

Social Comparisons and Deception Across Workplace Hierarchies: Field and Experimental Evidence*

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Benjamin Edelman
Harvard Business School
25 Harvard Way
Boston, MA 02163
bedelman@hbs.edu

Ian Larkin
Anderson School of Management
University of California, Los Angeles
110 Westwood Plaza
Los Angeles, CA 90095
ian.larkin@anderson.ucla.edu

Abstract

We examine how unfavorable social comparisons differentially spur employees of varying hierarchical levels to engage in deception. Drawing on literatures in social psychology and workplace self-esteem, we theorize that negative comparisons cause senior employees to seek to improve reported relative performance measures via deception. In a first study, we use deceptive self-downloads on SSRN, the leading working paper repository in the social sciences, to show that employees higher in a hierarchy are more likely to engage in deception, particularly when the employee has enjoyed a high level of past success. In a second study, we confirm this finding in two scenario-based experiments. Our results suggest that longer-tenured and more successful employees face a greater loss of self-esteem from negative social comparisons, and are more likely engage in deception in response to reported performance that is lower than that of peers.

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Social Comparisons and Deception Across Workplace Hierarchies: Field and Experimental Evidence

Introduction

Social comparisons permeate organizations. Co-workers observe many indications of their colleagues' success, from promotions to favorable assignment of critical resources to praise by superiors. As companies increasingly provide employees with detailed performance feedback, more employees know their individual and relative performance (Prewitt, 2007). There is longstanding evidence that unfavorable social comparisons – when a worker does not experience the signs of success that she sees others receiving – drive a host of negative emotions, including negative self-esteem (Tesser, 1988; Kuhnen and Tymula, 2012), envy (Feather, 1989, 1991), and resentment (Folger, 1987; Weiner, 1986). More recent research has found that negative social comparisons can lead to detrimental actions by employees, including competitive behavior which sometimes destroys value (Garcia et al., 2006), increased absenteeism (Schwarzwald et al., 1992), increased probability of leaving a job (Card et al., 2012), and lower effort provision (Greenberg, 1987; Cohn et al., 2012). Recent laboratory experiments have shown that social comparisons can lead to deceptive actions (Moran and Schweitzer, 2008; Gino and Pierce, 2009a).

Because deception is difficult to observe in the field (Gino and Bazerman, 2009), the literature linking social comparison and deception is largely restricted to laboratory experiments with student subjects. However, social comparisons in the workplace inherently involve self-comparison of an employee and her “referent others” (Festinger, 1954). The hierarchical structure of organizations therefore crucially affects how employees form social comparisons. In the laboratory, it is difficult to replicate the hierarchical structure of organizations—the presence of bosses, middle managers, junior workers, and the like—which hinders understanding of the link between social comparison and deception across organizational hierarchies. Furthermore, laboratory studies on deception usually focus on deceptive acts that spur increased payments to subjects (e.g., Moran and Schweitzer, 2008; Gino and Pierce, 2009a). In contrast, in real organizations, deceptive acts can also serve to increase an employee's status and to

thereby reduce unfavorable social comparison, particularly for measures of performance that are known to peers (Larkin et al., 2012). Put another way, the employee may care about others' perception of her performance relative to peers just as much as or more than her own perception of relative performance.

This study extends the literature on social comparison and deception by examining how the negative effects of social comparison differentially spur deception across organizational hierarchies, and by examining deceptive acts that primarily enhance an actor's status. Following the literature, we hypothesize that unfavorable social comparisons drive deception. However, we consider deceptive acts designed to improve the deceiver's status, which we hypothesize are likely to occur if the performance measure driving the comparison is public. We also hypothesize that this deception is more likely when reported performance is close to a "meaningful standard," defined as a standard associated with both a high rank and an arbitrary cutoff, such as the Fortune 500 list of companies or the US News Top 25 Colleges (Garcia et al, 2006). Considerable research shows that unhealthy competition is more likely when competitor performance is close to meaningful standards (Garcia et al., 2006).

Finally, and most importantly, we build theory on the link between job hierarchies, social comparison, and deception, exploring whether employees high in a hierarchy or low in a hierarchy are more likely to engage in status-enhancing deception. Building on the self-esteem literature, we hold that employees higher in a hierarchy and those with greater past success, whose self-esteem is more dependent on continued success, are more likely to react to unfavorable social comparison by engaging in deception.

We test our hypotheses using both field and experimental methods. In the field study, we use a comprehensive database of paper downloads from the Social Science Research Network (SSRN), the leading online repository of working papers in the social sciences. Because every SSRN paper's cumulative reported download count is reported prominently on each working paper's webpage and in emails sent to many SSRN members, there is evidence that some authors care deeply about their papers' reported download count (Bainbridge, 2007). Furthermore, SSRN has long observed that some authors deliberately download their papers many hundreds or even thousands of times, solely to increase the paper's reported download count (Gordon, 2005). This practice is against SSRN's terms of service and is

clearly recognized by the SSRN community as deceptive (Gordon, 2005). Since we observe deceptive downloads, as well as the position within the academic hierarchy of all SSRN authors, the SSRN download data provide a compelling method by which to test our hypotheses.

We also evaluate the hypotheses around hierarchical level and status-enhancing deception via two scenario-based experiments, one that mimics the SSRN environment and one which uses a completely different workplace environment – players on a professional baseball team hypothetically using a performance enhancing substance. In both scenarios there is a “high status” condition where subjects are asked to consider a deceptive act in light of being a senior and successful employee, and a “low status” condition where the hypothetical employee is of junior status, and considers the same deceptive act.

In both the field and experimental studies, we confirm the findings of recent research that negative social comparisons lead to deceptive acts. The field study also corroborates the importance of the presence of a meaningful standard as a driver of deceptive acts. Most strikingly, the field and experimental studies both suggest that employees higher in the hierarchy are more likely to engage in status-enhancing deception. In the field study, the increase in deception is especially pronounced for employees with a high degree of previous success, and holds even after using a rich set of controls around an employee’s demographic background and career concerns. Interestingly, in the experimental study, subjects in the “high status” conditions did not feel more competitive with peers than those in the “low status” conditions, nor did they think the hypothetical deceptive act was any less deceptive. However, manipulation checks confirm that subjects viewed their status in the hierarchy differently across conditions. This is further evidence that hierarchical status differences drive the differential propensity to deceive across hierarchies.

This multi-method study provides evidence on the link between negative social comparisons and deception by employees with different levels of status in an organization, and suggests that status-enhancing deception is more likely to be undertaken by employees whose status is already high. The next section reviews the theory and hypotheses. We then report the results of the field study, followed by the results of the scenario-based experiment. Finally, we discuss the limitations and implications of the study.

Theory development and hypotheses

Social comparison theory describes the innate desire to evaluate one's own standing or performance via comparisons to peers, rather than in absolute terms (Festinger, 1954). Unfavorable social comparisons occur "when a person lacks another's superior quality, achievement or possession" (Parrott and Smith, 1993). Unfavorable social comparisons have been shown to induce many negative emotions, including shame (Tangney and Fischer, 1995), hostility (Smith et al., 1994), resentment (Parrott and Smith, 1993) and envy (Buck et al., 2004).

Social comparisons have been shown to drive emotional responses only in certain situations. First, tasks that participants consider meaningful and important are required for social comparisons to drive an emotional response (Tesser, 1988; Beach and Tesser, 2000; Tesser and Smith, 1980; Tesser et al., 1988). Second, social comparisons are more likely to lead to negative emotional responses if the process of gauging performance is difficult to understand or seems unfair (Smith et al., 1994; Vecchio, 1995).

Given that deception often occurs due to emotional, contextual triggers, previous research hypothesized that unfavorable social comparisons can lead to deception. Following the previous literature (e.g., Moran and Schweitzer, 2008; DePaulo et al., 1996; Grover, 1997), we define deception as a communication or action that is intended to mislead others.

Because small acts of deception are often carried out without significant forethought (Hegarty and Sims, 1978), decisions to engage in deception often occur due to cognitive factors in response to a contextual trigger (Grover, 1993; Treviño, 1986). Cognitive factors linked to deception include power asymmetries (Tenbrunsel and Messick, 2001), self-deception (Tenbrunsel and Messick, 2004), and competitiveness (Hegarty & Sims, 1978; Garcia et al., 2006).

Laboratory experiments have confirmed the link between social comparisons and deception (Moran and Schweitzer, 2008; Gino and Pierce, 2009a; Gino and Pierce, 2009b). In these studies, researchers induced envious feelings through the introduction of social comparisons, then measured

cheating in hypothetical scenarios and laboratory tasks. All three studies found that social comparison led to increased deceptive behavior.

The tasks used in these experimental settings were designed to be meaningful, and also contained some degree of process unfairness or lack of clarity. For example, Gino and Pierce (2009a) introduced social comparisons by randomly assigning each subject to a high-status or low-status group, and paying a larger show-up fee to the former. Moran and Schweitzer (2008) told participants to imagine that they “put in long hours to increase their chance” of winning a promotion but came in second; participants in this condition reported feeling that the decision was unfair. These studies measured deceptive behavior in different ways. Gino and Pierce (2009a; 2009b) examined cheating when grading performance and thereby receiving higher payoffs, while Moran and Schweitzer (2008) examined responses to a questionnaire around unethical behavior such as making promises that would not be kept.

