *i* has at least one publication with Stapel, while the independent variable, *co-author* × *post-discovery,* indicates that author *i* is one of Stapel's focal co-authors, and that, at year *t*, Stapel's fraud has been discovered. We see a significant positive effect of co-authorship in the pre-discovery period across all four specifications, ranging from 55% in specification (3) to roughly 68% in specification (2). We find that this significant positive effect remains in the post-discovery period in one of our four specifications; in the remaining three specifications, the coefficient trends in the right direction, but is insignificant. This positive coefficient on the *co-author* × *post-discovery* variable indicates that, following the discovery of the fraud, focal co-authors were still more highly cited than they had been prior to the period in which they began co-authoring with Stapel. However, the size of the benefit greatly drops in this post-discovery period compared to the pre-discovery period; authors experience 7-39% higher citation rates following the discovery of the fraud, while in the pre-discovery period, their citations had been 55-68% higher than they were prior to starting co-authorship. Post-estimation tests reveal that the pre- and post-benefits differ significantly in 3 of the 4 specifications. It is only in specification (3), in which treated authors are matched with control authors based on their pre-collaboration citation and publication rates, that the benefit they experience following the discovery of the fraud is still comparable to the benefit they received prior to its discovery. Thus, when compared to authors who never performed at post-collaboration levels, treated authors continue to accrue a benefit.

Insert Table 6 about here

III.4 Moderating Effects of Gender, Career Age, and Collaboration Intensity

Finally, we examine the moderating effect of a co-author's gender and career age on the net effect of collaboration, as well as the moderating effect of the proportion of publications shared with Stapel on the post-discovery costs incurred by co-authors. Table 6 presents the moderating effect of gender on the pre-discovery benefits and post-discovery costs of co-authorship. The reference category for all three models presented in this table is *before co-authorship started*. The independent variables interact our previous main independent variables, *co-authorship started, co-authorship started* × *pre-discovery* and *co-author* × *post-discovery,* with the two gender indicators, *male* and *female*. Columns (1a), (2a), and (3a) show the net effect as a whole, while columns (1b) (2b), and (3b) break out the effect into the pre- and post-discovery periods. The results of these regressions prove to be rather shocking – they indicate that both the pre- and post-discovery benefits of co-authorship we saw in our previous models accrue only to male co-authors. This effect is consistent across all three specifications. The differential effect is most pronounced in the post-discovery period, when male co-authors continue to accrue significant benefits. Female co-authors, however, experience a significant decrease in their citation counts compared to the pre-discovery period.

Insert Table 7 about here

Table 7 presents the moderating effect of a co-author's career age on the net effect of collaboration. We split our sample of focal co-authors into three career age categories: *early*, *mid*, and *late*, based on how many years they have been publishing compared to the other authors in the sample, at the time the fraud was discovered. We then interact our previous main independent variables, *co-authorship started*, *co-author* × *pre-discovery* and *co-author* × *post-discovery* with these three career age indicators. The reference category for all three models presented in this table is *before co-authorship started*. Columns (1a), (2a), and (3a) show the net effect as a whole, while columns (1b) (2b), and (3b) break out the effect into the pre- and post-discovery periods. Across all three specifications, we can see that only authors within the 'early' category experience a significant net benefit to collaboration. This finding goes against hypothesis 4a, but is in line with 4b, as authors at earlier stages in their careers seem to be more shielded from post-discovery costs than authors further along in their career.

Insert Table 8 about here

Finally, table 8 presents the moderating effect of the proportion of publications a co-author has with Stapel on his or her post-discovery citation rates. We estimate the difference between co-authors with a mean proportion of publications with Stapel (*co-author x post-discovery)* and co-authors with proportions greater than the *mean (co-author x post-discovery x proportion of publications with Stapel)*. The reference category for all three models presented in this table is *before co-authorship started*. Across all three specifications, we find that co-authors who publish with Stapel at greater than the mean level increase a greater drop in citations in the post-discovery period.

IV. THEORETICAL CONTRIBUTION

The primary contribution of our work is empirical. We measure both the pre-discovery benefits and the post-discovery costs of collaborating with a fraudster and obtain empirical evidence of an ultimate net benefit. In addition, we find that this benefit accrues differently across a heterogeneous set of collaborators based on various individual-level characteristics. These empirical results lead to two primary theoretical contributions. First, the primary, positive net effect of collaboration can lead us to understand more about the motivation individuals may have both in deciding to work with a fraudster, and to remain silent when faced with evidence of misconduct. Second, our findings regarding the moderating effects of individual-level characteristics can lead us to draw conclusions about which potential co-authors stand to gain the most from working with a fraudster, and which have the most to lose.

IV.1 Motivation for working with a fraudster

At the height of their fraud, prior to being discovered, successful fraudsters often hold very high levels of status. The Stapel case illustrates this phenomenon. Thanks to his misconduct, Stapel had become not only a highly cited – and respected – academic in his field; he had also obtained a prominent position as the dean of Tilburg University's School of Social and Behavioral Sciences and was described as 'one of Europe's best social psychologists' (Enserink 2011). The catchy 'findings' of his articles made him a celebrity in the Netherlands, as he was often featured in newspapers and on television shows. Potential collaborators were lining up to work with him. Several psychological mechanisms can explain the incentive that a high-status player such as Stapel had to commit fraud, as well as the incentive that potential collaborators had to work with him, and to remain silent when faced with evidence of his misconduct.

Social status is defined as the respect, prestige, and admiration that individuals enjoy in the eyes of others (Anderson et al. 2001). An individual's status is initially determined by inferences drawn about his or her value based on a variety of observed characteristics and actions (e.g. Berger et al. 1977, Ridgeway 1991). Once an individual's status has been established, his or her subsequent actions are judged with that status in mind (e.g. Bowles and Gelfand 2010, Fiske et al. 2002). In general, high status is associated with a wide array of social benefits. For instance, high-status individuals are generally more respected than their lower-status peers (e.g. Barkow et al. 1975, Goldhamer and Shils 1939), are viewed as being more competent (Fiske et al. 2018), and are allowed more control over group decisions and processes (e.g. Bales 1950, Berger et al. 1977). Several recent studies have examined the effects of wrongdoer status on observer punishment recommendations. In cases where transgressions are ambiguous, observers have a tendency give the wrongdoer moral credentials, by reinterpreting the transgression as more positive than it really is, when the wrongdoer is of high status rather than low status (Polman et al. 2013). For instance, if a high-status individual is seen pushing someone, observers are more likely to assume that the individual is performing the act for a worthy reason (such as trying to push the person out of harm's way), rather than as an act of aggression. However, when there is no ambiguity about the transgression, high-status individuals are actually assigned greater intentionality and thus punished more severely than are low-status individuals (Fragale et al. 2009).

