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Fraud and Innovation

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Abstract

We show that fraudulent firms allocate resources differently than honest companies. Resources obtained through fraudulent means are likely to be viewed as unearned gains and are less likely to be invested in productive activities, such as recruiting talent. We posit that honest and fraudulent companies also invest in different types of innovation: honest firms pursue technically significant innovations, while fraudulent companies are likely to make smaller investments in less challenging inventive opportunities that contribute to the appearance rather than the substance of innovation. We test these predictions in a longitudinal dataset tracking the personnel recruitment and patenting activities of 467 Chinese high technology firms, all of which applied for state-funded innovation grants. We identify fraud by comparing two sets of financial books prepared by each company in the data in the same fiscal year, which are legally required to be identical but are discrepant in over 50 percent of cases, in a direction that benefits the firm. We find that relative to honest companies, fraudulent firms are more likely to receive state grants and are less likely to recruit new employees or produce important inventions in the post-grant period.

Keywords: fraud, innovation, state subsidy, emerging markets

We ask two questions in this paper. First, compared with honest actors, are organizations that fraudulently acquire resources more likely to divert them from their intended purposes? Second, conditional on resource infusions, are fraudulent companies more or less innovative than honest firms?

To date, the theoretical and empirical literatures have been generally silent on organizations' deployment of resources acquired through fraudulent means. Theoretically, the literature suggests that the means of resource *acquisition* is likely to influence how moneys are *allocated*, which implies that in allocation decisions, fraudulently acquired resources will be treated differently than legitimately obtained funds. Another unresolved theoretical debate in the innovation literature concerns whether actors who break rules are more innovative,

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because their willingness to disregard convention opens a broader and less-explored solution space.

We might want to believe that deliberate acts of corporate deception are rare events, but evidence suggests that fraud is common. Dichev et al. (2013) surveyed the chief financial officers (CFOs) of 169 U.S. public companies and found that 20 percent had engaged in earnings management. Using forensic accounting, Stuart and Wang (2016) found that more than half of a sample of Chinese technology firms deliberately misreported their financial results. Studies have documented that many companies have backdated executive stock options (Heron and Lie, 2009), engaged in price fixing (Darby and Karni, 1973), and curtailed health benefits for retirees (Briscoe and Murphy, 2012). Palmer and Yenkey (2015) studied teams of athletes who consumed performance-enhancing drugs. Moreover, fraudulent conduct has been observed across many geographic areas and stages of market development, such as the U.S. (Pierce and Snyder, 2008; Yue, Luo, and Ingram, 2013), Kenya (Yenkey, 2015, 2018), Korea (Jeong and Siegel, 2018), and Sweden (Jonsson, Greve, and Fujiwara-Greve, 2009).

The ubiquity of fraudulent corporate conduct belies the paucity of literature on the subject in organization theory. A small group of organizational scholars has directly studied fraud, and there is significant interest in the general subject of organizational misconduct (Jonsson, Greve, and Fujiwara-Greve, 2009; Sharkey, 2014; Aven, 2015; den Nieuwenboer, da Cunha, and Treviño, 2017). Still, there is scant empirical work on fraud, especially in large samples (exceptions include Pierce and Snyder, 2008; Yenkey, 2015, 2018). This lacuna in the literature exists neither because of a lack of scholarly interest in the subject nor because of a lack of theoretical applicability; many branches of organization theory are of central relevance to the phenomenon of organizational misconduct (Greve, Palmer, and Pozner, 2010; Palmer, Smith-Crowe, and Greenwood, 2016). We believe that the primary impediment to developing a literature on corporate misconduct is one of data (un)availability: the very fact that perpetrators of fraud attempt to conceal their actions poses a challenge to methodologically valid work on the problem (Greve, Palmer, and Pozner, 2010).

The research design and unique data we have collected for this paper allow us to directly observe most acts of fraud in a relatively large sample of organizations. We do this by tracking misrepresentations of corporate financial information rather than relying on publicized cases of fraud that has been discovered—what we label ''caught-fraud.'' In this context, we can directly observe the underlying actions of fraudulent firms, despite the fact that fraudulent actors are rarely detected. We can then compare the innovation-related inputs and outputs of firms that legitimately acquire resources with those that do so through illegitimate means. Moreover, we can observe companies' investment decisions, which allows us to investigate differences in the deployment of resources between honest and fraudulent firms at a granular level. To our knowledge, we are addressing novel research questions with a novel research design. Until now, the literature has not carefully examined how firms utilize fraudulently acquired gains.

Our sample is 467 early-stage technology firms in five regions in China, all of which applied for an innovation grant. For this group of companies, we observe for the entire sample (1) whether the company misrepresented its financial statements in disclosures to state agencies, which defines fraud; (2) whether

the company was awarded an innovation grant; (3) whether firms invested in hiring R&D and other employees; and (4) outputs of investments in innovation, in the form of different types of patent filings.

Fraud appears to pay in the context of China's state innovation subsidy program. More than 50 percent of firms in our sample committed fraud based on our definition, and among fraudulent firms, 60 percent received a state grant. In comparison, 47 percent of honest firms received a grant. Because a greater share of state resources is allocated to fraudulent companies, important questions follow regarding the utilization of state-provided resources. Do fraudulent grant winners allocate funds as intended, such as to hire R&D personnel and to invest in innovation projects? More generally, to what extent do fraudulent companies behave differently than honest grant-winning firms in the post-grant period?

We argue that fraudulent firms and honest companies are likely to allocate resources to different uses. To test the central prediction, we rely on a difference-in-difference-in-differences (DDD) regression framework for identification. The differences in the dataset are: honest versus dishonest companies; before and after a grant application is submitted; and winning a grant versus being denied the grant. The econometric setup compares the resource allocation decisions of fraudulent versus honest firms that are ''treated'' when they receive state-funded innovation grants. Both the descriptive statistics and the regression results establish that there are significant differences in resource allocation by type of firm: fraudulent companies appear to be much less likely to allocate funds to the intended, productive purposes.