Another potential reason to engage in deception is that the deceptive act may in itself relieve the unfavorable social comparison. Previous research has shown that subjects act strategically in order to avoid negative social comparisons, for example by giving poor recommendations to peers (Garcia et al., 2010), selectively forming a comparison group (Tesser et al., 1984), or eliminating the possibility of competition with high performing peers (Pillutla and Ronson, 2005). However, existing research has not examined the possibility of deceptive action as a method to lessen negative social comparisons.

Social comparisons often result from perception by others as much as from a person’s own perception of herself (Festinger, 1954; Maslow, 1943). For example, a recent study of grocery checkout staff found that employees who scanned products more slowly did not react when placed behind fast colleagues, even though their position let them see the colleague’s higher speed. However, these employees increased their pace when a fast employee was placed behind them in a position to see their speed (Mas and Moretti, 2009). These results suggest that the costly decision to work harder results from concern about a negative comparison made by others.

We therefore hypothesize that unfavorable social comparisons will cause deception where the deceptive act makes the comparison appear less unfavorable. Specifically, we hypothesize:

Hypothesis 1: Employees with negative social comparisons are more likely to engage in a deceptive act that enhances their own reported quality, achievement, or possession and therefore reduces the reported difference.

In addition, considerable theoretical and experimental evidence suggests that social comparisons are most salient when one compares herself to “similar others” (Vecchio, 1995). Festinger (1954) introduced the concept of a “referent other,” which was formalized (Tesser, 1988; Beach and Tesser, 2000) and tested in experiments (Tesser et al., 1988). Recent research has shown that when a person’s self-esteem is threatened because a similar person has high abilities, the person tends to take actions to protect his or her self-image by taking steps to prevent the similar person from succeeding (Garcia et al., 2010).

There is a gap in the current literature on the link between reference groups and deception, likely because laboratory experiments lack a clear means to introduce salient groups of referent others (Gino and Pierce, 2010). However, the literature strongly establishes that social comparisons cause more emotional reactions when the comparison is made by similar peers (Tesser et al., 1988), so it is natural to think that peers are more likely to engage in deception when facing unfavorable comparisons with those similar to them, particularly when the deceptive act itself helps alleviate the difference in comparisons. This leads to our second hypothesis:

Hypothesis 2: Employees are more likely to engage in status-enhancing deceptive acts when the unfavorable social comparison is between similar employees, and less likely when the unfavorable social comparison is between dissimilar employees.

Recent work has linked social comparisons to competition (Garcia et al., 2006; Garcia and Tor, 2007). This work demonstrates that social comparisons tend to most engender competition when the comparisons are close to a “meaningful standard” (Garcia et al., 2006). A “meaningful standard” is one associated with a high rank, but also includes rankings close to an otherwise arbitrary cutoff, such as the 499th biggest company in the Fortune 500 (Garcia et al, 2006). Competition that occurs due to meaningful standards is often detrimental, because it can lead to non-cooperative, value destroying behavior. For example, Garcia et al. (2006) introduced a number of scenarios to experimental subjects, such as being at risk of exclusion from the list of Fortune 500 corporations -- e.g., a corporation currently ranked #500 or #501 -- versus a control condition where there was no risk of exclusion -- e.g., companies with a rank of #350 or #351. These papers indicate that experimental subjects are more likely to behave competitively, and in so doing reduce the amount of value created, when reaching to achieve a meaningful standard.

Competition has been shown to produce both positive and negative emotions depending on the context. Negative emotions such as disappointment, frustration, and anger are more likely consequences of competition when expectations are higher than actual achievement (McGraw et al., 2005), and when competition occurs between an actor who feels threatened by another’s status or control over resources (Fiske et al., 2002). By their nature, meaningful standards represent a potential discontinuity between status levels and perceptions of achievement (Garcia et al., 2006), and one might hypothesize that employees at risk of exclusion from the standard are therefore likely to experience negative emotions. When a person is near a competitive meaningful standard, the psychological costs of deception are likely to be smaller than the benefits of avoiding the negative emotions from being excluded from the standard:

Hypothesis 3: Employees are more likely to engage in status-enhancing deceptive acts when their reported performance is close to a meaningful standard.

Another facet common to nearly all organizations is the presence of organizational hierarchies (Blau, 1968). It is natural to ask whether such deception is equally likely for all levels within a work hierarchy, or whether employees systematically differ in their propensity for this form of deception by their hierarchical level. Above, we hypothesize that status-enhancing deception counters the negative self-esteem that stems from unfavorable social comparisons, so we again turn to the self-esteem literature to build hypotheses about the link between hierarchical status and deception.

The link between an employee's standing in the workplace and her self-esteem is well established (Tharenou, 1979). Employees tend to exhibit highest self-esteem in areas where they have enjoyed past success and are likely to be successful in the future (Coopersmith, 1967; Branden, 2001), a factor which applies most to employees senior in a given hierarchy. Employees with greater initial success have been shown to suffer degraded performance after a loss in status, consistent with previously-successful employees feeling a greater threat to self-efficacy after a decline in status (Marr and Thau, 2014). Research has shown that social comparisons are especially salient along the dimensions where a person has greatest familiarity and where the person has already enjoyed strong performance and success (Garcia et. al., 2010). Recent research shows that employees with a history of strong performance on a given task react to the granting of a corporate award to employees who lack this track record by exerting less overall work effort (Gubler et al, 2014). Individuals also tend to overestimate their ability in areas where they have a high degree of familiarity (Moore and Cain, 2007). This overconfidence may cause employees who are high in a hierarchy to react to negative social comparisons by attributing them to unfair or unclear processes (Smith et al., 1994; Vecchio, 1995). Because advancement in a hierarchy usually requires success, and because social comparisons are more salient in areas where actors enjoy existing success (Garcia et al., 2010), those higher in a hierarchy may react to the negative self-esteem caused by unfavorable social comparisons by deceiving. Formally:

Hypothesis 4: Employees higher in a workplace hierarchy are more likely to engage in status-enhancing deceptive acts than employees lower in a workplace hierarchy.

There are, of course, reasons to believe the opposite of Hypothesis 4 – that employees lower in a workplace hierarchy are more likely to engage in status-enhancing deception. Most notably, lower-ranked employees may compete for scarce resources such as promotions or career-enhancing status within an organization. To empirically test Hypothesis 4, it is important to control for these “career concerns” reasons for deception.

The line of reasoning behind Hypothesis 4 raises an additional corollary – that more successful employees within a given hierarchical level will be more likely to react to negative social comparisons in the area of success by deceiving:

Hypothesis 5: More successful employees in a given job task are more likely to react to negative social comparisons regarding that task by deceiving.

Hypotheses 4 and 5 stem from the same logic – that employee self-esteem is highest in areas of success, and therefore the highest cost to self-esteem from negative social comparisons is likely for more successful employees.

We test these hypotheses in two studies, one using field data and one using scenario-based experiments.

Study 1: Field study of deceptive downloading on SSRN

Our first study examines deceptive downloads on the leading online repository of academic working papers in the social sciences, the Social Science Research Network (SSRN). SSRN was founded

in 1994 to distribute early-stage research “at the lowest possible cost for authors and readers” (Jensen, 2012). SSRN has nearly 200,000 members, largely drawn from academia, and has in its history hosted nearly 400,000 working papers which are downloaded over 8 million times a year (Jensen, 2012). SSRN offers networks which loosely correspond to traditional social science disciplines (e.g., Economics, Law, Finance, Political Science), and each network contains a number of e-journals roughly corresponding to fields within each discipline (e.g., Development Economics, Economic History, Labor Economics). Working papers are not peer reviewed, and e-journal editors assess subject matter fit, not merit.

Every paper submitted to SSRN receives a public web page presenting the article title, author(s), and abstract as well as statistics on popularity. Notably, a paper’s page reports how many times the page itself has been viewed (called an “abstract view” because the abstract is included on the paper’s web page), how many times the paper has been downloaded, and the paper’s overall download rank among all papers on SSRN.

In addition, many e-journals provide a “Top Downloads” page that lists the ten most downloaded papers in the journal (the “All Time Top 10 List”), as well as a “Recent Hits Top 10 List” reporting popular papers first announced within the last 60 days.

Substantial anecdotal evidence indicates that many scholars and institutions pay attention to reported SSRN download counts. Scholars have developed ratings of institutional research prowess based on download counts which correlate highly with other measures (Black and Caron, 2006). Furthermore, Sierra (2007) reports that some institutions use SSRN download counts to evaluate faculty job candidates. Faculty often report download counts via blogs and other social media. For example, a prominent legal academic reported on his blog that “SSRN download counts are like crack to me” (Bainbridge, 2007). There is also evidence that scholars care about their download counts compared with peers. In an article entitled “Now Star Professors Get Their Ratings Too,” the *New York Times* explored academics’ interest in reported download counts on their SSRN papers, asking “Would you believe that academics could become caught up in such petty, vain competition?” (Cohen, 2008).

Because many participants care about download counts, SSRN attempts to provide a meaningful measure of how often a paper has been downloaded. Among other criteria, SSRN excludes downloads by automatic software (such as search engine crawlers), and SSRN attempts to exclude multiple downloads by the same user. SSRN's terms of service specifically prohibit attempts to manipulate download counts.