Based on the definition of social status outlined above, Stapel had clearly achieved a high level of status at the height of his fraud. According to his own account, his fraud started out small – he changed a few values here and there, in order to obtain stronger results (Stapel 2014). As he achieved more and more success from his fraudulent acts, they started to grow more serious, until he was finally making up entire studies. His high status may have given him the courage, and arguably the overconfidence, to ultimately commit blatant and widespread fraud, as he realized that his actions were not being questioned. In the meantime, even if his colleagues sensed that something was wrong, his high status may have made him an appealing co-author nonetheless. This appeal can be explained by several psychological mechanisms.

First, potential collaborators may have felt that working with Stapel was a safe bet because of the benefits that he faced due to his high status. In fact, for many years, Stapel's status acted as a shield which protected him from having to defend his questionable research practices. He was known for producing 'perfect' papers, that all reported significant results. Meanwhile, none of his collaborators were exposed to the raw data he collected. Eventually, as the collaborators spoke to each other about their experiences with Stapel, some came to realize that his results may be too good to be true. One such co-author was Ad Vingerhoets, who turned to a retired professor for advice on the matter. The professor replied with the following: "Do you really believe that someone with Stapel's status faked data?" (Bhattacharjee 2013). Other colleagues, meanwhile, who tried to replicate Stapel's results, believed that they consistently failed to do so because they 'lacked his skill' (Jump 2011). Ultimately, his high status acted as a safeguard which allowed him to continue fabricating data for many years, and to publish numerous papers based on studies that never existed (the exact number of studies based on falsified data has not yet been determined; as of the time of writing, 58 of Stapel's publications have been retracted).

One of the psychological phenomena that can explain the draw that individuals have to work with high-status others is that of basking in reflected glory. This construct holds that individuals understand that observers exhibit a tendency to evaluate connected individuals similarly (Cialdini et al. 1976). The idea that we will be judged by the individuals we are connected to is engrained in us from an early age. For instance, teenagers have been found to affiliate with popular peers in order to gain status themselves. The closer their relationship to the high-status peer, the higher the likelihood that their peers will deem them to be likeable, and that they will obtain high levels of status themselves (Dijkstra et al. 2010). Stapel's high status may have drawn collaborators to work with him in order to bask in his reflected glory. However, based on our findings, collaborators can experience a measurable benefit to their own performance when working with a fraudster. Thus, the glory that Stapel's collaborators experienced is not simply reflected; rather, Stapel's success actually became their own.

Although we remain agnostic about whether Stapel's co-authors had any knowledge of his misconduct, we theorize that, if they did sense that something was not quite right with his work, the benefit they were obtaining from their collaboration may have given them an incentive to remain silent. The choice to 'look the other way' can be explained by the process of selective exposure. Selective exposure includes the tendency to expose ourselves to information that is in line with our existing attitudes, while ignoring counter-attitudinal information (Sears and Freedman 1967). Although this notion has been around for more than half a century, it has been greatly debated. This is due to the fact that empirical work testing selective exposure has come up with conflicting findings. However, several recent studies have found evidence of individuals engaging in selective exposure strategies. A few examples include evidence of selective exposure to political information in the media (Knobloch-westerwick 2009) and in choosing online information regarding political candidates (Garrett 2009). Thus, as Stapel was such a highly-regarded figure in his field, in order to reduce cognitive dissonance, collaborators that were suspicious of his behavior may have been inclined to look the other way.

When taken together, the psychological mechanisms described above may have created the ideal conditions for Stapel's fraud. His high status both gave him the confidence to perpetuate more and more serious levels of fraud, and his colleagues the confidence to work with him, despite potential suspicions they may have had. For many years, his status shielded him from having to face questions about his research practices. Due to selective exposure strategies, his collaborators may not have even allowed themselves to become aware of the fraud, and ultimately the betrayal, of their highly esteemed colleague.

IV.2 Differential effects of collaboration

In addition to our primary finding, our results provide evidence of a differential benefit to collaborating with a fraudster, which varies based on two individual-level characteristics of the co-authors: gender and career age. We chose to examine the moderating effects of these two specific characteristics, as both are known to be tied to perceptions of an individual's status (Eagly 2013, Ridgeway 1991) and the amount of credit assigned under conditions of uncertainty (Heilman and Haynes 2005). Our findings indicate that male co-authors stand to gain more from collaborating with a fraudster than do their female peers, in particular, if they are at an early point in their career at the time of collaboration.

IV.2.A Gender

Our findings add to the literature on the differential perceptions of men and women's contributions in mixed-gender teams. Although social psychology literature has consistently shown that women tend to be viewed as less competent than their male colleagues (e.g. Heilman 2001, Huddy and Terkildsen 1993), the literature examining perceptions of contributions in mixed-gender teams is rather limited. Recent studies have found that, under conditions of uncertainty, women receive more blame when they are part of an unsuccessful mixed-gender team (Haynes and Lawrence 2012), and less of the credit when part of a successful mixed-gender team (Heilman and Haynes 2005). Women have even been found to assign themselves less credit than they assign to their male teammates, when they are uncertain about how much they have contributed to a successful joint work outcome (Haynes and Heilman 2013). Within the context of academia, women are less likely to receive tenure the more they co-author with men, while men are tenured at roughly the same rate regardless of whether they co-author or solo-author (Sarsons et al. 2017). This is only the case in disciplines in which there is ambiguity in regard to the amount of work each author put into the publication; within disciplines in which co-authors are listed in order of contribution, men and women receive equal credit for co-authored work.

Our findings add to this body of literature, as we find that the benefit from collaborating with a fraudster only accrues to male co-authors. Across all three samples for which we have gender information (the full sample of authors in the field of psychology, and our two matched samples), we find that the pre-discovery benefit to co-authorship is only significant for men – the effect for women is insignificant. In the post-discovery period, the benefit men experience decreases compared to the pre-discovery period, but is still positive. Women, meanwhile, experience a drop in their citation rates compared to the pre-collaboration period, although this drop is not significant. It is important to note that the publications in our sample are within the field of psychology; a field within which co-authors are typically listed by order of contribution. Thus, according to the findings of Sarsons (2017), this effect should be even stronger in a field where authorship order is alphabetical. Thus, our study presents additional evidence of a tendency to assign less credit to women in mixed-gender teams. In the case of collaborating with a fraudster, women do not stand to benefit from their collaboration, and may even experience a drop in their performance following the discovery of the fraud.