FRAUD AND INVESTMENTS IN INNOVATION

We begin with an assumption to structure our theorizing: there are two types of fraudulent organizations. One type is created for the principle purpose of fraudulently acquiring resources. The founders of these companies, which we label fraudulent by ''trait,'' commence operations with illegitimate intentions. These firms are born to defraud resource holders. A second set of fraudulent firms are opportunistic deceivers. These organizations are created to pursue lawful business objectives, but at a point in their evolution, they perpetrate fraud to acquire resources or to gain an unfair advantage over competitors. We may think of these firms as fraudulent in a given ''state'' versus as a permanent trait. We cannot directly observe fraudulent firm type, but we separately theorize about the two types and will return to this form of heterogeneity in the discussion section.

Organizations that are born to perpetrate fraud almost certainly allocate the resources they receive in ways that were not intended by resource providers. In fact, this is the essence of being fraudulent by trait: these entities exist to deceive resource holders. Insofar as the individual managers who perpetrate fraud are motivated by personal gain versus organizational prosperity, they will avoid investments in firm-level objectives except to maintain whatever veil of legitimacy is necessary to sustain fraud.

More subtly, for organizations that are fraudulent by trait, avoiding investments in innovation and organizational development is a rational calculation based on expected returns from honest relative to dishonest endeavors. We know from classic literature in organization theory that successful practices

are reinforced and repeated in organizational settings. When fraud leads to desirable outcomes, the practices associated with it will become routinized (Levinthal and March, 1993). If an organization learns to implement fraudulent approaches rather than legitimate solutions, it inevitably eschews investments to develop staff and routines to diagnose problems, research solutions, implement decisions, and execute. As they serially forego investments in technical and organizational development, fraudulent firms simultaneously hone their skills in misleading constituents, because those skills help them increase the return to fraud. In short, the routinization of fraud is to be expected under standard arguments in learning theory. Actors pursue the activities for which they have achieved competence, and they forego the ones for which they lack depth of expertise (Cyert and March, 1963). This is the logic in Luo's (2005) analysis of corruption. He argued that firms that rely on bribery use it as a substitute for innovative technological and organizational development. Organizations that rely on fraud from an early stage are unlikely to ever develop the skills to conduct legitimate innovative and business development activities.

The second type of fraudulent firm houses a mix of legitimate business practices together with fraudulent resource acquisition. The central question we must consider is whether these fraudulent-by-state firms also divert resources from intended uses. We theorize that the answer to this question is yes. At the person level, the sociology literature suggests that perceptions of the valid use of funds are directly tied to how resources are obtained. In her sociological analysis of money, Zelizer (1994: 3) wrote that ''unlike an 'honest dollar', 'dirty' money is stained by its ethically dubious origins.'' She described a study of the Oslo prostitution market in which individuals used legal sources of income for rent payments and bills, while prostitution moneys were allocated to drugs, alcohol, and discretionary clothes. Zelizer argued that ''dirty money'' is apt to burn a hole in the pocket. Likewise, in an ethnographic study, Rainwater, Coleman, and Handel (1959) observed the frequent use of ''tin-can'' accounting: working-class housewives often stored funds for a given expense category in separate cans or envelopes, and once again, the source of income was tied to choices regarding its dispensation.

The finding of a system of mental accounts, in which money is perceived differently depending on its origin, also is the cornerstone to the judgment and decision literature. Thaler (1985) showed that, in contrast to the standard consumption model in which purchase decisions arise from a single budget-constrained optimization problem, people compartmentalize their income into different mental accounts and make resource allocation decisions within this set of multiple mental budgets. This cognitive process creates a direct link between spending behavior and the means of resource acquisition, because the mental account into which resources are placed depends on the source of funds. Moreover, a classic finding is that the psychological cost of spending earned income exceeds that associated with spending ''windfall'' funds (Kahneman and Tversky, 1979). Thus individuals are prone to spend unearned income and to save or invest earned income.

Might we expect a similar dynamic to occur at the organizational level? Because breaking rules to acquire resources generally requires less effort on the part of organizational actors than does acquiring resources through legitimate means, ill-gotten gains are akin to unearned income at the organizational level. We believe this may cause executives to use fraudulently acquired

resources for non-investment purposes, including perquisites that benefit them as individuals. Although we know of no direct evidence of this phenomenon in the literature on corporate fraud, there is an association between windfall earnings and the decision to increase managerial compensation (Blanchard, Lopez-de-Silanes, and Shleifer, 1994). Likewise, Davidson, Dey, and Smith (2013) found that senior executives who engaged in insider trading were more likely to pursue lavish lifestyles outside of work, including purchases of luxury residences, cars, and boats.

In addition to attracting financial resources with a limited investment of effort, acts of fraud may create a release valve from external pressures that motivate organizations to invest in challenging projects. The selection pressures that promote discipline in organizations may weaken for fraudulent actors because when they mislead constituents with false representations of performance, dishonest organizations are buffered from the pressures of their operating environment (Oliver, 1991; Marquis and Qian, 2013). By overstating their achievements, companies delay exposure to competitive stimuli that induce them to improve. In particular, if external audiences perceive an organization to be on track, the organization's leadership team will feel less compelled to invest in capabilities to improve its competitiveness.

In general, successful organizations that are not subject to market forces develop complacent leadership teams. Dating back to Cyert and March (1963), organizational theorists have postulated that the incentive to search for improvements derives from leaders' dissatisfaction with existing results. Because successful, fraudulent organizations experience better performance than they counterfactually would have, fraud generally elevates performance relative to aspiration levels. Moreover, insofar as individual managers who perpetrate fraud are motivated by individual gains, they will veer from the path of problem-driven search at the organizational level. Thus, even if fraud leads to the acquisition of general resources such as financial capital that may be used to invest in organizational capabilities, it co-occurs with the onset of managerial complacency that weakens the incentive to invest in productive gains.

Whether fraud is a trait or occurs in a more transitory state, we anticipate that perpetrators of fraud are more likely to allocate acquired resources to uses that were unintended by providers of capital. The psychology of endowment effects and mental accounts and the organization theory of satisficing, learning, and aspiration-driven search lead us to believe that companies that fraudulently acquire resources for the intended purpose of investment are less likely to follow through on committing these resources to organizational development and innovation. We hypothesize:

> **Hypothesis 1 (H1):** Compared with honest companies that acquire resources through legitimate means, fraudulent companies that obtain resources through misrepresentation of merit are less likely to invest them in their intended, productive uses.