SSRN reports that it spends "significant sums of money on sophisticated systems to identify both repetitive downloading by individuals and potentially fraudulent download patterns over time" (Gordon, 2005). SSRN does not publicly disclose its specific methods for identifying deceptive downloads because this information might help perpetrators find ways to exploit the system. However, SSRN did share with us many of its methods on the condition that we keep them confidential. To prevent manipulation of its data, SSRN uses methods that go well beyond a simple monitoring of a user's Internet Protocol (IP) address and logon information. SSRN's methods have become both more accurate and more strict over time, including identifying patterns that tend to indicate deceptive downloads. Importantly, SSRN retained detailed information about historical downloads in order to assess deceptive downloads that occurred before implementing updated methods. As a result, some perpetrators "got away" with inflating their download counts in the first instance, yet we can nonetheless identify their deceptive downloads thanks to the more sophisticated anti-manipulation methods that SSRN developed later. Our data therefore allow us to observe deceptive behavior that often occurred repeatedly and over a long period of time, since authors downloading their own papers likely believed no one noticed their behavior.

Data and empirical methodology

SSRN provided us with records of all downloads of SSRN working papers between 2001 and late 2007. Records included identifiers for each paper, the author(s), the SSRN networks and e-journals to which the paper belonged, the exact date and time of each download, and SSRN's judgment of whether the download was questionable according to SSRN's latest, strictest rules.

We restricted our analysis to authors with at least 200 total downloads across all their SSRN papers. We also discarded papers that were not downloaded, legitimately or illegitimately, at least 10

times in a single month in their history on SSRN. We limited the sample in this way to focus on papers achieving a baseline level of popularity. These restrictions left us with 71,567 working papers by 15,397 authors.

In order to measure an author's overall career success, we also gathered data from Google Scholar as to total citations for each author's 100 most highly-cited papers as of September 2010. The median author has Google citations on fewer than 40 papers. For authors with citations on more than 100 papers, the average 100th most cited paper has less than 1% of the citations of the most cited paper. (Authors with more than 100 cited papers tend to have at least a few papers with extremely high citation counts.) Therefore, little bias results from restricting this data to an author's top 100 cited papers.

Portions of our analysis are limited to a randomly-selected sample of SSRN authors. We gave research assistants (RAs) the names of 1,004 authors who had written 4,765 papers. This random sample therefore represents about 6.5% of the total papers and authors in the database. The RAs used Internet search engines to search for an author's home webpage and/or CV. They then compared the institutional affiliation listed by SSRN with the information on the author's webpage or CV, to ensure an exact match.

The RAs then filled out a custom data entry tool that resembled a standard resume. Specifically, the RA entered information from the webpage and/or CV on the author's educational and work history, including names of institutions, titles of degrees and positions, and relevant dates, directly into a tool that had fields for each of these areas. The tool had the ability to accept as lengthy an educational or work history as the author provided on a webpage or CV. RAs also looked for any listing of author nationality and gender; the latter (but not the former) included pictures on webpages where, in the RA's view, gender was clearly identifiable. The RAs were instructed not to use any information beyond an individual webpage and CV. Therefore, social networking sites, news sites, blogs, and the like were not included. Unknown to the RAs, we assigned approximately 10% of authors to multiple RAs, as a spot check for quality. The entered information for this 10% overlapped almost completely, confirming the accuracy of data collection.

The RAs ceased looking for information if none was found within 15-20 minutes. The RAs managed to find at least some resume information on nearly 80% of the authors in the database, and found full resumes for more than 50%. These 1,004 authors and their papers formed a dataset we call the “resume sample.”

We took steps to protect the privacy of SSRN authors. We assigned every author an identification number, and we omitted author names from the data we analyzed. Our RAs, who found authors’ resumes and coded resume information, had access to the names of individual authors, but they could not view data about paper downloads or deception, and they did not know the subject of our study. Our procedures prevented anyone, including us, from connecting download data with a particular author.

For tractability and to build a useful measure of deception (discussed further below), the unit of analysis in our dataset is the author-paper-month. We grouped individual paper downloads into monthly totals of genuine downloads and of downloads SSRN determined should not be counted (both for benign reasons and because the downloads resulted from deception). Because our unit of analysis is the author-paper-month, not the paper-month, a paper appears in our data multiple times if it has multiple authors. In the resume sample, a paper appears multiple times if more than one author on the paper was among the 1,004 authors in the resume sample.

Table 1 reports summary statistics of download and paper data for the full set of papers and for the resume sample. The resume sample is similar to the full sample, confirming that no bias resulted from randomization.

As noted in Table 1, the average paper is downloaded slightly more than nine times per month. Of these, slightly more than two are tagged by SSRN’s filters as failing requirements for inclusion in the paper’s download count. The average paper is downloaded nearly 400 times in its history on SSRN.

INSERT TABLE 1 HERE

Measuring deception

Our analysis requires identifying and measuring deceptive downloads. SSRN's download records give one imperfect measure of deception: whether a download should have counted in the paper's reported total under SSRN's latest and most stringent criteria. However, this measure also captures several benign instances of download inflation. For example, authors and others may misplace electronic or printed papers, and by design SSRN is an easy and convenient tool for retrieving papers. We therefore adjusted SSRN's download measure to better focus on deception.

We considered using SSRN's detailed download data to build an alternate measure of deceptive downloads. We rejected this approach because SSRN has spent considerable time and money on a system to identify questionable downloads, giving SSRN a substantial advantage at this task.

Instead, we used SSRN's existing analysis to construct alternative measures indicating the extent to which an author engaged in deceptive downloads of a given paper in a given month. In rare cases, some other person may have caused the deceptive downloads; however, SSRN's investigations reveal that the author herself is by far the most likely source of the self-download, and we therefore consider all deceptive downloads to have originated from the author. Rather than focus on the individual download, our measures reflect the extent to which SSRN deemed a high percentage of the paper's downloads to be questionable in a month. Our rationale is that a few questionable downloads per month could reflect benign factors, but multiple, repeated instances of questionable downloads are highly likely to reflect deception.

We built six separate measures of a monthly "deception" variable with increasingly strict definitions of what constituted "deceptive" downloading behavior that month. Table 2 summarizes our approach to building these alternate measures. We formed two separate categories of deception measures: a continuous measure of the extent of deception in a month, calculated as an adjusted proportion of downloads flagged as questionable by SSRN; and a binary measure of whether a given author-paper suffered significant questionable downloads in a given month.

For both the continuous and the binary deception variables, we formed three variants of each measure (“strict,” “loose,” and “baseline”) with restrictions as detailed in Table 2. As the names indicate, the measures differ according to the extent to which some “questionable” downloads are considered benign. Table 3 shows the proportion of author-paper-months flagged as deceptive under the various criteria.

INSERT TABLE 2 HERE

In the econometric specifications in the following section, we used all six of the deception estimates presented in Table 2. Coefficient estimates across the alternative deception measures were remarkably similar in magnitude. In the “strict binary” case, which is the most conservative, a few explanatory variables begin to lose statistical significance, but remain borderline. The six measures are also highly correlated. Since the estimates were so similar across definitions of deception, we report only the “baseline” estimates for the continuous and binary measures.

INSERT TABLE 3 HERE

As noted in Table 3, deceptive behavior on SSRN is relatively rare. By our baseline binary measure, only 2.5% of author-paper-months are associated with deceptive downloads. Our baseline continuous measure indicates somewhat more deception, as 5.2% of the average author-paper-month’s downloads are deemed to be deceptive. However, the distribution has a long right tail, with deceptive downloads reaching 50% or more for some author-paper-months. The median author-paper-month has 2.1% of downloads classified as deceptive.

Measuring peer groups and social comparisons

All five of our hypotheses relate to the success of an author’s peer group on SSRN. To examine these hypotheses, we build three notions of peer groups.

First, we consider departmental peers—other authors who listed the same department as their primary affiliation on their SSRN author page. Table 1 reports some summary statistics on departmental peers, finding that the average author in the full sample had over 220 department peers. However, this is skewed by outliers. The median department has only 28 peers. Outliers occur because a few institutions do not report detailed departmental affiliations on SSRN. In these cases, all scholars at the institution are classified in the same “department.” For example, “Harvard Law School” has approximately 200 peers. However, most data are reported at the department level, such as the “Harvard Business School - Negotiation, Organizations and Markets Unit” which has approximately 15 peers. These differences in reporting levels do not bias our results; dropping large or very small departments from the analysis does not change the results in any meaningful way.

A second potential referent group is peers who do similar research, whether at the same institution or (more often) elsewhere. Within SSRN’s structure, these peers are identifiable because they tend to publish in the same SSRN e-journals. We therefore form a notion of “SSRN e-journal peer” defined by author-papers within the same e-journal. To focus on contemporaneous work, we limit each paper’s e-journal peers to other papers that enter the e-journal within 60 days of the paper in question.

A final potential referent group is an author’s co-authors. Co-authors are highly likely to work on similar research, and are therefore a natural reference group for authors. We therefore repeat peer analysis using a peer group given by all of an author’s co-authors. Since an author cannot reasonably experience a negative social comparison towards a coauthor on a project the author and coauthor performed together, we discard from this analysis any papers on which the author worked with that co-author.

Recent theoretical and empirical work in economics (e.g., Shue, 2013; Card et al., 2012) suggests that using mean effects to proxy for peer effects across the distribution not only has robust theoretical properties, but also effectively correlates with alternate measures of peer success, such as the success of the most successful peer group member, or success of the median member. We therefore focus on mean peer success, although the results are robust to using median peer success.