IV.2.B Status

The literature examining the effects of social status provides an abundance of benefits to high status individuals, including higher levels of respect (Barkow et al. 1975, Goldhamer and Shils 1939), perceptions of higher competence (Fiske et al. 2018), and greater control over group decisions and processes (e.g. Bales 1950, Berger et al. 1980). Our results, however, present a benefit to low-status individuals, as we found that lower status co-authors gained more from collaborating with Staple than did their higher-status peers. These findings are robust across our three samples of co-authors. Although we cannot draw conclusions from our analysis as to the mechanism behind this effect, we theorize that it is driven by the lack of information in the market about authors at early stages of their careers. Thus, being associated with a star researcher such as Stapel may provide information to the market about junior co-authors, which carries more weight than the information provided about more senior co-authors. Surprisingly, even in the post-discovery period, junior co-authors are left better off than their more senior colleagues. Across two of our three samples, we find that authors in the 'mid-career' category lose the most in the post-discovery period. This finding is in line with Polman et al. (2013), who find that both high- and low-status wrongdoers receive moral license, but through two different mechanisms. In the case of Stapel's co-authors, it may be the case that lower-status, junior co-authors receive more sympathy from the market, while the most senior co-authors receive moral credentials.

V. DISCUSSION AND CONCLUSION

In this dissertation, we provide empirical evidence of both costs and benefits to fraud. Ultimately, our study presents evidence of performance gains associated with collaborating with a fraudster. Across multiple samples, and through two different empirical approaches, we find that collaborators experience a significant performance gain upon starting to work with the fraudster, prior to the discovery of the fraud. Surprisingly, this effect seems to occur regardless of the strength of the relationship between the collaborator and the fraudster – collaborators who had only shared a small body of co-authored work with the fraudster still accrued benefits upon starting to work with him. Once the fraud was discovered, collaborators experienced a significant drop in their performance. In the case of post-discovery costs, however, we find that collaborators who shared a larger proportion of work with the fraudster suffered greater losses. In addition to quantifying the pre-discovery benefits and post-discovery costs, our approach allows us to calculate the net effects of collaborating with a fraudster, to determine if collaborators were ultimately better or worse off as a result of the collaboration. We find that, despite the significant drop in the post-discovery period, collaborators experienced net gains to performance. In other words, compared to similar others, individuals who worked with the fraudster performed better upon working with him, even once the fraud was discovered. We find considerable heterogeneity in this effect across our sample of collaborators. Ultimately, the benefit only seems to accrue to males; female collaborators do not experience a significant effect. Finally, we find that the least senior collaborators in our dataset benefit the most; while those in the 'mid-career' range gain the least.

The contribution of this dissertation is primarily empirical. However, by successfully quantifying both the costs and benefits to fraud, we are able to draw several theoretical conclusions. First, our findings allow us to develop theory on the motivation of collaborators to work with a fraudster, and to look the other way when facing evidence that indicates fraudulent behavior. Second, our findings allow us to draw conclusions on which collaborators have the most to gain, and which have the most to lose, when collaborating with a fraudster.

It is important to note that our findings are subject to a set of boundary conditions, some of which point to avenues of future research. Our most serious limitation is the fact that we only examine the effects of collaborating with a fraudster on a single performance indicator – an author's citation rates. While this variable is certainly an important component of an author's performance, it is important to note that we cannot conclude whether collaborators' overall performance ultimately suffered or improved as a result of working with the fraudster. In order to determine if collaborators were truly better or worse off as a result of the fraud, we would need to examine many other performance indicators in addition to citation rates. As many of these performance measures are not straightforward to measure, a qualitative approach could provide a more comprehensive understanding of what each of the collaborators experienced as a result of their connection to the fraudster. For the purposes of this dissertation, we have included a section within the appendix (Appendix C) in which we examine the current status of the co-authors.

An additional weakness of our study is that we examine a relatively recent case of fraud, which greatly limits the amount of data we have in the post-discovery period. In the case of the Stapel fraud, only the passage of time will tell if costs will continue to build over the years, or if they will weaken as the fraud becomes more of a distant memory. Future studies could, however, overcome this limitation by examining other, older cases of fraud, in order to obtain a larger number of post-discovery observations. Finally, our focus on a single case of fraud makes the generalizability of our findings questionable. Our small sample size leaves room for doubt, that our findings are a result of features particular to our data. Repeating the present study, but on a different case of fraud, could alleviate this worry.

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Figure 1a: Distribution of Citation Counts – All Publications