In an extension, we also compare companies that illegitimately gain resources with the set of fraudulent firms that failed in their attempt to obtain a windfall gain. In other words, we compare fraudulent grant winners with fraudulent firms that did not secure grant moneys. If fraudulently obtained resources are fully misallocated, then:

Hypothesis 2 (H2): Compared with fraudulent companies that fail to obtain resources, fraudulent firms that succeed at acquiring funds are no more likely to invest in organizational development and innovation.

Fraud and Innovation Outputs

If H1 is supported and early-stage technology companies that fraudulently acquire resources are less likely than honest companies to invest resources in R&D or other productive activities, does it necessarily follow that companies that perpetrate fraud are less innovative? Or is there reason to believe that fraudulent firms might be more innovative than rule-abiding peers? Although this question has not been examined directly, there are theoretical reasons to hypothesize that fraud promotes innovation.

First, actors that dismiss norms of conduct may have greater latitude in experimenting with novel ways of doing things. By exploring the outskirts of institutionalized practices, rule breakers grant themselves a certain moral and regulatory flexibility in their transactions with other actors and with their normative and regulatory environments. By definition, to innovate requires a departure from conventional methods of doing things (Kirzner, 1973). When societal norms or dominant designs constrain the achievement of a goal, innovators often set aside conventional operating procedures in search of novel relationships between means and ends. When their new approaches are socially accepted, innovators are lauded for bold acts of entrepreneurship. Conversely, when they invent approaches that are socially proscribed, innovators are judged to perpetrate misconduct (Greve, Palmer, and Pozner, 2010). The first group is perceived as contributing to society's objectives and the second group as destroying them.

There are many examples of novel approaches that exist in regulatory gray zones or that brush the boundary between innovative and illegal conduct. Edelman (2017) cited Uber as a case in point. Uber purportedly launched its service in cities without gaining regulatory approval, and for several years it ran a program to locate and dodge authorities. Because it disregarded regulations, Edelman wrote, ''Uber's business model is predicated on lawbreaking. And having grown through intentional illegality, Uber can't easily pivot toward following the rules.'' If this and similar accounts are accurate, a number of highly innovative companies have achieved this status by disregarding the rules that tether established organizations to current approaches to conducting their business.

On one hand, fraudulent organizations are unlikely to match honest firms in levels of investment of resources in innovation projects (H1). On the other hand, because they violate rules, fraudulent firms experience lower compliance costs and can search across a broader, less-explored terrain for innovative solutions. In principle, this means that fraudulent actors may have a higher conversion rate of investments of resources in innovation projects. Fraudulent actors also have an incentive to present an external image of innovative success to legitimize third-party investments of resources into their organizations. The appearance of ''doing the right things'' promotes freedom from performance pressures imposed by these third parties.

To reconcile these ideas that seem in opposition, we draw a distinction between the types of innovations that organizations pursue. We distinguish fundamental innovation from more superficial cousins—minor advancements relative to the state of the art. Some discoveries are challenging, and innovation of this type requires significant and persistent investment and effort. Discoveries of this nature depend on accrued learning to develop absorptive capacity (Cohen and Levinthal, 1990). In the absence of persistent effort, organizations may be unable to keep pace with and assimilate novel technical developments and ultimately will fall behind the technical frontier. Innovations that are more superficial embody advancements that are smaller in nature and contribute much less to the progression of technology. We argue that superficial innovations can play a dual role: they are of modest technical or commercial significance, but often more importantly, they provide legitimacy that shields an organization from external scrutiny and that contributes to efforts to obtain additional external resources. This is a particularly important form of window dressing for fraudulent organizations, which must maintain a public perception of lawful business activities. Regardless of whether firms are fraudulent by trait or state, we expect a perceived benefit to these organizations for ownership of inventions that are less costly and less difficult to obtain. Combining these ideas, we hypothesize:

> Hypothesis 3 (H3): Compared with honest companies that acquire resources through legitimate means, fraudulent companies that obtain resources through misrepresentations of merit are (a) less likely to generate significant technical advances and (b) more likely to generate trivial innovations.

Likewise, we anticipate that fraudulent firms that successfully acquire external resources will have a stronger incentive to create the appearance of a track record of inventiveness than will fraudulent firms that fail to obtain capital infusions:

> Hypothesis 4 (H4): Compared with fraudulent companies that fail to obtain resources, fraudulent firms that succeed at acquiring funds are (a) no more likely to generate significant technical advances and (b) more likely to generate trivial innovations.

METHODS

Research Design and Empirical Setting

Although financial fraud and technological innovation each have received significant scholarly attention, the two phenomena rarely have been examined together. The first and most significant obstacle to research on these topics is the daunting challenge of systematic fraud identification. It is well understood in the literature that studying only cases of caught-fraud is likely to introduce significant biases. As Greve, Palmer, and Pozner (2010: 94) stated, ''A frequent dilemma in research on misconduct is that data become available when a social-control agent detects misconduct and decides to act against it.'' Suppose researchers observe a negative correlation between caught-fraud and investments in innovation. In a sample comprising only cases of caught-fraud and a comparison group, does this convincingly tell us that fraudulent firms are

less innovative? What if, instead, it tells us that social-control agents are less likely to investigate and identify fraud when it occurs in the subgroup of innovative firms? Stating the problem in general terms, if firms with certain attributes are less likely to be investigated by regulatory authorities, there is an obvious bias in estimates of the effect of any of these attributes or their correlates on either the incidence or consequence of fraudulent actions in datasets that depend on the detection of fraud.

Even if one assumes that the characteristics of caught-fraudulent firms are representative of the underlying distribution of these characteristics in the full population of fraudulent actors, a valid assessment of the association between fraud and innovation still would be elusive. This is because of a second obstacle: once an organization is under investigation for fraud, a firm's innovation activities likely will be disrupted. Key employees and other stakeholders may fear negative repercussions from an affiliation with the tainted actor and withdraw from transactions with the organization (Jensen, 2006; Kang, 2008). Therefore, an observed divergence in innovation trajectories after a case of caught-fraud may not accurately reflect the population-level relationship between fraud and innovation.