These variables allow us to test Hypothesis 1: whether deception is correlated with peer success. To test Hypothesis 2—that deception is likely to occur when “similar peers” succeed as opposed to other peers—we build measures of peer success that are limited to peers whose SSRN profiles report the same title (e.g., “Assistant Professor”).

Several challenges result from our use of job titles presented on SSRN. First, not all institutions or authors report job titles on SSRN. Second, we only observe an author’s job title as of 2007, the end of our dataset. For these reasons, we focus our analysis for Hypothesis 2 on authors with a reported title, and we only consider the last 24 months of data, because few authors change titles within this timeframe. Results are robust to using other cutoff dates. Table 4 summarizes these data.

INSERT TABLE 4 HERE

Hypothesis 3 holds that competition and social comparison are more likely to occur around a measurable standard. To test this hypothesis, we recorded whether an author-paper was on a “border position” in any journal with a Recent Hits Top 10 List. The Recent Hits Top 10 List represents a “meaningful standard” because inclusion on such a list is associated with a large increase in a paper’s visibility, both on SSRN’s website and via emails SSRN sends to e-journal subscribers. We define a paper as positioned at a border position if the paper that month ranks between 8 and 12 in total downloads among papers eligible for the Recent Hits Top 10 List in any journal with such a list. Less than 1% of all author-paper-months are in this position.

Hypothesis 4 suggests that employees higher in the hierarchy are more likely to engage in deception due to social comparisons. Hypothesis 5 examines the link between career success and deception. To test these hypotheses, we focus on the interaction of job title and peer effects, and Google Scholar citations and peer effects. We also carry out subsample analysis on full professors only, and compare the results from this subsample to the results on a subsample of associate and assistant professors.

Controls

Previous work has linked several other factors to deception, most notably economic, demographic, and organizational antecedents of deception. The “rational” or economic model of deception posits that committing fraud or engaging in other unethical behavior is a rational choice based on costs and benefits (Becker, 1968). As noted in the theory section above, it is logical to think that employees junior in the hierarchy would enjoy larger economic benefits from engaging in status-enhancing deception. To control for the economic and other career concerns of authors, we focus on three events: tenure, job promotion, and job mobility. If an author faces an impending tenure decision or promotion decision, or plans to change institutions, the author may see increased economic returns to making her papers more visible on SSRN. In the resume sample, summary statistics of which are reported in Table 5, we observe substantial job mobility, both across and within institutions. Within a 2-year period, 14% of authors changed institutions, and over 8% changed positions within the same employer, almost all of which were promotions. In any given month, about 5% of authors were assistant professors with four years or more of tenure at the month in question, suggesting they were likely coming up for a promotion decision in the next few years. About 3% of authors were assistant professors with less than two years of tenure. Despite this mobility, in a given month, on average an author had worked at an employer for over 4 years, and had the same title for over 6 years. Although we would ideally distinguish between voluntary departures and involuntary ones, for example those stemming from tenure denials, it is impossible to infer from the data at hand when authors choose to leave and when they are forced to.

INSERT TABLE 5 HERE

We also note that Hypothesis 4 in effect provides a test of whether employees who have fewer career concerns are more likely to engage in deception. Using full professors to test this hypothesis provides another check against the alternative explanation of career concerns, since full professors

arguably have fewer economic or career concerns than their junior colleagues, especially when controlling for mobility.

The “demographic” model of deception holds that actor background explains propensity to deceive (Hegarty and Sims, 1978). Controlling for demographic characteristics is straightforward. In the resume sample of 1,004 separate authors, we examine several measures of an author’s background, as noted in Table 5. Nearly 10% of the resume sample identified themselves on their resumes or via a website picture as being female, and 10% identified themselves as having non-U.S. citizenship. The actual numbers are surely higher, biasing downwards any effect we find for these variables. At the time of download, one third of authors were full professors, and about 10% each were associate and assistant professors. About a third of authors were affiliated with a school ranked in the top 50 by Shanghai Jiaotong University’s Academic Rankings of World Universities list, a widely-used measure of university quality. About 20% of authors were affiliated with schools ranked lower than 250 on this list.

As noted, we could not find resumes for about 20% of authors. To avoid selection bias, we retained these author-papers throughout our analysis, but we included a “missing” dummy variable for each category. Table 1 indicates that the sample statistics for the resume sample are close to those of the full dataset, suggesting that the authors without online resumes are similar to other authors as to the characteristics we measured.

Our final category of control variables addresses institutional culture, which has been shown to influence deception (e.g., Treviño et al., 1998; Treviño and Weaver, 1998). First, in some specifications we add institution- or department-level fixed effects. If a particular institution has a culture that makes its members more prone to deceptive downloading behavior, this behavior would be absorbed into the fixed effects. In other specifications we build measures of the average number of deceptive downloads by department peers. If deceptive behavior by peers spills over to other peers at the same institution, we would expect an author whose peers engage in deceptive downloads to herself engage in more deceptive downloading.

Regression framework

Our data represent an unbalanced panel of 77 months of SSRN paper downloads. We used this panel to estimate the determinants of deceptive self-downloads. In each analysis, our dependent variable is one of the six measures of deception reported in Table 2.

Our specific regression equation is the following:

$$D_{it} = \beta_0 + \beta_1 * I_t + \beta_2 * P_{it} + \beta_3 * S_t + \beta_4 * A_t + \beta_5 * (P_t * A_t) + \varepsilon_{it}$$

where:

- i is the author-paper
- t is the month
- I is a set of paper characteristics
- P is a measure of the success of peer papers on SSRN (or Google Scholar citations)
- S is a dummy variable representing whether the paper is on the border position on a top-10 list
- A is a set of author characteristics, including Google Scholar citations
- ε is the error term.

We controlled for other factors which might affect deceptive downloads. Key controls included the following:

- Paper popularity as measured by “legitimate” downloads, since authors may be more or less likely to deceptively download their popular papers. We also included the square of this measure.
- The network(s) of the paper in question, since different disciplines may have different rationales for inflating downloads. For example, law journals are edited by students, which might make SSRN download counts more important for judging paper quality.

- The paper’s “age” as measured by time since release on SSRN, since downloads for most papers occur soon after submission.
- A dummy variable representing whether the paper had more than one author, since the presence of more than one author might decrease a potential downloader’s concern about being detected, and gives multiple actors the chance to engage in deception.

Our analysis groups each month’s data, but authors can react on an instantaneous basis to changes in peer paper downloads or to their presence near a Top 10 List. To account for delays in our view of author response, we lagged two sets of variables: the social comparison variables represented by average peer downloads, and the presence of a paper near a Top 10 List. In alternative specifications we did not lag these variables, and we also tried lagging the variables by two months. Results did not change qualitatively.

We ran a series of robustness checks. In one specification we used monthly growth rates rather than levels on key variables. In another we amended the binary variable on a deceptive month to account for “major” and “minor” deception. We also ran the analysis on a sub-sample of single-author papers only. In these and several other robustness checks, our results did not change significantly.

Importantly, our approach escapes many of the problems common in empirical studies of deception. Most of our explanatory variables are indisputably exogenous from the decision to engage in deception, and it is difficult to think of situations where reverse causality is a problem. For example, it is unlikely that deceptively downloading on SSRN causes an author to become a full professor (since such promotions reflect a robust review process), and it is implausible that deceptive downloads on SSRN cause the papers of an author’s peer group to become more popular. Similarly, there is no reason to believe that SSRN’s enforcement of download counting rules was correlated with an author’s position, the popularity of an author’s peers on SSRN, or any of our other explanatory variables. We also benefit from the ability to apply SSRN’s latest anti-manipulation technology to historical download counts, reducing worries about detection versus commission. Omitted variable bias and measurement error are

the two largest concerns facing our approach, but we believe we created reasonable proxies and alternative variables, and our results are highly robust to alternative specifications. Finally, the existence of “Top 10” lists provides an exogenous discontinuity in the returns to deception.

Our estimation procedure varies based on the dependent variable and sample. For the binary deception variable in the “resume sample,” we use a random effects logit model. For the binary deception variable in the full sample, we use a Generalized Least Squares (GLS) probability estimator because the logit model would not converge. Linear probability estimators have been shown to yield results similar to maximum likelihood estimators in most circumstances, especially with large samples (Imbens, 2007). We use a standard GLS random effects model for the continuous deception variable in both samples.

Results: full sample analysis

Table 6 presents an evaluation of Hypothesis 1 (deception due to peers’ success) and 3 (deception induced by proximity to a meaningful standard) using the full sample. Models (A) and (B) show basic results for the binary and continuous measures of deception, respectively. Average lagged department peer downloads and e-journal peer downloads are highly correlated with deceptive downloads, as is being on the border of a Top 10 List. For example, in the binary model a one standard deviation increase in lagged average peer success increases the probability of an author-paper-month meeting the definition of “deceptive” by 29%, lending support to Hypothesis 1. Proximity to a Top 10 List increases this probability by about 8%, lending support to Hypothesis 3. We also tested for co-author peer effects, but these were never significant.

Models (C) and (D) add controls for the institutional environment by introducing a measure of lagged average deceptive downloads by department peers as well as department-level fixed effects. Department peer deceptive downloads are not at all correlated with deceptive downloads by an author. Adding institutional fixed effects significantly increases the explanatory power of the model, as noted by the large increases in the R-squared statistic. However, the key results on peer and Top 10 effects do not

change with this addition. Department-specific idiosyncrasies appear to explain a portion of deceptive downloads, but these effects are largely orthogonal to peer and Top 10 effects.