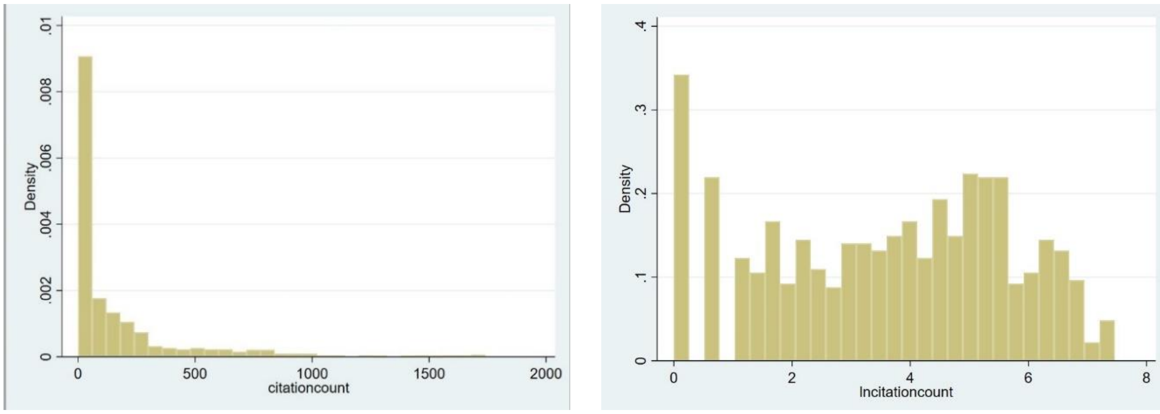

Figure 1b: Distribution of Citation Counts – Non-Stapel Publications

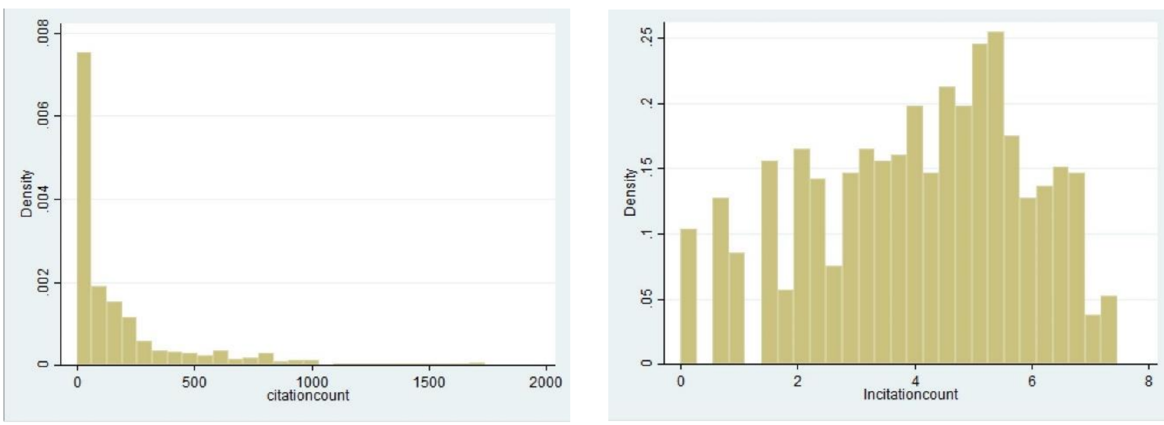

Figure 1c: Distribution of Co-Author Publications by Year

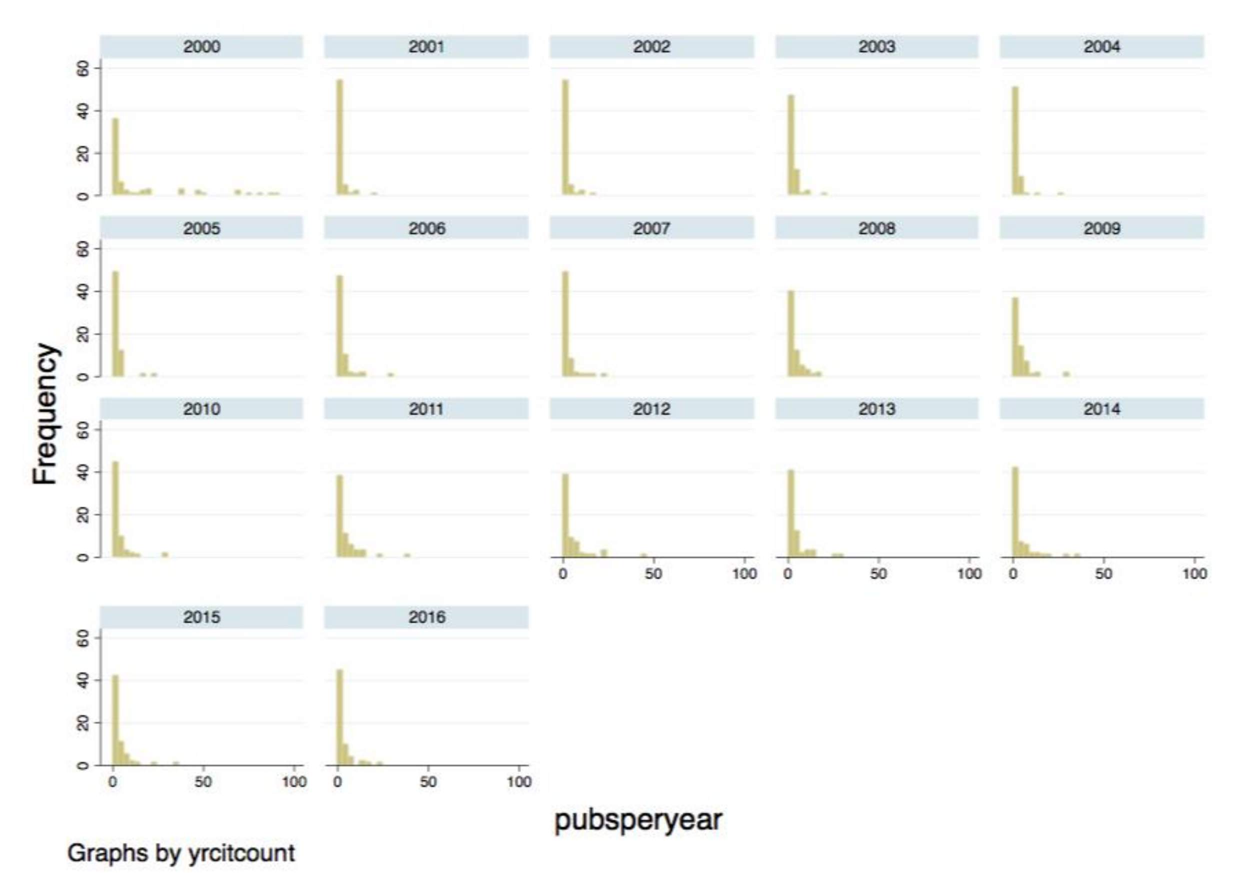

Table 1a: Mean Differences Between Treated and Unmatched Control Authors (Limited to Psychology)

Variable	Mean – Treated Authors	Mean – Control Authors	Mean Difference
N	52	793	
1 Career Age at 2010	14.49	14.53	0.04
2 Citations 2008	126.08	85.85	40.23
3 Citations 2009	148.75	98.31	50.43*
4 Citations 2010	166.53	112.33	54.20*
5 Publications 2008	3.06	1.89	1.17***
6 Publications 2009	3.49	2.11	1.38***
7 Publications 2010	2.80	2.14	0.67

Table 1b: Mean Differences Between Treated and Matched Control Authors – Matching Strategy 1 (Matched at year preceding first publication with Stapel)

Variable	Mean – Treated Authors	Mean – Control Authors	Mean Difference
N	33	33	
1 Career Age at 2010	10.30	10.09	0.21
2 Citations 2008	71.79	59.55	12.24
3 Citations 2009	87.67	74.79	12.88
4 Citations 2010	105.39	89.51	15.88
5 Publications 2008	2.79	2.15	0.64
6 Publications 2009	2.97	2.61	0.36
7 Publications 2010	2.76	2.52	0.24

Table 1c: Mean Differences Between Treated and Matched Control Authors – Matching Strategy 2 (Matched at 2010)

Variable	Mean – Treated Authors	Mean – Control Authors	Mean Difference
N	31	31	
1 Career Age at 2010	9.19	8.10	1.10
2 Citations 2008	46.39	45.26	1.13
3 Citations 2009	53.65	52.90	0.74
4 Citations 2010	56.00	59.58	-3.58
5 Publications 2008	1.61	1.32	0.29
6 Publications 2009	1.61	1.48	0.13
7 Publications 2010	1.32	1.03	0.29

Table 2a: Summary Statistics for Co-Authors

Variable					
1 Gender	Male	Female			
	36	27			
2 Area	Psychology	Other			
	52	11			
	Mean	Median	SD	Min	Max
3 Career age at year of first co-authorship with Stapel (years since first publication)	8.95	6	10.17	0	34
4 Proportion of publications with Stapel	0.206	0.080	0.249	0.004	1
5 # co-authors	70.48	36.5	108.69	1	679
6 # publications	58.05	29	81.99	1	523
7 # publications with Stapel	3.02	2	3.84	1	27

Table 2b: Summary Statistics by Area

	Psychology					Other				
Variable	Mean	Median	SD	Min	Max	Mean	Median	SD	Min	Max
1 Career age at year of first co-authorship with Stapel (years since first publication)	9.10	4	10.40	0	34	8.27	2	9.38	0	26
2 Proportion of publications with Stapel	0.201	0.085	.216	0.006	1	0.183	0.054	0.306	0.004	1
3 # co-authors	53.71	35	55.42	1	239	148.27	41	220.49	1	679
4 # publications	49.71	31	48.36	2	178	97.46	24	166.39	1	523
5 # publications with Stapel	3.27	2	4.19	1	27	1.82	2	0.75	1	3