We examine a unique, hand-collected dataset of 467 early-stage high technology firms in five regions in China for which data were available.[1] We look for evidence of whether fraudulent and honest companies depart in their behaviors when they receive external financial resources intended to promote technological innovation. A distinct feature of our setting allows us to overcome the aforementioned empirical challenges to more reliably pinpoint the relationship between fraud and innovation. Rather than relying on a public-control agent's fraud detection actions, we observe fraudulent behaviors directly by comparing two sets of financial books filed by the same set of firms at the same time but to two different state agencies. Although Chinese law requires these two sets of books to be identical, the firms in our data have an incentive to over-report their financial performance in one setting and to under-report their results in the other. This institutional feature allows us to observe whether a company has ''cooked its books,'' but it is independent of the decisions of public-control agents to investigate certain types of actors. This approach enables us to identify the relationship between fraud and innovation without the complications of a public accusation of fraud.

All firms entered our sample when they applied for a government-funded innovation grant. The grant program we study is China's Innovation Fund for Technology-Based Firms, known as Innofund. It is China's most significant grant program for promoting innovation and commercialization in young companies (Stuart and Wang, 2016). Innofund operates under the administrative auspices of the Ministry of Science and Technology (MOST). Generally, grantee firms receive 0.5–1 million yuan from the Innofund, with a guaranteed matching grant of 50–100 percent from local governments.[2] This is a substantial sum for early-stage companies in China. To place it in context, we estimate this to be around six times the average annual R&D spending for Innofund applicants.

A particular institutional feature drove this sampling strategy. As a requirement for submitting an application to MOST for Innofund moneys, all applicants

[1] Two major cities in a central province in 2005–2007 and another three major cities in a coastal province in 2007–2010.

[2] 1 U.S. dollar = 6.96 Chinese yuan on January 1, 2020.

must provide detailed financial statements. Moreover, as we describe below, reporting high profits to MOST maximizes the odds of obtaining a grant. To maintain a current business license, all firms also must submit an equivalent set of financial statements to their local State Administration of Industry and Commerce (SAIC).[3] Thus for each firm in our sample, we obtained two sets of financial statements pertaining to the same fiscal year, enabling us to observe fraudulent practices directly when there are notable inconsistencies across the financial statements. We use the presence or absence of discrepancies to classify firms as either ''fraudulent'' or ''honest.''

Chinese accounting law requires the financial data submitted to MOST and to the firm's local SAIC to be identical. First, the fiscal year for *all* companies in China is mandated to be the solar year, January 1 to December 31. Second, China adopted a ''unified accounting system'' statute with strict guidelines that regulate how firms must prepare and file their financial statements (Accounting Law, 2000). Third, in multiple passages of the Chinese Accounting Law, companies are prohibited from creating different sets of financial books. For instance, the accounting law states: ''the basis for preparing financial and accounting reports provided to different users of accounting documents [cannot be] inconsistent'' (Article 42). Likewise, ''Except for the statutory account books, a company shall not set up other account books'' (Article 172).

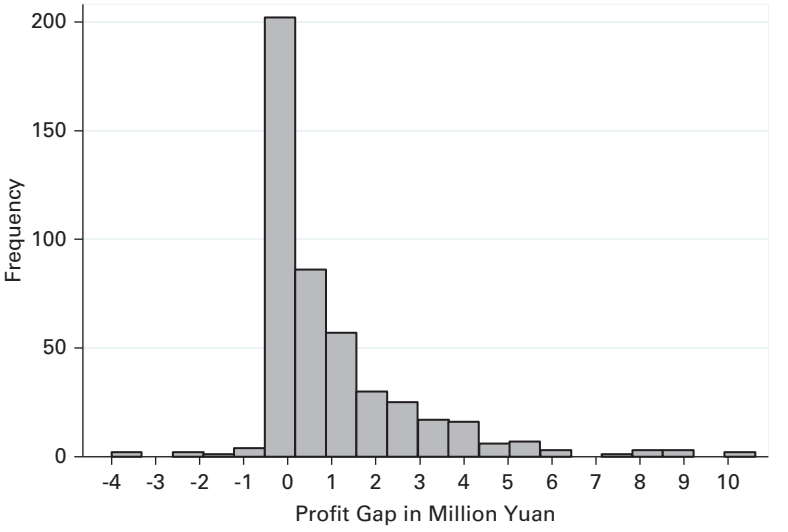
Although multiple Chinese laws require the financial statements submitted to MOST and to SAIC by the same company for the same fiscal year to be identical, there are many cases of significant inconsistencies in the two sets of books (Stuart and Wang, 2016). Many firms have clear incentives to overstate their financial performance to MOST. To support its primary goal of promoting technological innovation and the commercialization of new technologies, Innofund expressly considers financial performance in evaluating grant applicants. Innofund regards financial health as necessary for the successful use of grant funds for innovation purposes. Innofund officials informed us that total profit is the single most important metric in MOST's assessment of applicants' financial health. Each grant applicant is assigned a financial health rating by a panel of experts. Regardless of any other dimension of merit, companies that receive a low financial rating are automatically eliminated from consideration for the grant. Therefore, to stand a chance for an Innofund grant, many firms in our sample have an incentive to exaggerate their financial performance in their application to MOST.

In direct contrast, for the purpose of tax evasion, companies have an incentive to underreport true profits to SAIC. Although not directly responsible for tax collection, SAIC collaborates with the State Administration of Taxation (SAT) to conduct joint inspections of and to coordinate administrative actions against tax evaders (Wang and Zhang, 2010).[4] Recently, some regions in China

[3] China's Corporate Law states that all commercial entities in the country must register with their SAIC at the time of establishment and must submit annual inspection documents to maintain their legal status (Company Law, 2006; SAIC, 2006). These documents include detailed financial statements that must be approved by a registered accounting firm. We purchased SAIC data mainly through a credit rating company with a national partnership network with law firms.

[4] Theoretically, MOST and the local SAICs could coordinate with each other to discourage firms from submitting inconsistent financial statements. This data sharing has yet to occur because of the fragmentation of authority and the ''matrix muddle'' of China's state bureaucracies (Mertha, 2009). MOST and SAIC are both ministry-level agencies, and neither has authority over the other. Geographic distance creates additional barriers to cross-agency coordination.

Figure 1. Profit data manipulation across two financial books in China (N = 467).



have built information technology systems to facilitate data sharing among the local SAT, SAIC, and other departments (Tian, 2012). Given the close relationship among these agencies, tax evaders that underreport profits are likely to do so to both SAT and SAIC.