INSERT TABLE 6 HERE

We find that the rate of deceptive downloads varies dramatically between SSRN networks. Papers in networks associated with professional schools—Finance, Law, Management, and Accounting—are associated with significantly more deception than papers associated with the Economics Network.

To examine Hypothesis 2 (deception due to success by “similar” peers) and Hypothesis 4 (hierarchy and deception), we add information on author position as of the end of the sample. As noted previously, we limit this analysis to the final 24 months of the sample since our data on author position for the full sample is limited to a single observation at the conclusion of the data. We also drop authors for whom no title is listed on SSRN (but include non-professors with listed titles, such as “lecturer;” this represents the excluded category in the regressions). While these restrictions exclude nearly 75% of the dataset, over 850,000 author-paper-month observations remain. We also add a variable representing the quotient between an author’s Google citations and that of the median department peer. A number above one therefore represents an author with more Google citations than her average department peer. For brevity, we do not report paper-level controls in Table 7, as they are very similar to the effects reported in Table 6.

INSERT TABLE 7 HERE

As shown in Table 7, we find support for both Hypotheses 2 and 4. In models (A) and (B), we find a positive and significant interaction between full professor and the average number of downloads by department peers who are also full professors. This suggests that full professors react to “similar peers” in terms of rank. However, neither associate nor assistant professors appear to react significantly to

“similar peers.” In the binary model, a one standard deviation increase in the average downloads of peers leads to a 27% higher likelihood that a given author-paper-month will be classified as deceptive.

This finding supports Hypothesis 4: full professors, who are higher in the hierarchy than associate or assistant professors, are more likely to engage in deception. These findings also hold for e-journal peers; full professors react more than others to the popularity of authors publishing papers in the same SSRN e-journals around the same time. Interestingly, baseline levels of deception are not significantly different across titles. It is only when peers have popular papers that full professors sharply increase their use of deception.

Professors with higher Google Scholar citations than their median peers are also significantly more likely to engage in deceptive downloading. Authors who are one standard deviation more cited than their median peers are 11% more likely to engage in deception. Citations are an independent (but also publicly visible) measure of success, so this result supports Hypothesis 5.

Table 8 reports the binary model from Table 7 on two sub-samples of data: full professors; and assistant and associate professors. Full professors are more widely observed in the data than the other two categories combined, which is why we combined the latter two into a single category. Rather than use interaction effects, we introduce two levels of department peers: peers at the same level, and peers at different levels. Again, we do not report controls in the tables for simplicity, but they were similar to those in Table 6.

The results of Table 8 suggest that full professors are motivated to inflate their download counts in response to both department peers and peers largely outside the department. In contrast, deception by assistant and associate professors is not correlated with department peer success. Assistant and associate professors do deceptively download in response to e-journal peers, but the effect size on e-journal peers is statistically smaller than that of full professors at the 5% level. Furthermore, full professors are more likely than other professors to deceive when close to a Top 10 List, and when they are successful in terms of Google Scholar citations. Put together, there is significant evidence that full professors are

differentially driven to deceive based on peer comparisons, and that success in other dimensions also leads to greater deception.

INSERT TABLE 8 HERE

We ran all the reported tests in Tables 6, 7, and 8 on sub-samples of single-authored papers only. The results were nearly identical, suggesting co-authored papers are not driving the key results. This rules out the possibility that authors engage in deception only when responsibility for the act could plausibly fall on another party (i.e., the co-author or co-authors). It also rules out the explanation that full professors engage in increased deception only to help their junior co-authors or colleagues.

Tables 6, 7, and 8 indicate that peer effects are largely individual-specific, and are not explained by institutional or departmental effects. Because only full professors seem to be differentially affected by peer effects, it seems that career concerns are not driving the peer effects. However, the resume sample contains several more specific measures of career concerns, allowing us to further validate that the peer effects are not driven by economic concerns. Using the resume sample also lets us control for author characteristics beyond title at the time of download.

Results: resume sample analysis

Table 9 presents analysis of the authors for whom we coded detailed resume data. The findings track those for the full sample. There is evidence of strong peer effects which are particularly pronounced among full professors in reaction to the success of full professor peers, and the success of e-journal peers.

There is also further evidence of deception near the cutoffs of the Top 10 Lists. The data we collected on job title over the entire 77-month sample allow us to examine which authors respond most to proximity to a Top 10 List. In the binary model, we again find that full professors are statistically more likely to engage in deception in that circumstance, while the interaction is not significant for assistant or associate professors.

INSERT TABLE 9 HERE

Finally, the resume sample corroborates our full-sample finding that more highly cited authors engage in more deceptive downloading, even when controlling for other variables likely to be correlated with high citation counts such as future mobility or affiliation with a top 50 school. Coefficients and effect sizes drop by about a third, but are still highly significant.

We do not find strong evidence of career concerns leading to deception. Authors that change institutions in the subsequent 24 months are not more likely to engage in deceptive downloads, nor are authors who change job title within the same institution. Interestingly, assistant professors who likely face an upcoming tenure review are associated with significantly fewer deceptive downloads. Similarly, demographics do not appear to play a major role.

Overall, the resume sample further confirms that, even with improved controls for career concerns and demographics, peer success strongly predicts deceptive downloads. Furthermore, the resume sample confirms that these effects are strongest among full professors, who are more likely than others to engage in deceptive downloads when institutional peers of similar rank have success, when peers across institutions publish papers in the same e-journals, and when their papers are close to the borderline of a meaningful standard.

Study 2: Scenario-based experiment of Hypotheses 4

Hypothesis 4, which holds that employees higher in a hierarchy are more likely to engage in status-enhancing deception due to unfavorable social comparisons, is the most novel of our hypotheses. To validate Study 1's support of this hypothesis, we ran a series of scenario-based experiments to validate the finding that employees higher in the hierarchy are more likely to engage in such deception.

Methods

As is common with scenario-based experiments, we used a series of surveys run on Mechanical Turk, an Internet marketplace that allows “Requesters” to hire “Workers” for tasks. Mechanical Turk has been widely used for experiments in the social sciences, and “Workers” on the platform have been shown to respond to classic experiments such as the Prisoner’s Dilemma in ways that are highly similar to traditional subject pools (Paolacci et al, 2010). We limited our subject pool to “Workers” based in the United States.

Our study used a between-subjects design, and asked subjects to respond to a series of questions after reading a brief scenario. The scenarios only differed in their description of the subject’s place within the job hierarchy. We used two settings for the scenarios. The first (Study 2a) closely mimicked the SSRN environment. We chose this setting so we could carefully design the scenario to test whether hierarchical differences drove the effects found in Study 1. However, we also felt it was important to test Hypothesis 4 in a separate job environment. Because Study 2a necessitated a rather long introduction to hierarchies in academia and the possibility of deception, we wanted to choose a work setting where experimental subjects were already likely to be familiar with the work hierarchy and deception. We therefore chose professional baseball for the second study (Study 2b).

In both scenarios, we first introduced the hierarchy. The key passages for the two studies were the following:

Study 2a (all participants): You are an economics professor. The academic world is extremely competitive, with many tenured professors who are seen as leading authorities in their subjects, and many new assistant professors with new ideas hoping to make names for themselves. In general, professors feel most competitive with other professors in their same discipline who have similar experience and success to themselves.

Study 2b (all participants): You are a professional baseball player. Within the baseball profession, players have a wide range of experience, from players in their

first year of the league to players with many years of experience. Players tend to feel most competitive with players on other teams at a similar level of experience.

After the description of the hierarchy, we next implemented a “high status” and “low status” condition, where the subject was told where she stood in the hierarchy in the condition:

Study 2a (high condition): You are a tenured professor in economics at a leading university.

Study 2a (low condition): You recently completed a graduate degree and are now an assistant professor at a leading university.

Study 2b (high condition): You are in your tenth year playing major league baseball.

Study 2b (low condition): You are in your first year playing major league baseball.

Finally, we set up the potential deceptive act, which did not differ by condition:

Study 2a (all subjects): In economics, most professors post their research papers on an online database for others to read. The website publicly tracks how often each paper has been downloaded. A list of the “Top Ten” most downloaded papers is posted on the site’s home page and emailed to professors every week. Although the number of downloads is not important to a professor’s career, many professors see it as a sign of how much peers value their research. Because of this, many professors download their own papers thousands of times in order to increase their download counts and appear on the “Top Ten” list. While the site does not prohibit or identify self-downloads, the activity is considered deceptive and is frowned upon by universities and other professors alike. You have posted a paper on the website that you spent two years working on and which you are very proud of. Recently, you notice that another professor at a similar point in their career as you has posted a paper on the same topic as yours. This professor’s paper is being downloaded much

more often than yours. You have no idea if this professor is engaging in self-downloading, but you do know that the paper has received many more downloads than yours and will likely appear on the site's "Top Ten" list.

Study 2b (all subjects): You know that many other players take testosterone, a natural substance, to improve their performance. While the league has banned the use of testosterone, taking it is not illegal. Natural substances like testosterone do not have any negative side effects on health, and there is no chance you will be detected. You spent the entire offseason training and working out in order to prepare for the season. You notice that another player who has been in the league the same number of years as you is performing much better than you are. You have no idea whether this player is using testosterone, but you do know that this player is batting better than you this year.