Table 2c: Summary Statistics by Gender

	Male (N=36)					Female (N=27)				
Variable	Mean	Median	SD	Min	Max	Mean	Median	SD	Min	Max
1 Career age at year of first co-authorship with Stapel (years since first publication)	12.10	9	11.07	0	34	4.00	1	5.72	0	21
2 Proportion of publications with Stapel	0.109	0.045	0.139	0.004	0.500	0.314	0.307	0.277	0.016	1
3 # co-authors	98.5	76.5	129.49	1	679	27.13	13	32.08	1	119
4 # publications	81.72	62	96.05	2	523	20.35	10	19.65	1	62
5 # publications with Stapel	3.05	2	4.36	1	27	3.04	2	2.98	1	13

Table 3: Pre-Discovery Benefits - OLS

Dependent variable:	Annual citation count (logged)							
Time period of analysis:	2001 to 2011							
Empirical strategy	Focal authors' co-authors as controls		Focal authors' co-authors as controls		CEM Matching		Within-author controls	
Sample	Full		Psychology only		Matched at year preceding focal authors' first year of co-authorship		Entire focal author population	
N treated authors	63		52		33		63	
N control authors	2428		796		33		N/A	
	(1)		(2)		(3)		(4)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-authorship started x Pre-discovery	0.375 (0.105)	0.000	0.381 (0.132)	0.004	0.232 (0.134)	0.089	0.416 (0.143)	0.005
Author fixed effects	✓		✓		✓		✓	
Year fixed effects	✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓	
Observations	18400		7644		508		567	
R-squared	0.927		0.930		0.946		0.943	

Notes: Observations are at the author$_i$-year$_t$ level. Robust standard errors clustered at the author level are in parentheses. The reference category in all models is "before co-authorship started".

Table 4: Post-Discovery Costs - OLS

Dependent variable:	Annual citation count (logged)					
Time period of analysis:	Year co-authorship started to 2016					
Empirical strategy	Focal authors' co-authors as controls		Focal authors' co-authors as controls		CEM Matching	
Sample	Full		Psychology only		Matched at 2010	
N treated authors	63		52		31	
N control authors	2428		796		31	
	(1)		(2)		(3)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-author x Post-discovery	-0.302 (0.100)	0.003	-0.342 (0.113)	0.003	-0.625 (0.212)	0.005
Author fixed effects	✓		✓		✓	
Year fixed effects	✓		✓		✓	
Career age fixed effects	✓		✓		✓	
Observations	29989		11753		714	
R-squared	0.906		0.910		0.892	

Notes: Observations are at the author$_i$-year$_t$ level. Robust standard errors clustered at the author level are in parentheses. The reference category in all models is "after co-authorship started".

Table 5: Net Effects- OLS

Dependent variable:	Annual citation count (logged)							
Time period of analysis:	2001 to 2016							
Empirical strategy	Focal authors' co-authors as controls		Focal authors' co-authors as controls		CEM Matching		CEM Matching	
Sample	Full		Psychology only		Matched at year preceding focal authors' first year of co-authorship		Matched at 2010	
N treated authors	63		52		33		31	
N control authors	2428		796		33		31	
	(1)		(2)		(3)		(4)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-authorship started x Pre-discovery	0.673 (0.128)	0.000	0.684 (0.160)	0.000	0.549 (0.191)	0.005	0.675 (0.231)	0.005
Co-author x Post-discovery	0.391 (0.137)	0.004	0.358 (0.174)	0.040	0.281 (0.238)	0.241	0.066 (0.277)	0.812
Author fixed effects	✓		✓		✓		✓	
Year fixed effects	✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓	
Pre-post difference	0.282	0.005	0.326	0.004	0.268	0.201	0.609	0.006
Observations	30171		11885		838		778	
R-squared	0.905		0.910		0.921		0.892	

Notes: Observations are at the author$_i$-year$_t$ level. Robust standard errors clustered at the author level are in parentheses. The reference category in all models is "before co-authorship started".

Table 6: Moderating Effects of Gender - OLS

Dependent variable:	Annual citation count (logged)											
Time period of analysis:	2001 to 2016											
Empirical strategy	Focal authors' co-authors as controls				CEM Matching				CEM Matching			
Sample	Psychology				Matched at year preceding focal authors' first year of co-authorship				Matched at 2010			
N treated authors	52				33				31			
N control authors	796				33				31			
	(1a)		(1b)		(2a)		(2b)		(3a)		(3b)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-authorship started x Male	0.664	0.000			0.547	0.004			0.775	0.005		
	(0.163)				(0.184)				(0.263)			
Co-authorship started x Female	0.044	0.855			0.119	0.740			0.187	0.583		
	(0.240)				(0.358)				(0.339)			
Co-authorship started x Pre-discovery x Male			0.773	0.000			0.624	0.001			0.947	0.001
			(0.169)				(0.173)				(0.262)	
Co-authorship started x Pre-discovery x Female			0.341	0.169			0.281	0.431			0.415	0.211
			(0.248)				(0.355)				(0.328)	
Co-author x Post-discovery x Male			0.574	0.002			0.460	0.071			0.572	0.090
			(0.181)				(0.251)				(0.332)	
Co-author x Post-discovery x Female			-0.243	0.376			-0.150				-0.463	0.280
			(0.275)				(0.443)	0.736			(0.424)	
Author fixed effects	✓		✓		✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓		✓		✓	
Author x Year fixed effects	✓		✓		✓		✓		✓		✓	
Male - female difference - net	0.620	0.033			0.428	0.298			0.588	0.175		
Male - female difference - pre			0.432	0.149			0.343	0.393			0.532	0.205
Male - female difference - post			0.817	0.013			0.610	0.242			1.035	0.061
Observations	11885		11885		838		838		778		778	
R-squared	0.911		0.911		0.927		0.927		0.889		0.889	

Notes: Observations are at the author$_i$-year$_t$ level. Robust standard errors clustered at the author level are in parentheses. The reference category in all models is "before co-authorship started".

Table 7: Moderating Effects of Career Age - OLS

Dependent variable:	Annual citation count (logged)					
Time period of analysis:	2001 to 2016					
Empirical strategy	Focal authors' co-authors as controls		CEM Matching		CEM Matching	
Sample	Psychology		Matched at year preceding focal authors' first year of co-authorship		Matched at 2010	
N treated authors	52		33		31	
N control authors	796		33		31	
	(1)		(2)		(3)	
	point estimate	p-value	point estimate	p-value	point estimate	p-value
Co-authorship started x Pre-discovery	0.478 (0.141)	0.001	0.382 (0.150)	0.013	0.442 (0.197)	0.029
Co-author x Post-discovery	0.643 (0.169)	0.000	0.388 (0.233)	0.100	0.178 (0.273)	0.518
Co-author x Post-discovery x proportion pubs with Stapel	-0.184 (0.019)	0.000	-0.395 (0.107)	0.000	-0.456 (0.106)	0.000
Author fixed effects	✓		✓		✓	
Year fixed effects	✓		✓		✓	
Career age fixed effects	✓		✓		✓	
Author x Year fixed effects	✓		✓		✓	
Observations	11885		838		778	
R-squared	0.911		0.927		0.900	

Notes: Observations are at the authori-yeart level. Robust standard errors clustered at the author level are in parentheses. The reference category in all models is 'before co-authorship started'.