As a measure of fraud, we focus on the presence of a discrepancy in reported total profit, defined to be earnings before interest and taxes, across the MOST and the SAIC books. As noted, total profit is considered by Innofund in its assessment of applicants *and* it has direct tax implications, so it is the financial metric most likely to be doctored across the two sets of books. For each firm *i* that applies for an Innofund grant in year *t*, we calculated:

$$\text{ProfitGap}_{(it)} = \text{EBIT\_MOST}_{(it)} - \text{EBIT\_SAIC}_{(it)}$$

Figure 1 shows the frequency distribution of $\text{ProfitGap}_{(it)}$. While there are a handful of observations with a nontrivial negative profit gap, almost every firm in the sample either submits identical profits across the two books (shown by the large spike at zero in the figure) or reports a higher profit number in its application for an Innofund grant (the extensive skew to the right of zero in the figure).[5] Thus the vast majority of misstatements of profit align with applicants' incentives to make their financial statements submitted to MOST for Innofund grants show higher profits than the financial books submitted to their local SAIC.

Estimation

We wish to understand whether there is a causal effect of winning an innovation grant on firm-level, innovation-related dependent variables and whether the effect varies between honest and fraudulent companies. The setup of the data allows

[5] Nine firms reported a smaller profit to MOST than to SAIC. Deleting these firms from our analyses does not change our empirical results in any meaningful way.

for a difference-in-differences approach to estimating the treatment effect of winning a grant (Ashenfelter and Card, 1985). But because we have three differences—before versus after the time of innovation grant application (''Post''), fraudulent versus honest firms (''Fraud''), and grant recipients versus denied applicants (''Win'')—some of our regressions implement a difference-in-difference-in-differences (DDD) design. In formal terms, we began with the equation:

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_1 \text{Post}_t + \beta_2 \text{Fraud}_i + \beta_3 \text{Win}_i + \beta_4 \text{Post}_t * \text{Fraud}_i \\ & + \beta_5 \text{Post}_t * \text{Win}_i + \beta_6 \text{Fraud}_i * \text{Win}_i \\ & + \beta_7 \text{Post}_t * \text{Fraud}_i * \text{Win}_i + B_8 X_i + \varepsilon_{it} \end{aligned} \tag{1}$$

where Y_{it} measures either the resource allocation choices or innovation outcomes of firm *i* at time *t*, depending on the specification; Win_i is a binary variable set to 1 for all firms that receive an Innofund grant; Post_t is a dummy that equals 1 for the post-grant-application time period; Fraud_i is an indicator that equals 1 for fraudulent firms; X_i is a vector of control variables; and ε_{it} is the error term.

We assembled a longitudinal data set tracking each firm's inventive activities over time. We observed all firms in the data both before and after they applied for an Innofund grant. Because we have repeated annual observations for each firm in the sample, it is possible to estimate equation 1 with firm fixed effects (FE). In FE specifications, all covariates and interaction terms that do not vary within-firm will be fully absorbed into the time-stationary FEs and therefore drop from the estimations. After removing all time-stationary variables that are subsumed into the fixed effects, the final model specification can be written:

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_1 \text{Post}_t + \beta_4 \text{Post}_t * \text{Fraud}_i + \beta_5 \text{Post}_t * \text{Win}_i \\ & + \beta_7 \text{Post}_t * \text{Fraud}_i * \text{Win}_i + \varepsilon_{it} \end{aligned} \tag{2}$$

As we describe in further detail in Online Appendix A (http://journals.sagepub.com/doi/suppl/10.1177/0001839220927350), the hypothesis tests in DDD specifications involve comparisons among coefficients and therefore are (mostly) assessed with Wald tests of equality in coefficient values. For instance, to test the prediction in hypothesis 1 that honest grant winners place more employee recruitment ads than fraudulent grant winners do, we compare the magnitude of the coefficient sum ($\beta_1 + \beta_4 + \beta_5 + \beta_7$), which gives the pre–post change in recruitment ad placements for fraudulent grant winners, to the magnitude of the coefficients ($\beta_1 + \beta_5$), which is the pre–post change in placement of recruitment ads for honest grant winners.

By taking a split sample approach, it is possible to simplify the analysis to a set of two-way, difference-in-differences (DD) specifications. For example, if we wish to compare the effect of winning a grant for fraudulent relative to honest grant winners as just described, we can subset the analysis sample to include only companies that win a grant. The subsetting of the sample removes one of the differences from the analysis (grant recipients versus denied applicants, since we subset on winners only), but it still allows a DD estimate of the treatment effect of a grant if the recipient is honest. In the subsample, we simply compare the before-to-after change in the outcome variable for

honest grant winners to the before-to-after trend that is established by fraudulent grant winners. Because one of the differences is removed, when we subset the data in this way we no longer need to estimate a three-way interaction effect. When we present the results, we will begin with subsets of DD specifications, and then we will show that the findings are nearly identical when we estimate all three differences in a single DDD specification.

To estimate the regressions, we followed Bertrand, Duflo, and Mullainathan (2004), who showed that DD estimators often produce downwardly biased standard errors and recommended pooling multiple over-time observations per firm into a single pre- and a single post-treatment period for bias correction. In other words, we collapsed the data's panel structure so that there is a single pre- and post-grant application observation per firm. We report only these more-conservative estimates.

Variables and Measurements

We examined firms' resource allocation through their human resource recruitment and patenting practices. We believe the former to be a novel and general approach to studying innovation inputs. In past work, R&D spending has been the principle proxy for levels of investment in innovation, but there are tax and financial market implications of the categorization of corporate spending by expense category (Chen et al., 2018), which can lead to distortions in financial reporting and extensive measurement error. The placement of online recruitment ads offers a new approach to proxy for companies' intended investment levels while circumventing some of the well-known measurement problems associated with R&D spending. We describe each of the outcome variables below.

Job postings. We examined firms' intentions to expand through the placement of online recruiting ads. We constructed three measures of job postings for each company-year: the total number of positions in *t*, the number of non-R&D positions advertised, and the count of R&D positions in *t*.[6] Some job advertisements in the dataset do not specify the exact number of positions available. Typically, such ads either provide a range (e.g., ''5–10 positions open'') or a more ambiguous statement (such as ''multiple openings''). While it was not possible for us to disambiguate these references, in all cases of a range, we used its mean. In the case of an advertisement for ''multiple positions,'' we assigned the average number of jobs posted by all job-posting firms in that year.