We then asked subjects four questions, which did not vary across condition:

Study 2a (all subjects): How likely are you to download your own paper multiple times in response to the success of the peer's paper?

Study 2b (all subjects): How likely are you to take testosterone in response to the success of the player on the other team?

Study 2a (all subjects): How much status do you feel you have in the economics profession?

Study 2b (all subjects): How much status do you feel you have in the baseball profession?

Study 2a (all subjects): How competitive do you feel with the other professor whose paper is being downloaded more than yours?

Study 2b (all subjects): How competitive do you feel with the other baseball player whose batting is better than yours this year?

Study 2a (all subjects): How deceitful do you feel it is to download your own paper?

Study 2b (all subjects): How deceitful do you feel it is to take testosterone?

All questions were asked on a standard 7-point scale. Subjects were paid \$1.50 for completing the survey. We also asked two “reading comprehension” checks, one containing a simple math problem, and one asking subjects to leave a question blank. Only a single subject failed these checks, and the results do not differ when including or excluding this subject. Study 2a contained 60 subjects in the “high” condition and 68 in the “low” condition; the corresponding numbers for study 2b were 75 and 83. The number of subjects is not equal in the four conditions because the survey software we used contained a single link on which all subjects clicked, after which they were randomly assigned to one of the four conditions.

Results

In both scenarios, high-status subjects reported a higher likelihood of choosing to deceive in their given scenario. In Study 2a, the average answer to the deception question in the “high” condition was 3.65 (SE=0.25), while in the low condition it was 2.93 (SE=0.24). Using a simple two-sided t-test of equality of means, these two figures are significantly different at the $p=3.8\%$ level. The corresponding statistics in Study 2b were 3.13 (SE=0.22) and 2.48 (SE=0.19), which are significantly different at the $p=2.7\%$ level. The proportion of subjects saying their likelihood of deceiving was at least 3 out of 7 is similarly higher for “high” condition subjects. In Study 2a, the proportion of “high” condition subjects giving at least a 3 on this question was 68.3% (SE=6.1%), while in the “low” condition it was 55.1% (SE=6.1%); in a test of proportions, these are significantly different at the $p=5.7\%$ level. In Study 2b, the corresponding statistics were 54.7% (SE=5.8%) and 36.1% (SE=5.3%), which are significantly different at the $p=1.9\%$ level.

In both Study 2a and 2b, subjects differed in their perceived status within the profession. In Study 2a, subjects in the “high” condition reported status of 4.73 out of 7 (SE=0.16), vs. status of 4.26

(SE=0.14) in the “low” condition. The p-value on the t-test statistic of equality of means is 3.4%. In Study 2b, the corresponding figures are 5.18 (SE=0.16) and 4.63 (0.17), with a p-value of 2.7%. This manipulation check confirms that the hierarchical differences presented in the scenarios led subjects on average to perceive status differences.

However, subjects in the “high” and “low” conditions did not feel differentially competitive with peers. In Study 2a, the means for the competitiveness question in the “high” and “low” condition were 5.41 and 5.42, respectively, while in Study 2b they were 5.83 and 5.86, respectively. The p-values on t-tests of means are above 90% in both cases. Similarly, subjects in the “high” conditions did not report considering the act to be more deceitful. In Study 2a, the means for the question about the degree of deceitfulness were 5.77 (SE=0.16) and 5.40 (SE=0.19), $p=17\%$. In Study 2b, the means were nearly identical – 5.51 and 5.54, with a p-value above 90%.

Discussion

Our results confirm the importance of social comparisons in the workplace, and provide the first evidence that the impact of negative social comparisons differs across work hierarchies. Employees of higher status and success are more likely to react to unfavorable comparisons with peers by engaging in deception designed to make the comparisons less unfavorable. This finding has important practical and theoretical implications for scholars and managers of organizations.

Our findings are robust both within and across studies. In Study 1, the correlations between peer success and deception hold even after introducing a rich set of controls for career concerns, demographics, and institutional culture. Study 1 also represents one of the first large-scale investigations of deception and peer comparisons in the field, drawing on SSRN’s maintenance of data on deceptive downloads during times where deceivers may not have known they were being observed.

In addition, Study 1 in particular contributes to a stream of literature that investigates decisions made by academics in order to better understand behavior of general employees. For example, Haas and Park (2010) examine how peer groups and the attitude of superiors influence academics’ attitude to

withhold information from others, in violation of professional norms. Martison et al. (2005) conduct a series of surveys about serious misconduct by academic scientists to address questions of fraudulent behavior, such as falsifying data. Multiple studies examine patenting behavior by university scientists (e.g., Azoulay et al., 2007; Henderson et al., 1997). Because many academics put their vitas on public web pages, academic careers are uncommonly easy to follow, which has facilitated study of hiring and mobility decisions (e.g., Oyer, 2006). Like these studies, our research involves a key element of an academic's job—writing papers—and examines behavior—the act of deception—that is of broad interest to scholars of organizations.

Study 2, which finds that experimental subjects given scenarios where they were in positions of high status indicate they are more likely to engage in deceptive behavior than subjects in low status positions, corroborates the impact of hierarchy on negative social comparisons and deception. This study provides evidence that hierarchical status differences, and not differences in perceived competitiveness or judgments around deceptiveness of the act, drove the differences in subjects' reported likelihood of engaging in the deceptive act. These results corroborate the findings of Study 1 that employees higher in a work hierarchy are more likely to react to negative social comparisons by engaging in status-enhancing deception. Of course, our null result on competitiveness does not rule out the hypothesis that differential competitiveness led to higher deception, since participants may have been reluctant to admit feeling competitive, or may not have felt as competitive as they would actually feel had they experienced the scenario.

Limitations

There are several limitations to this work, particularly to Study 1. The most fundamental is that, despite building theory that relied on the significant literature showing the negative emotional responses stemming from unfavorable social comparisons, we have no information about the actual emotional state of academics who engaged in deception on SSRN, and we did not test for differential emotional response

in the scenario-based experiments. As explained in the theory section, a large number of affective emotions could explain our results, including envy, disappointment, anger, and feelings of unfairness. We see our results as consistent with the recent experimental literature that links envy and deception (Moran and Schweitzer, 2008; Gino and Pierce, 2009a; Gino and Pierce, 2010), but we cannot definitively say that envy or any other emotion led to the observed results.

Similarly, we cannot measure the complete career impact of higher SSRN download counts, and we do not know authors' exact beliefs about what this impact might be. We introduced some proxies around career concerns, but there are numerous ways by which academics might compete besides promotions and institutional moves. For example, there is likely competition over salaries, research budgets, grants, student admissions, and myriad other categories. We note, however, that these concerns are likely to hold across all levels of the hierarchy, while our results did not. Relatedly, it is difficult to think of non-observed career concerns that correlate with our findings on hierarchy and previous success. Still, absent an exogenous shock to social comparisons, a large-sample study cannot definitively prove that social comparisons cause deceptive behavior. Two recent studies that used exogenous shocks to social comparisons (Mas and Moretti, 2009; Card et al., 2012) are both consistent with our results, particularly because both found that social comparisons affect emotions as well as subsequent employee actions. Although we find that more highly-tenured and successful employees are more likely to engage in deception, it is important to remember that deception is still an infrequent act. In the field study, for example, the vast majority of full professors do not choose to engage in deception, even when faced with negative social comparisons. It would be interesting and important to extend this research to examine the within-tenure variation in propensity to deceive.

The scenario-based experiments (Studies 2a and 2b) put non-specialist subjects into a hypothetical situation, where it may be implausible to expect subjects to reasonably feel similar to an actual employee within a hierarchy. Also, subjects may not be able to accurately infer the actions they might take in a given scenario, or may be reluctant to disclose what they would do in a situation. As such,

these experiments are best viewed as supporting Study 1, the field study, which examined actual decisions by employees in a setting highly familiar to them.

A final limitation of our paper is the uncertain generalizability of its findings outside of industries such as academia (Studies 1 and 2a) and professional sports (Study 2b). However, firms increasingly provide firm-wide feedback on the performance of individual workers, particularly in competitive functions such as sales (Larkin, 2014). This feedback is often and increasingly given in social comparison terms (Blanes i Vidal and Nossol, 2011). Furthermore, the increased use of online systems to measure and track worker performance could mean that electronic recordkeeping systems like SSRN become even more commonplace, both within and across firms. Indeed, General Electric already has an advanced online tool that includes performance evaluations, some of which are given in relative terms (Kumar and Rangan, 2011).

Theoretical Implications

The results of this paper have several important implications for understanding the role of social comparisons in organizations. First, the results suggest that more comprehensive theories are needed as to the benefits and costs to firms of providing performance feedback. Much of the empirical work, including this paper, considers a narrow set of benefits (such as increased motivation) or costs (such as increased deception), and most of the theoretical literature considers social comparisons from the point of view of the individual, not the organization. There are some recent exceptions (Card et al., 2012; Larkin et al., 2012; Larkin, 2010), but the literature offers practitioners little insight about when and how to best use social comparisons. Our results suggest that their use may carry larger costs than many organizations realize, since deception is commonly unobserved by firms.

Our results also imply that social comparisons apply unequally across work hierarchies as well as individual workers. Based on the theory developed in this paper, we speculate that negative social comparisons engender a strong emotional response, which directly leads to deception. This hypothesis is intuitive given the strong link between self-esteem and past success, but in the organizational context,

most existing theory concentrates on the unfavorable social comparisons for “lower” employees compared to “higher” ones in the hierarchy. Our results suggest that it is also important to look within hierarchy and to consider differential effects between levels of hierarchy. Furthermore, the results suggest social comparisons potentially interact with success in pernicious ways, which opens many theoretical and practical questions.