Appendix A: Data Collection

This appendix describes the method used to collect citation and publication data for the set of authors in our dataset. We utilized Scopus as our source of data for two reasons. First, Scopus assigns a unique author identifier to each individual author, which allows us to avoid duplicates in our dataset. Second, Scopus's publicly-available API allowed us to query the database and obtain a large set of data relatively quickly.

The first step in our data collection process was to identify all of Stapel's co-authors (who we refer to as 'focal co-authors'), as well as all of the co-authors of these focal co-authors (co-authors of focal co-authors), who themselves never worked with Stapel. We obtained this information through the use of the Scopus API, in combination with Python bindings. These bindings provide easy-to-use lexical functions in order to obtain data through the API. The combination of these two tools allowed us to obtain the unique author identifiers for all of Stapel's focal co-authors, as well as of the co-authors of the focal co-authors, based on the single input of Stapel's author identifier. However, as Scopus pulls author names directly from publications, this process led to many duplicate entries in our dataset, as there is often variance in how an author's name is spelled on a given publication (take, for example, Diederik Stapel, who can either be listed as Stapel, Diederik, Stapel, D., or Stapel, D.A.) After completing a lengthy manual process of identifying duplicates within the dataset, we were left with 63 focal co-authors and 2,428 co-authors of the 63 co-authors.

Next, we created a script that uses the output described above to query the Scopus API by utilizing the Python bindings for Scopus Author. Based on the unique author identifiers, this script fetches all possible fields of information available in the Scopus Author class. This includes annual publication counts, category, number of co-authors, total citations, and several other pieces of information. We saved all of these records as Python pickle files (one file per author), which took multiple days to run. In order to find annual publication rates for authors, we created a script to render all information about the authors as a single CSV file, by converting information from each individual pickle file into a single CSV.

Our next step was to calculate the annual citation counts for each of the authors. Scopus has a citation report, which is generated at the individual publication level. As we examine citations at the author-year level, our challenge was to sum the data from the publication-level citation reports by author. First, we created a script that uses the same Scopus Author class and Python bindings as above, which allowed us to run multiple citation reports through our query, rather than having to run each individual author's citation report manually. This script writes a text output directly onto the console which enters the Scopus advanced query to ask for the citation reports. When the citation report downloads are completed, they are filtered so that they fall into Scopus's 20,000 item limit. Each file is downloaded and reshaped, and then stacked to create a master CSV file.

Next, we utilized a Perl script to normalize the data. This script applies various manual corrections in order to normalize author names (for example, author names that contain symbols are converted for consistency). The normalization process splits the 'authors' column into the stacked CSV file to create a separate entry for each author, and sums up all of their citation counts by year, while dropping the publication-level data. The list that was generated contained roughly 3,000 names that contained potential duplicates – these entries were manually checked, in order to determine which authors were unique. Duplicates were removed.

Finally, as the citation report we generated does not contain a unique author identifier, our task was to match the citation report with the first CSV containing author-level information. We created a script that searched for matches between the two files based on author name. All matches for which entries were not found were discarded, as these were entries of additional authors on co-authored publications, whom we did not wish to include in our dataset.

The process outlined above is necessary in order to create citation reports at the author level. We realize that this process introduces a chance of both "Type I" and "Type II Error" into our dataset, as the conversion process is imperfect, and may lead us to either mistakenly exclude an author who should be in our dataset from it, or include one who should not be included. However, we deem this chance of error justifiable, as the process allows us to compile a comprehensive citation report of the entire set of focal co-authors and their entire set of co-authors.

Appendix B: Current Status of the Co-Authors

"Meanwhile, all the way down the scale, science, academia, psychology, social psychology, the University of Tilburg, my close colleagues, co-authors, students, and supervisees, have all taken a substantial hit. Colleagues, students, and supervisees have worked for years on experiments and studies whose empirical basis now turns out to be nothing more than dust. Dozens of published papers are going to have to be retracted, removed from their CVs, and apologized for. Some of their best insights and discoveries are revealed to be no more than mirages. The pride with which they defended their theses has gone, shriveled, replaced in many cases with feelings of guilt and shame. "It's unfair!" I want to shout. "I did it. They didn't do anything wrong. They couldn't have known. They worked really hard, in good faith. It's me, only me you should blame."

-Diederik Stapel, Faking Science: A True Story of Academic Fraud

It is an irrefutable fact that Stapel's co-authors incurred great costs following the discovery of his fraud – many of the losses they incurred are immeasurable. For the purposes of this dissertation, we attempted to understand the current career status of each of the 63 co-authors. We succeeded in finding recent information about 60 of the 63 co-authors, by conducting searches through Google and LinkedIn. Unfortunately, we have been unable to determine the current location of the remaining three authors. Of the 60 located co-authors, 53 currently hold positions in academia or medicine. We found that ten of the co-authors have not published any work since the discovery of the fraud. Four of these ten co-authors had been Stapel's students; three were his PhD students, while one was a master's student. The fact that these authors did not publish after the discovery of the fraud may mean that they suffered such great costs following the scandal that they chose to change careers and leave academia behind.

Fifteen of Stapel's co-authors had been his students – fourteen were PhD students, while one was a master's student. Ten of the graduate students completed their PhDs prior to the discovery of the fraud (between 2005 and 2010), and three following the discovery (two in 2011, and one in 2014). Despite the fact that many of their dissertations were based on fraudulent data, they were all cleared of misconduct, and went on to receive their degrees. Twelve of the fourteen PhD students went on to successfully obtain academic careers upon completing their degrees. One chose to complete a second PhD in a different field, while another became an entrepreneur (as did the former master's student). We have included a list of the 63 co-authors, which includes their current institution, in the supplemental materials in table S1. Table S2, meanwhile, presents the ten co-authors who have not published since the year the fraud was discovered.

Of course, it is impossible to understand the full extent of the impact of collaborating with Stapel on the careers of his co-authors, without obtaining information from them firsthand. It would be interesting to find out whether they felt the burden of a stigma on their careers following the scandal, and if so, whether that burden has become lighter over time. We hope to supplement the findings of our current study with a future qualitative study, in which we obtain information directly from the co-authors. We believe that it would of great interest to discuss the strategies they used in order to overcome the hurdles they may have experienced following the scandal.