We acquired data from the three leading job-related websites in China. One is among the oldest job boards in China and is colloquially known as ''China's Monster.com''; the other two are classified ad sites originally modeled after Craigslist. After a few mergers, these three companies are now owned by a

[6] We categorized any job position containing the following terms as ''R&D-related'': (1) chief scientist; (2) chief technology officer; (3) (deputy) director of technology; (4) (deputy) director of engineering; (5) (deputy) lab director/manager; (6) product manager; (7) quality-control, plant, or production manager; (8) team leader in R&D; (9) data scientist/engineer; (10) researcher, research scientist/engineer; (11) technical specialist; (12) advanced/senior engineer; (13) IC designer; (14) R&D team member; and (15) any position that requires an advanced degree in a STEM field.

NYSE-listed parent company, but each subsidiary is separately managed. There is a market segmentation among these sites, so it is important to collect data from all three. Positions posted on the job board tend to be high skill, including middle- and upper-level managers and more-senior technical positions. Positions posted on the other two sites target lower-level managers and the broader white- and blue-collar workforce.

Innovation. We measured innovation output by the number of patent applications filed by each company. Although scholars have used patents to measure technological change, one concern is that the quality of individual patents varies widely: some inventions are extremely valuable, whereas others are of almost no commercial import. In the U.S., scholars have used patent bibliometric-based measures and paid patent renewal rates to create more precise measures of patent quality.

The Chinese patent system rarely requires patent applicants to cite works of prior art, and therefore we cannot rely on conventional, citation-based measures of patent quality. However, one institutional feature in the Chinese patenting system allows us to differentiate among patents. Patents are filed under three categories in China—invention, new utility, and new design—in decreasing order of innovativeness and commercial value. Design patents are uncommon and generally viewed as lacking in commercial value, so we excluded them from the analysis.[7]

Invention patents are much more significant than utility patents. According to China's Patent Law (2008), invention patents apply to novel products, processes, or their improvements, whereas utility patents protect new technical solutions proposed for the shape or structure of a product that are fit for practical use. The relative importance of invention versus utility patents is reflected in the patent review system of China's State Intellectual Property Office (SIPO), which imposes differential application fees, requirements for public notification, lengths of examination, and patent terms according to category.[8] Scholars have leveraged the difference in patent types to study Chinese firms' innovation outcomes (Huang, 2010; Huang, Geng, and Wang, 2017; He et al., 2018). Likewise, in evaluating grant applicants' technological merit, the Innofund applies a formula that weights each invention patent at six times the value of a utility patent. This difference in value allowed us to be more precise in our effort to differentiate inventive outputs with important commercial and technological merit from those of more marginal economic and technological significance.

[7] Design patents are narrow and protect only one feature, the external appearance of a product. They do not protect a product's functionality, inner workings, or novel application of an existing technology. In our sample, only 4% of observations have design patents, and five firms account for half of all design patents filed. Therefore, design patents are tangential in the data and possibly conflating in this sample of companies.

[8] An invention patent must be published online for 18 months before a SIPO examination, whereas design and utility patents are not subject to this requirement. It also may take three years before SIPO issues a final decision for an invention patent, versus 18 months for a new utility patent. Likewise, the examination fee is three times higher for an invention patent. The primary benefit of an invention patent is that it is in force twice as long as the other two types of patents, 20 versus 10 years, and it is easier to enforce invention patent rights.

To construct a holistic view of each firm's patenting activities, we constructed three measures: the count of all patents, the count of important (invention) patents, and the count of more marginal (utility) patents.[9] All the patenting information comes from SIPO.

Innofund grant. The principle hypotheses we explore concern the relationship between fraudulent resource acquisition and resource allocation. The sample contains all companies that applied for an Innofund grant from five Chinese regions for which data were available. These include two major cities in a central province and three major cities in a coastal province. We excluded applicants from smaller cities because our data providers were unable to collect the financial statements of companies in smaller counties.[10]

We then created a dummy variable *Win* set to 1 if a company received an innovation subsidy grant from the Innofund. Slightly more than half (53.96 percent) of the firms in the sample received grants.

Post-grant era. We created a dummy variable to capture the post-grant period in which (for grant winners) the state funds would have been transferred to the firm and available for investment. All firms in the data applied for an Innofund grant, and we defined the year of grant application to be T_0, regardless of whether the company was awarded funds.

In defining the post-grant period and therefore the time window during which we assess the effect of the grant funds on innovation inputs and outputs, we take a few factors into consideration. Internal Innofund documents show that grant moneys typically are distributed to awardees by the end of the calendar year following submission of the application. Firms submit their applications in grant year T_0; proposal reviews are conducted in late T_0 to early T_{0+1}; winning proposals are announced and grants are allocated in mid to late T_{0+1}. Based on this timeline, we categorized T_{0+1} and the three prior years as the ''pre-grant'' period, and we categorized the subsequent four years (T_{0+2}, T_{0+5}) as the ''post-grant'' period. We used the same categorization for both patenting and hiring. We assumed that firms are able to quickly make the internal decision to hire staff and deploy resources to accelerate the innovation projects proposed in grant applications. We have explored all reasonable assumptions about time lags and find the results to be broadly similar across the range of one-, two-, and three-year lags.

Fraud. To construct a binary indicator of whether a company commits fraud, we began with the measure of $\text{ProfitGap}_{(it)}$ described above. We subtracted

[9] In robustness checks, we also looked at the weighted valuable patents that include both inventions and new utility patents. Following the rule adopted by the Innofund, we counted each utility patent as 1/6 invention patent.