In particular, scholars in multiple disciplines have long argued that high-functioning organizations should reward success. However, our results suggest that successful workers may increasingly rely on continued success as a source of workplace self-esteem, and that threats to this self-esteem could lead to harmful choices by successful employees. Our results suggest a need for better theories of the costs and benefits of rewarding high performers, especially as technology makes comparisons with peers easier and more prevalent.

Finally, our results indicate that despite the emotional response engendered by unfavorable social comparisons, employees can react to them in quite “rational” ways, in this case by reducing the apparent difference in performance via deception. Much of the existing literature is focused on “biased” reactions such as engaging in non-cooperative behavior (Garcia et al., 2006) or taking actions that harm one’s self out of guilt (Gino and Pierce, 2010). Of course, it could be that SSRN authors “forget” that they ever inflated their paper’s download count, which would represent a bias. However, we believe it is more likely that authors primarily inflate their downloads because of their appearance to others, an interpretation which is corroborated by other large-sample work (Mas and Moretti, 2009).

Implications for practice

The academic literature on the use of social comparisons in organizations presents managers with limited insight due to mixed findings and a lack of clear guidelines about how and when to introduce comparisons with peers in the workplace. Our research adds an important dimension to this practice question, indicating that managers should consider the role of hierarchy and past organizational success when deciding on the optimal use of social comparisons.

Most directly, our results suggest that the use of social comparisons is risky for successful, highly-tenured employees. But it is exactly these employees who managers likely find most appropriate to use as examples for others. While further empirical work is needed, our results suggest that managers should consider the potential effect on peers of using high-tenure employees as examples to motivate others. Specifically, drawing comparisons to successful highly-tenured employees risks an adverse response from other highly-tenured employees whose recent performance has decreased in comparison to their more successful peers.

Our research may also help managers hone their intuition on who is most likely to engage in fraud. Some might expect that newer, more junior employees are most likely to engage in fraud because they have the most to gain from this behavior. While this may be true from the perspective of career concerns, our research demonstrates that the largest psychological benefit from deception, especially the relatively small acts examined in Study 1, occur for senior employees in an organization.

More broadly, our results may be of interest to those who design the reports and tabulations that often serve as the basis for “meaningful standard” comparisons. In modern organizations, such reports are often automatically-generated “top 10” lists and similar reporting tools. Reduced IT costs invite managers to design ever-more “top” lists, and often the details of these lists are left to software designers with limited insight into management and organizations. Our results flag the risk of misbehavior for those near the boundaries of such lists, an important but previously-little-noticed consequence of these seemingly-routine functions.

Future Directions

Although deception is not commonly observed, we see an important role for further work exploring the link between social comparisons and deceptive behavior in real-world settings. Further investigations would test the generalizability of the findings in our work and allow for tests of more nuanced theories, particularly around the role of hierarchies, past success, and emotions. Researchers in the accounting literature commonly use measures of accounting restatements and unexplained accruals,

both of which are quite common, as indicators of fraudulent behavior by executives, and full compensation data are available for the most highly compensated executives at all public firms. So, too, are data on social networks, due to the explosion of online networking sites such as LinkedIn. Scholars in social psychology and other fields have recently collected data on academics who engage in questionable research practices (e.g. Simonhson et. al, 2014), and our data and results suggest a host of opportunities to explore further the phenomenon of academic misconduct.

The emotional and other mechanisms underlying this paper's results are also a promising and very important avenue for future research, given the dearth of studies that combine field observations with data on emotional response. For example, organizations may want to respond differently to deception caused by envy versus deception caused by perceived unfairness. Both factors have been shown to lead to deception, but organizations might counteract the former by reducing available information on peers, and might address the latter by making the procedure by which performance is measured clearer or fairer. These organizational responses have differing costs and effectiveness, which underlies the importance of fully understanding the underlying mechanism that links unfavorable social comparisons to deception.

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Tables

Table 1: Summary Statistics of Download and Paper Data

Unit of observation is the author-paper-month.

VARIABLE	Full sample (N=3,144,628)				Resume sample (N=207,596)			
	Mean	Std Dev	Min	Max	Mean	Std Dev	Min	Max
# downloads	9.07	199.82	0	171,977	9.40	29.58	0	2,834
# DLs which are questionable	2.21	194.35	0	171,832	2.09	12.12	0	2,346
% of questionable downloads	0.119	0.178	0	1	0.118	0.176	0	1
Total paper downloads	373.15	766.70	0	45,042	413.21	1160.22	0	43,744
Total SSRN papers by author	23.04	29.12	1	242	21.20	22.44	1	149
Sole-authored paper†	0.24	0.43	0	1	0.24	0.42	0	1
Paper has two authors†	0.41	0.49	0	1	0.39	0.49	0	1
Paper has ≥3 authors†	0.35	0.48	0	1	0.37	0.48	0	1
Month published in SSRN	7/2002	27 mos.	5/1997	10/2007	9/2002	27 mos.	5/1997	10/2007
Proportion of papers in Economics Network†	0.52	0.50	0	1	0.55	0.50	0	1
Finance Network†	0.48	0.50	0	1	0.50	0.50	0	1
Law Network†	0.30	0.46	0	1	0.28	0.45	0	1
Management Network†	0.17	0.38	0	1	0.18	0.38	0	1
Accounting Network†	0.11	0.31	0	1	0.09	0.28	0	1
Number of networks	1.71	0.86	1	7	1.72	0.86	1	6
Avg downloads of other papers in e-journal	3.43	1.77	0	72.83	3.38	1.77	0	35.14
Avg downloads of e-journal peer papers entering SSRN within 60 days of paper's entry	3.73	2.83	0	260.86	3.71	2.81	0	77.52
Number of author affiliations listed on SSRN website	1.34	0.71	0	7	1.42	0.77	0	4
Number of department peers	227.62	452.34	0	2,642	277.20	513.34	0	2,496
Avg downloads of department peers' SSRN papers	2.77	3.91	0	1,555	2.80	4.43	0	933
Number of papers published on SSRN by coauthors	1.92	18.77	0	845	0.94	10.87	0	264
Avg downloads of coauthors' other papers	0.05	0.50	0	35.94	0.08	0.84	0	67.98
Paper is on border position within a "new paper" Top 10 List†	0.009	0.09	0	1	0.009	0.09	0	1
Number of papers on GS*	64	498	0	7,160	62	445	0	7,160
Total GS citations*	650	669	0	31,988	634	652	0	31,988
# of GS papers w/citations*	39	37	0	100	38	36	0	100
Total GS citations of median department peer*	1,018	309	0	7,872	991	298	0	7,872

†: Dummy variable. If the corresponding attribute is true, then the variable is coded as 1.

* GS = Google Scholar. Citations represent total of top 100 cited papers. Google Scholar data are reported at the median, not the mean, due to the large effect of outliers.

Table 2: Alternate Deception Measures – Definitions

	Calculation	Rationale
Continuous variable measures		
Baseline	SSRN’s reported % of questionable downloads (DLs) if the paper had more than 10 total DLs in a given month; otherwise 0	Avoids characterizing a paper as subject to questionable DLs if the paper had few DLs.
Loose	SSRN’s reported % of questionable DLs	SSRN’s baseline assessment of questionable DLs, without further modification.
Strict	SSRN’s total questionable DLs minus 5, divided by total DLs	Grants each paper five free DLs tagged as questionable, per month.
Binary measures		
Baseline	≥ 25 DLs; % of questionable DLs ≥ 0.25	Attempts to compromise between having false positives and false negatives.
Loose	Sliding scale starting at minimum of 10 monthly DLs and 80% questionable DLs, up to 250 monthly DLs and 5% questionable DLs	Identifies more questionable downloads, but may have false positives.
Strict	Sliding scale starting at minimum of 25 monthly DLs and 50% questionable DLs, up to 250 monthly DLs and 10% questionable DLs	Avoids false positives, but overlooks some questionable downloads.

Table 3: Alternate Deception Measures

Unit of analysis is author-paper-month.

	Full sample (N=3,144,628)		Resume sample (N=207,596)	
	% questionable	Std Dev	% questionable	Std Dev
Continuous measures (% of questionable downloads)				
“Baseline”	0.052	0.124	0.053	0.123
“Loose”	0.119	0.178	0.118	0.176
“Strict”	0.014	0.066	0.014	0.066
Binary measures (1 if an author-paper-month is classified as “questionable”)				
“Baseline”	0.025	0.156	0.026	0.160
“Loose”	0.039	0.201	0.035	0.182
“Strict”	0.010	0.099	0.008	0.090

Table 4: Summary Statistics for Author Data from “Last 24 Month Sample”

Unit of observation is author-paper-month.

N=897,108

VARIABLE	Mean	Std Dev	Min	Max
Average downloads of papers of department peers who are full professors (for author-paper-months associated with full professors)	1.86	4.51	0	905
Average downloads of papers of department peers who are associate professors (for author-paper-months associated with associate professors)	0.87	2.16	0	413
Average downloads of papers of department peers who are assistant professors (for author-paper-months associated with assistant professors)	0.73	1.94	0	617

Table 5: Summary Statistics for Author Data from “Resume Sample”

Unit of observation is author-paper-month.