Appendix C: Robustness Checks with Fixed-Effects Poisson Distribution

As an additional robustness check, we tested our three main effects with a Poisson distribution. The results are shown in tables S3 through S5 below. Table S3 estimates the pre-discovery benefits to fraud, as well as the moderating effects of proportion of publications with Stapel. We find evidence of a significant pre-discovery benefit across three of our four samples, and no significant moderating effect of proportion, just as in the OLS models reported earlier. Table S4 estimates the post-discovery costs of fraud, as well as the moderating effects of proportion. Here, our results are mixed; we find a significant main effect of co-author x post-discovery in only one of our three models. Finally, S5 estimates the net effects of fraud. We find evidence for our main effect across three of the four models, as well as evidence of a significant, positive effect of the proportion moderator. These results add strength to our findings, as they provide evidence against the notion that our findings are a result of features of our chosen empirical model.

S1: Co-author characteristics

Name	Current Institution	Field	Country	Gender (Male = 1)	Total publications*	Total citations*	Publications with Stapel	Coauthor network size	Current position
Avramova, Yana R.	Antwerp	Psychology (all)	Belgium	0	9	36	3	5	PhD student
Blanton, Hart	University of Connecticutt	Social Psychology	US	1	62	2617	3	84	Full professor
Chartrand, Tanya L.	Duke University	Psychology (all)	US	0	52	7546	2	119	Full professor
Dijksterhuis, Ap P.	Radboud University	Social Psychology	Netherlands	1	107	7179	1	144	Full professor
Galinsky, Adam	Columbia University	Psychology (all)	US	1	170	8478	1	239	Full professor
Gordijn, Ernestine H.	University of Groningen	Social Psychology	Netherlands	0	45	965	4	40	Full professor
Hafner, Michael	Universitat der Kunste, Berlin	Social Psychology	Germany	1	25	303	3	42	Senior Assistant professor
In 'T Veld, K.H.R.	Unknown	Psychology (all)	Unknown		2	2	1	3	Unknown
Jansen, Stefanie S.	Stefanie Jansen Kids and Family Insight	Psychology (all)	Netherlands	1	2	2	1	3	Business owner
Johnson, Camille S.	San Jose State University	Social Psychology	US	0	29	259	13	13	Associate professor
Joly, Janneke F.	Saxion University	Social Psychology	Netherlands	0	9	25	4	8	Researcher/teacher
Jordan, Jennifer M.	IMD Switzerland	Psychiatry and Mental Health	Switzerland	1	46	956	1	91	Full professor
Judd, Charles M.	University of Colorado, Boulder	Social Psychology	US	1	125	9890	2	128	Full professor
Klarke, Boor	Leiden University Medical Center	Medicine	Netherlands	0	10	342	2	25	Gynecologist
Ko, Seijin	San Diego State University	Social Psychology	US	1	7	136	1	8	Adjunct/Lecturer
Koomen, Willem	no current CV, not on U of Amsterdam website	Social Psychology	Netherlands	1	78	1317	27	38	Unknown
Lammers, Joris	Cologne University	Social Psychology	Germany	1	37	612	8	29	Assistant professor
Lerouge, Davy	Tilburg University	Social Psychology	Netherlands	1	6	79	2	4	Assistant professor
Lindenberg, Siegwart	University of Groningen & Tilburg	Psychology (all)	Netherlands	1	87	3327	6	77	Full professor
Maringer, Marcus	Wageningen University	Social Psychology	Netherlands	1	9	194	3	8	Researcher/lecturer
Martin, Leonard L.	University of Georgia	Psychology (all)	US	1	48	2086	2	63	Full professor
Marx, David M.	San Diego State University	Social Psychology	US	1	27	712	8	35	Associate professor
Meijers, Marijn H C	University of Amsterdam	Social Psychology	Netherlands	0	8	71	2	11	Assistant professor
Mulder, Henriette	University of Groningen	Psychology (all)Groningen	Netherlands	0	2	19	1	3	Faculty coordinator
Muller, Dominique	University Grenoble Alpes	Social Psychology	France	1	24	1911	2	39	Full professor
Nelissen, Rob M A	Tilburg University	Social Psychology	Netherlands	1	20	495	1	19	Assistant professor
Nelson, Leif D.	Berkeley	Psychology (all)	US	1	33	2348	2	76	Full professor
Noordewier, Marret K.	Leiden University	Social Psychology	Netherlands	0	20	58	9	9	Assistant professor
Norton, Michael I.	Harvard	Psychology (all)	US	1	93	4410	2	147	Full professor
Otten, Sabine	University of Groningen	Social Psychology	Netherlands	0	47	1130	1	69	Full professor
Pollmann, Monique M H	Tilburg University	Psychology (all)	Netherlands	0	13	152	1	26	Assistant professor
Reicher, Stephen D.	University of Saint Andrews	Social Psychology	Scotland	1	13	81	1	17	Full professor
Renkema, Lennart J.	TF Publishing	Social Psychology	Netherlands	1	8	62	3	4	CEO
Ruys, Kirsten I.	University of Amsterdam	Psychology (all)	Netherlands	0	25	445	7	26	Web Developer
Scheele, Fedde	Vienna University Medical Center	Medicine	Netherlands	1	70	1216	2	125	Full professor
Scherpbier, Albert	Maastricht School of Medicine	Medicine	Netherlands	1	311	6002	2	466	Full professor
Schwarz, Norbert	University of Southern California	Psychology (all)	US	1	134	15957	4	150	Provost professor
Schwinghammer, Saskia A.	Wageningen University	Psychology (all)	Netherlands	0	8	17	4	4	Assistant professor
Semin, GvÉ¬ºn Refik	Utrecht University	Social Psychology	Netherlands	1	68	3198	1	110	Full professor
Siero, Frans W.	University of Groningen	Social Psychology	Netherlands	1	35	577	3	56	unknown
Spears, Russell	University of Groningen	Social Psychology	Netherlands	1	178	9499	5	128	Full professor
Stoker, Janka	University of Groningen	Business	Netherlands	0	24	248	3	28	Unknown
Strack, Fritz	Universitat Wurzburg	Psychology (all)	Germany	1	105	7676	1	92	Full professor
Suls, Jerry M.	University of Iowa	Psychology (all)	US	1	129	6391	1	150	Full professor
Tesser, Abraham	University of Groningen	Social Psychology	Netherlands	1	100	3495	3	3	Emeritus professor
Teunissen, Pim W.	Maastricht School of Medicine	Medicine	Netherlands	1	55	947	2	150	Associate professor
Trampe, Debra	University of Groningen	Business	Netherlands	0	9	122	3	12	Market research methodologist
Van De Ven, Niels	Tilburg University	Social Psychology	Netherlands	1	19	305	1	23	Associate professor
Van Den Bos, Arne	University of Groningen	Social Psychology	Netherlands	1	4	10	2	1	Researcher
van der Linde, Lonneke A J G	Unknown	Sociology and Political Science	Unknown	0	2	5	1	1	Unknown
Van Der Pligt, Joop	Passed away in Jan 2015: University of Amsterdam	Social Psychology	passed away	1	141	3045	2	114	Full professor
van der Velde, Sytske W.	University of Groningen	Social Psychology	Netherlands	0	2	7	1	2	Lecturer
Van Der Vleuten, Cees Pm M	Maastricht University	Medicine	Netherlands	1	523	13830	2	679	Full professor
Van Der Zee, Karen I.	University of Groningen	Social Psychology	Netherlands	0	62	1472	1	86	Full professor
Van Diemen-Steenvoorde, J. A A M	Ministerie van Volksgezondheit, Utrecht	Medicine	Netherlands	0	14	191	1	41	Inspector general for health
Van Horen, Femke	VU University Amsterdam	Sociology and Political Science	Netherlands	0	8	70	1	13	Associate professor
Van Yperen, Nico W.	University of Groningen	Social Psychology	Netherlands	1	84	1812	2	87	Full professor
Velthuijsen, Aart S.	University of Amsterdam	Psychology (all)	Netherlands	1	5	83	2	4	Full professor
Visser, Maaike	ING Amsterdam	Psychology (all)	Netherlands	0	2	11	1	2	Consultant
Wiekens, Carina J.	Hanze University, Groningen	Social Psychology	Netherlands	0	6	32	3	1	Lecturer
Winkielman, Piotr	UC San Diego	Psychology (all)	US	1	79	5005	1	110	Full professor
Wood, Joanne V.	University of Waterloo	Social Psychology	Canada	0	62	3089	2	80	Full professor
Zeelenberg, Marcel	Tilburg University	Psychology (all)	Netherlands	1	120	5379	2	150	Full professor