[10] In past years, SAICs sold firm-level financial data to interested parties with a valid Chinese license to practice law. But following an article the *New York Times* published describing the hidden wealth of a top Chinese politician's family based on SAIC filings (Barboza, 2012), Beijing requested that local SAICs restrict access to these data. Compliance with the policy varied between SAIC regions, however, so we focused on the locations in which we were able to acquire information. If anything, we would anticipate lower levels of fraud in the locations for which these data were made available to us, but we cannot verify this presumption.

the firm's total profit filed with SAIC from the profit filed with MOST, which we then normalized by the profit reported to MOST. The normalization provides a percentage-based measure of the misstatement of profits. We calculated:

$$\text{Weighted\_ProfitGap}_{(it)} = \frac{\text{EBIT\_MOST}(it) - \text{EBIT\_SAIC}(it)}{\text{EBIT\_MOST}(it) + 1} \quad (3)$$

We next created a dummy variable *Fraud* to measure whether a firm cooked its books. We coded *Fraud* = 1 if a company's weighted profit gap is $\geq$ .20. We did this to allow for accidental omissions of small earnings or cost items that led to minor discrepancies in financial reporting, which may occur in good faith. Of course, such an interpretation of the data does not reconcile with the fact that over 95 percent of the misreported financial statements occur in one direction: the profit number reported to MOST is larger than the number given to SAIC. If the difference in reported profit was caused by human error, we would expect the mean of $\text{Weighted\_ProfitGap}_{(it)}$ to be zero. In the data, the overall mean of $\text{Weighted\_ProfitGap}_{(it)}$ is a striking .81, even including the approximately 45 percent of companies that report no profit discrepancy. Thus in this sample of companies, the misstatements of profit between the two agencies typically are by a wide margin.

Using the 20 percent threshold for profit discrepancy between the two sets of books, 55 percent of the sample—256 out of the 467 companies—reported substantively different financial results to MOST and SAIC. At $\geq$ 40 percent profit discrepancy, 239 companies (51 percent) committed fraud, and at $\geq$ 5 percent profit discrepancy, 274 companies (58.7 percent) committed fraud. Because the significant majority of fraudulent companies trigger the dummy variable at all of these thresholds, regression results are relatively stable across the choices.

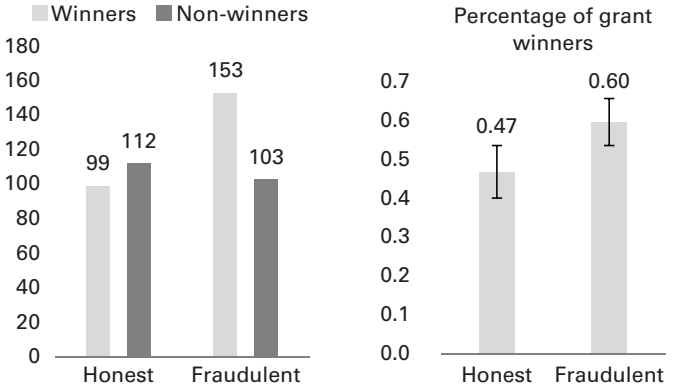
DESCRIPTIVES AND MODEL RESULTS

We estimated the regressions in two steps. First, we ran firm fixed effects models to examine whether capital infusions cause increases in firms' post-grant recruitment activities and patenting, and whether the effect differs for fraudulent versus honest grant winners. Second, to further address the concern that grant-winning and non-winning firms are drawn from groups that exhibit different, observable characteristics that simultaneously influence winning probabilities and innovation outcomes, we reran the analysis using a sample constructed from a coarsened exact matching (CEM) algorithm. Before reporting multivariate regressions, we report figures and trends in the data by company type and grant status.

Descriptive Statistics

Figure 2 describes the number of firms in each of the cells that form comparison groups in the regressions. Recall that other than the time period dummy (pre- or post-grant), the distinctions are between honest and fraudulent firms and between grant winners and unfunded companies. Figure 2 is striking in and of itself. In a 467-firm sample, the majority of companies, 55 percent, misreport their profit number to Innofund or SAIC (or both) at the 20 percent profit discrepancy threshold. Moreover, the figure reveals the base rates of winning

Figure 2. Distribution of firms across types and funding status.



* T test suggests that the difference across the two groups is significant at the .01 level.

grants across firm types. Although we focus this paper on how firms allocate state-provided resources, Figure 2 appears to illustrate that fraud pays, at least in terms of obtaining an innovation grant (Stuart and Wang, 2016). The left panel shows that of the 211 honest firms in the data, 99 win grants. By contrast, 153 of the 256 fraudulent firms obtain state grants. The right panel shows this difference in percentage terms, and the error bars illustrate 95-percent confidence intervals. In short, a majority of fraudulent firms acquire state resources, while less than half of honest companies do so. Of course, the financial return to fraud is why it is so widespread; if fraudulent companies were commonly identified and their applications rejected, the practice would be less prevalent.

Table 1 reports the mean, standard deviation, and correlation matrix for key variables. Because we ran firm-level fixed effects regressions, we report within-firm correlations, along with variable means and standard deviations.

Because we are working with panel data and a triple difference specification, the regressions are complex. For this reason, we began with simple cross tabulations that illustrate the unconditional relationships in the data. Figure 3 shows the pre-to-post-innovation-grant outcome variables split between fraudulent vs. honest companies, and between grantees that are funded vs. unfunded. For example, the upper left pane in Panel A shows the pre-to-post grant change in total recruitment ads placed by honest firms, separated by winners and losers. It illustrates that honest, unfunded firms place ads to recruit an average of 3.4 more employees in the post-grant time period relative to the pre-grant window. Honest firms that receive a grant increase their recruitment by 11.1 more positions in the post-grant period—a much larger increase. Conversely, unfunded fraudulent companies add 2.3 more job requisitions in the post-grant period, and funded fraudulent companies add 5.7 more job ads relative to their pre-grant means. Therefore, while there is some increase in post-grant hiring among fraudulent firms, consistent with hypothesis 1, among honest companies there is a much larger change in hiring plans in response to winning a grant.