N=207,596*

VARIABLE	Mean	Std Dev	Min	Max
Female author	0.09	0.29	0	1
Non-U.S. citizen (includes dual citizens)	0.09	0.29	0	1
Has Ph.D.	0.75	0.43	0	1
Has J.D.	0.10	0.31	0	1
Has M.B.A.	0.13	0.34	0	1
Has M.S. or M.A.	0.43	0.49	0	1
Has other Masters degree (M.P.P., M.D., etc)	0.03	0.18	0	1
Has non-academic work experience	0.27	0.44	0	1
Is a full Professor ¹	0.33	0.47	0	1
Is an Associate Professor	0.13	0.34	0	1
Is an Assistant Professor	0.10	0.30	0	1
-- with <2 years of tenure	0.03	0.17	0	1
-- with >4 years of tenure	0.05	0.22	0	1
Is a Post Doc	<0.005	0.01	0	1
Is a Ph.D. student	0.02	0.15	0	1
Is another kind of student (M.S., college, etc)	0.01	0.10	0	1
Is at a top 50 school	0.34	0.47	0	1
Is at a school outside the top 250	0.20	0.39	0	1
Changes employer within the next 24 months	0.14	0.34	0	1
Changes title within the next 24 months	0.08	0.28	0	1
Length of time with current employer (months)	53.9	75.0	0	560
Length of time with current title (months)	76.6	98.3	0	548

* We could not find full information for all authors in the “resume sample.” We found some background information for 79% of authors, and full resumes for 56%. When information is missing, it is coded as such so that any bias goes against finding an effect.

¹ This variable and all subsequent variables are measured as of the time when the paper was downloaded.

Table 6: Full sample results (without interactions)

Sample Dependent deception variable Specification N	(A)	(B)	(C)	(D)
	Full Baseline binary Random effects GLS [^] 3,126,175	Full Baseline continuous Random effects GLS [^] 3,126,175	Full Baseline binary Random effects GLS [^] 3,126,175	Full Baseline continuous Random effects GLS [^] 3,126,175
Peer effects				
Average department peer downloads _{t-1}	.003 (.001)**	.007 (.002)**	.003 (.001)**	.006 (.002)**
Average department peer deceptive downloads _{t-1}			. -0.001 (.002)	.000 (.002)
Average e-journal peer DLs in papers released within 60 days _{t-1}	.006 (.001)**	.170 (.049)**	.005 (.001)**	.156 (.052)**
Proximity to meaningful standard				
Paper on border position in journal with Top 10 List _{t-1}	.289 (.112)**	.0780 (.031)*	.303 (.149)*	.081 (.034)*
Paper Characteristics				
Legitimate downloads	.061 (.010)**	.302 (.070)**	.057 (.016)**	.275 (.084)**
Legitimate downloads squared	-.00004 (.00000)**	-.00001 (-.00000)**	-.00003 (.00001)**	-.00002 (.00000)**
Paper with multiple authors	.188 (.094)*	.065 (.021)**	.145 (.073)*	.046 (.012)**
Months since SSRN release	-.030 (.009)**	-.055 (.015)**	-.059 (.016)**	-.070 (.018)**
Economics Network	-.221 (.090)*	-.336 (.168)*	-.285 (.071)**	-.445 (.157)**
Finance Network	.395 (.110)**	.600 (.151)**	.406 (.105)**	1.18 (.261)**
Legal Network	.207 (.107)*	.456 (.245)	.141 (.068)*	.358 (.195)
Management Network	.274 (.120)**	.303 (.124)**	.210 (.070)**	.285 (.128)**
Accounting Network	.316 (.110)**	.270 (.061)**	.406 (.155)**	.315 (.147)**
Department fixed effects	N	N	Y	Y
R-squared	.105	.065	.178	.103

[^] Standard errors are robust and clustered at the author-paper level.

** and * represent significance at the 1% and 5% levels, respectively.

Table 7: Full sample results (with interactions)

	(A)	(B)
Sample	Last 24 months	Last 24 months
Dependent deception variable	Baseline binary	Baseline continuous
Specification	Random effects	Random effects
	GLS [^]	GLS [^]
N	861,045	861,045
Peer effects		
Average department peer downloads _{t-1}	.002 (.001)*	.004 (.001)**
Average e-journal peer DLs in papers released within 60 days _{t-1}	.006 (.002)**	.135 (.052)**
Proximity to meaningful standard		
Paper on border position in journal with Top 10 List _{t-1}	.304 (.113)**	.101 (.037)**
Author characteristics		
Full professor	.046 (.020)*	.081 (.033)**
Associate professor	.013 (.011)	.012 (.015)
Assistant professor	.049 (.040)	.099 (.090)
Relative Google Scholar citations	.081 (.029)**	.117 (.399)**
Interactions		
Department same peer DLs _{t-1} * Full	.006 (.002)**	.038 (.012)**
Department same peer DLs _{t-1} * Associate	-.004 (.003)	.001 (.002)
Department same peer DLs _{t-1} * Assistant	.003 (.004)	.035 (.025)
E-journal peer DLs _{t-1} * Full	.008 (.003)**	.049 (.014)**
E-journal peer DLs _{t-1} * Associate	.002 (.003)	.002 (.002)
E-journal peer DLs _{t-1} * Assistant	.010 (.009)	.067 (.050)
Department fixed effects	Y	Y
Paper characteristics (as in Table 6)	Y	Y
R-squared	.184	.104

[^] Standard errors are robust and clustered at the author-paper level.

** and * represent significance at the 1% and 5% levels, respectively.

Table 8: Sub-Sample Analysis

	(A)	(B)
Sample	Full Professors	Asst/Asstc Professors
	Last 24 months	Last 24 months
Dependent deception variable	Baseline binary	Baseline continuous
Specification	Random effects	Random effects
	GLS [^]	GLS [^]
N	276,420	208,546
Peer effects		
Department same peer downloads _{t-1}	.003 (.001)**	.001 (.000)
Department other peer downloads _{t-1}	-.002 (.002)	-.001 (.002)
Average e-journal peer DLs in papers released within 60 days _{t-1}	.011 (.004)**	.006 (.003)*
Proximity to meaningful standard		
Paper on border position in journal with Top 10 List _{t-1}	.451 (.148)**	.213 (.104)*
Author characteristics		
Relative Google Scholar citations	.014 (.043)**	.048 (.027)
Department fixed effects	Y	Y
Paper characteristics (as in Table 6)	Y	Y
R-squared	.183	.162

[^] Standard errors are robust and clustered at the author-paper level.

** and * represent significance at the 1% and 5% levels, respectively.

Table 9: Resume sample results

Sample Dependent deception variable Specification N	(A)	(B)
	Resume Baseline binary Random-effects logit [^] 206,687	Resume Baseline continuous Random effects GLS ^{^^} 206,687
Peer effects		
Average department peer downloads _{t-1}	.003 (.001)**	.005 (.002)**
Average e-journal peer DLs in papers released within 60 days _{t-1}	.006 (.0003)*	.133 (.040)**
Proximity to meaningful standard		
Paper on border position in journal with Top 10 List _{t-1}	.228 (.112)*	.092 (.027)**
Author characteristics		
Relative Google Scholar citations	.059 (.027)*	.081 (.395)*
Change institution in next 24 mos.	.064 (.035)	.063 (.024)*
Change job title in next 24 mos.	.030 (.028)	.042 (.031)
Full professor	.031 (.014)*	.015 (.007)*
Assistant professor	.006 (.005)	.003 (.002)
4 yr. + assistant professor	-.401 (.149)**	-.613 (.241)**
Less than 4 yr. assistant professor	.066 (.042)	0.052 (.045)
At top 50 school	.158 (.166)	.131 (.116)
At school ranked 51-250	.282 (.160)	.175 (.090)
At school ranked 250+	-.201 (.190)	-.058 (.031)
Non-U.S. citizen	-.254 (.119)*	-.222 (.142)
Female	-.117 (.175)	-.104 (.059)
Has a Ph.D.	.189 (.121)	.018 (.013)
Professional degree holder	-.038 (.065)	-.020 (.114)
Professional work experience	-.039 (.075)	.028 (.020)
Interactions		
Department same peer DLs _{t-1} * Full	.008 (.004)*	.044 (.021)*
Department same peer DLs _{t-1} * Associate	-.003 (.003)	-.013 (.024)
Department same peer DLs _{t-1} * Assistant	.011 (.009)	.069 (.046)
E-journal peer DLs _{t-1} * Full	.002 (.001)*	.055 (.021)**
E-journal peer DLs _{t-1} * Associate	-.000 (.001)	-.004 (.004)
E-journal peer DLs _{t-1} * Assistant	.002 (.002)	.002 (.002)
Top 10 border _{t-1} * Full	.091 (.043)*	.051 (.027)
Top 10 border _{t-1} * Associate	-.037 (.038)	-.011 (.018)
Top 10 border _{t-1} * Assistant	.133 (.097)	.146 (.098)
Department fixed effects	Y	Y
Paper characteristics (as in Table 6)	Y	Y
Log-likelihood	-14715	
Wald chi2(44)	4115.4**	
R-squared		0.247

[^] Coefficients represent marginal effects evaluated at the variable mean.

^{^^} Standard errors are robust and clustered at the author-paper level.

** and * represent significance at the 1% and 5% levels, respectively