* Through 2016

S2: Co-authors with no publications post-discovery

Name	Year of last publication	Years of Publication	Stapel Student	Current Institution
In 'T Veld, K.H.R.	1998	1998		Unknown
Jansen, S.	1998	1998	✓	Stefanie Jansen Kids and Family Insight - Business owner
Joly, J.	2011	2006-2011		Saxion University
Lerouge D.	2010	2006-2010		Tilburg University
Maringer M.	2011	2007-2011	✓	Wageningen University
Mulder H.	2010	2010		University of Groningen
Renkema L.	2009	2008-2009	✓	TF Publishing - Business owner
van der Linde L.	2011	2011	✓*	Unknown
Van Diemen-Steenvoorde J.	2009	1997-2009		Ministry of Health, Utrecht
Velthuijsen A.	2011	1996-2011		University of Amsterdam

* Indicates masters student

S3: Pre-Discovery Benefits – Fixed Effects Poisson

Dependent variable:	Annual citation count															
Time period of analysis:	2001 to 2011															
Empirical strategy	Focal authors' co-authors as controls				Focal authors' co-authors as controls				CEM Matching				Within-author controls			
Sample	Full				Psychology only				Matched at year preceding focal authors' first year of co-authorship				Entire focal author population			
N treated authors	63				52				33				63			
N control authors	2428				796				33				N/A			
	(1a)		(1b)		(2a)		(2b)		(3a)		(3b)		(4a)		(4b)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
After co-authorship started	0.322 (0.063)	0.000	0.305 (0.065)	0.000	0.270 (0.181)	0.001	0.237 (0.090)	0.009	-0.012 (0.076)	0.877	-0.046 (0.096)	0.631	0.126 (0.054)	0.020	0.148 (0.060)	0.013
Proportion pubs with Stapel			0.818 (0.975)	0.401			1.339 (1.114)	0.229			1.126 (0.783)	0.150			-1.047 (1.197)	0.382
Author fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Year fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Observations	18128		18128		7598		7598		509		509		569		569	

Notes: Observations are at the author$_i$-year$_t$ level. The reference category in all models is "before co-authorship started".

S4: Post-Discovery Costs – Fixed Effects Poisson

Dependent variable:	Annual citation count											
Time period of analysis:	Year co-authorship started to 2016											
Empirical strategy	Focal authors' co-authors as controls				Focal authors' co-authors as controls				CEM Matching			
Sample	Full				Psychology only				Matched at 2010			
N treated authors	63				52				31			
N control authors	2428				796				31			
	(1a)		(1b)		(2a)		(2b)		(3a)		(3b)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-author x Post-discovery	0.031 (0.049)	0.531	0.096 (0.048)	0.044	-0.035 (0.057)	0.542	0.030 (0.056)	0.592	-0.152 (0.093)	0.101	-0.052 (0.104)	0.619
Co-author x Post-discovery x Proportion pubs with Stapel			-1.783 (0.294)	0.000			-1.675 (0.332)	0.000			-0.881 (0.364)	0.015
Author fixed effects	✓		✓		✓		✓		✓		✓	
Year fixed effects	✓		✓		✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓		✓		✓	
Observations	29766		29766		11709		11709		717		717	

Notes: Observations are at the author$_i$-year$_t$ level. The reference category in all models is "co-authorship started".

S5: Net Effects- Fixed Effects Poisson

Dependent variable:	Annual citation count															
Time period of analysis:	2001 to 2016															
Empirical strategy	Focal authors' co-authors as controls				Focal authors' co-authors as controls				CEM Matching				CEM Matching			
Sample	Full				Psychology only				Matched at year preceding focal authors' first year of co-authorship				Matched at 2010			
N treated authors	63				52				33				31			
N control authors	2428				796				33				31			
	(1a)		(1b)		(2a)		(2b)		(3a)		(3b)		(4a)		(4b)	
	point estimate	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*	*point estimate*	*p-value*
Co-authorship started	0.386 (0.074)	0.000	0.326 (0.075)	0.000	0.335 (0.107)	0.002	0.244 (0.123)	0.047	0.274 (0.185)	0.139	0.143 (0.229)	0.534	0.330 (0.110)	0.003	0.175 (0.108)	0.104
Co-author x Post-discovery	0.422 (0.091)	0.000	0.426 (0.092)	0.000	0.306 (0.127)	0.016	0.280 (0.143)	0.050	0.104 (0.211)	0.622	0.037 (0.247)	0.882	0.179 (0.163)	0.272	0.130 (0.171)	0.447
Co-authorship started x proportion pubs with Stapel			2.042 (1.530)	0.182			3.100 (2.053)	0.131			3.413 (2.854)	0.232			2.848 (0.631)	0.000
Co-author x Post-discovery x proportion pubs with Stapel			0.244 (1.581)	0.878			1.412 (2.088)	0.499			2.206 (2.851)	0.439			1.916 (0.736)	0.009
Author fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Year fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Career age fixed effects	✓		✓		✓		✓		✓		✓		✓		✓	
Observations	29948		29948		11841		11841		839		839		781		781	

Notes: Observations are at the author$_i$-year$_t$ level. The reference category in all models is "before co-authorship started".