Table 1. Correlations and Descriptive Statistics (N = 934 for 467 Firms)

	1	2	3	4	5	6	7	8	9	10
1. Total patents										
2. Inventions	.791•••									
3. Utility patents	.907•••	.460•••								
4. Total positions	.303•••	.304•••	.231•••							
5. R&D positions	.182•••	.202•••	.125•••	.608•••						
6. Non-R&D positions	.299•••	.296•••	.231•••	.985•••	.460•••					
7. Post grant	.118•••	.063+	.128•••	.211•••	.095••	.215•••				
8. Winning × Post grant	.183•••	.134•••	.174•••	.246•••	.142•••	.244•••	.608•••			
9. Fraudulent × Post grant	.022	–.069•	.079•	.047	–.036	.060+	.615•••	.454•••		
10. Winning × Post grant × Fraudulent	.081•	–.011	.125•••	.091••	.015	.098••	.443•••	.728•••	.72•••	
Mean	5.997	2.843	3.154	7.272	.941	6.332	.5	.27	.274	.164
S. D.	11.062	5.237	7.628	13.224	2.601	11.823	.5	.444	.446	.37
Min.	0	0	0	0	0	0	0	0	0	0
Max.	92	55	62	152	37	137	1	1	1	1

+ $p < .10$; •$p < .05$; ••$p < .01$; •••$p < .001$.

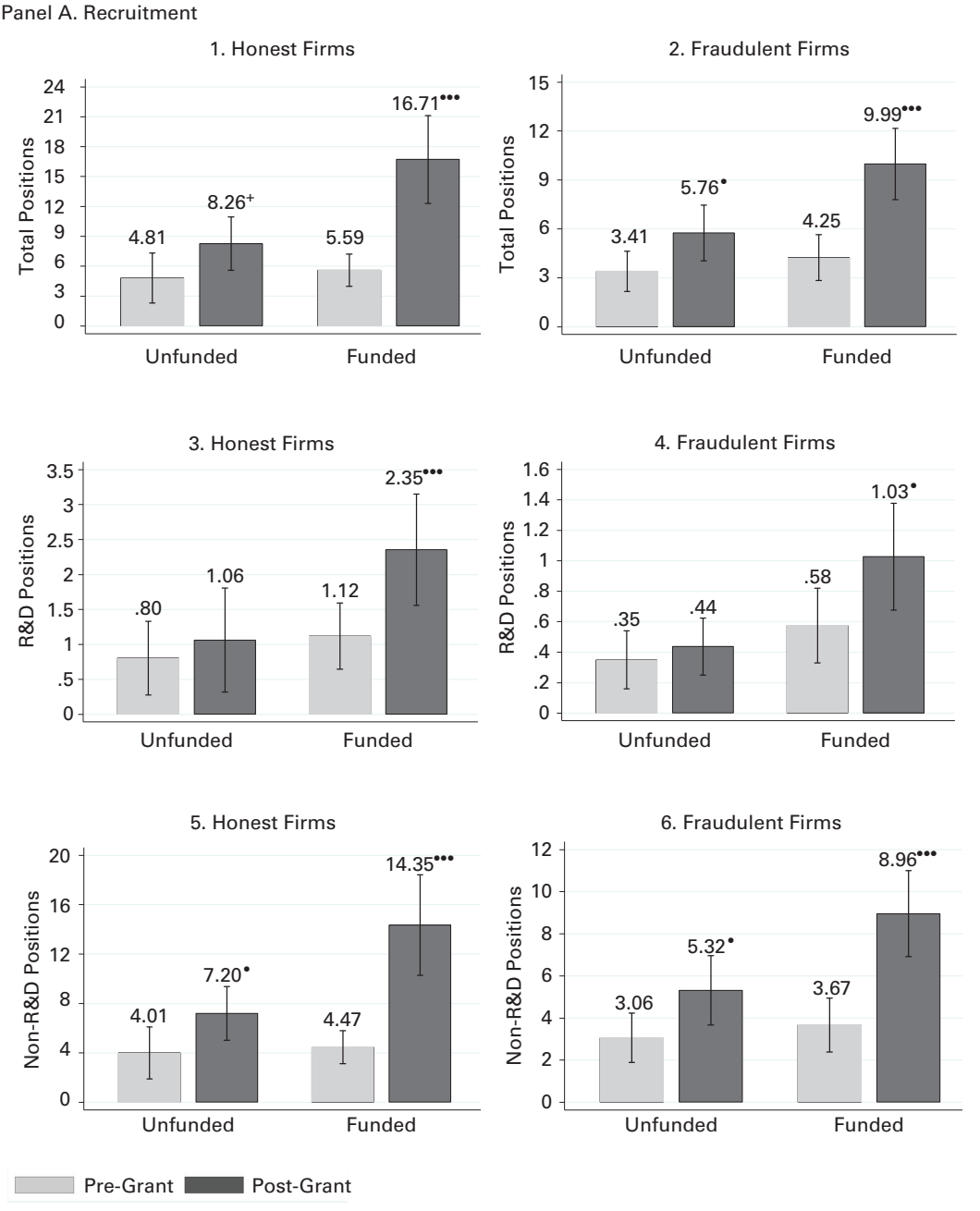
The same pattern is observed in graphs 3 and 4 of Figure 3, panel B, which displays trends in invention (important) patents. Consistent with hypothesis 3, honest grant winners file about two more invention patents in the post-grant versus the pre-grant period. For the other three categories of firms—fraudulent grant winners, fraudulent losers, and honest unfunded firms—there is only a minimal change in invention patents between the pre- and post-grant time periods. For the less important utility patents, graphs 5 and 6 of panel B demonstrate the opposite: fraudulent grant winners have the largest spike in utility patents in the post-grant period. This accords with hypothesis 4: fraudulent grant winners appear to pump up applications for window-dressing patents in the post-grant period to engender an appearance of productivity.

Hypothesis 2 anticipates that fraudulent grant winners exhibit no difference from fraudulent non-winners in hiring behavior. The data in Figure 3 are less consistent with this prediction. Graph 2 of panel A shows that fraudulent winners post ads for 5.7 more jobs in the post-grant period, while fraudulent non-winners increase placements by only 2.3 positions relative to the pre-grant mean.

Multivariate Regressions

Pre trends. Difference-in-differences estimators rest on an identifying assumption. Colloquially, the DD estimate of the treatment effect is the change in the outcome Y in the post-period for the treated units relative to their pre-period outcome, minus the change in Y in the post-period for the untreated units relative to their pre-period outcome. The treated group is de-trended by the change in outcome in the control group, and this yields an estimate of the average treatment effect in the sample. The validity of our estimator rests on the assumption that the treated and control units have followed the same trend in the outcome variable in the pre-grant-application period regardless of the level. This assumption of ''parallel trend lines'' allows us to believe that the

Figure 3. Pre- and post-grant descriptive statistics across different types of firms.

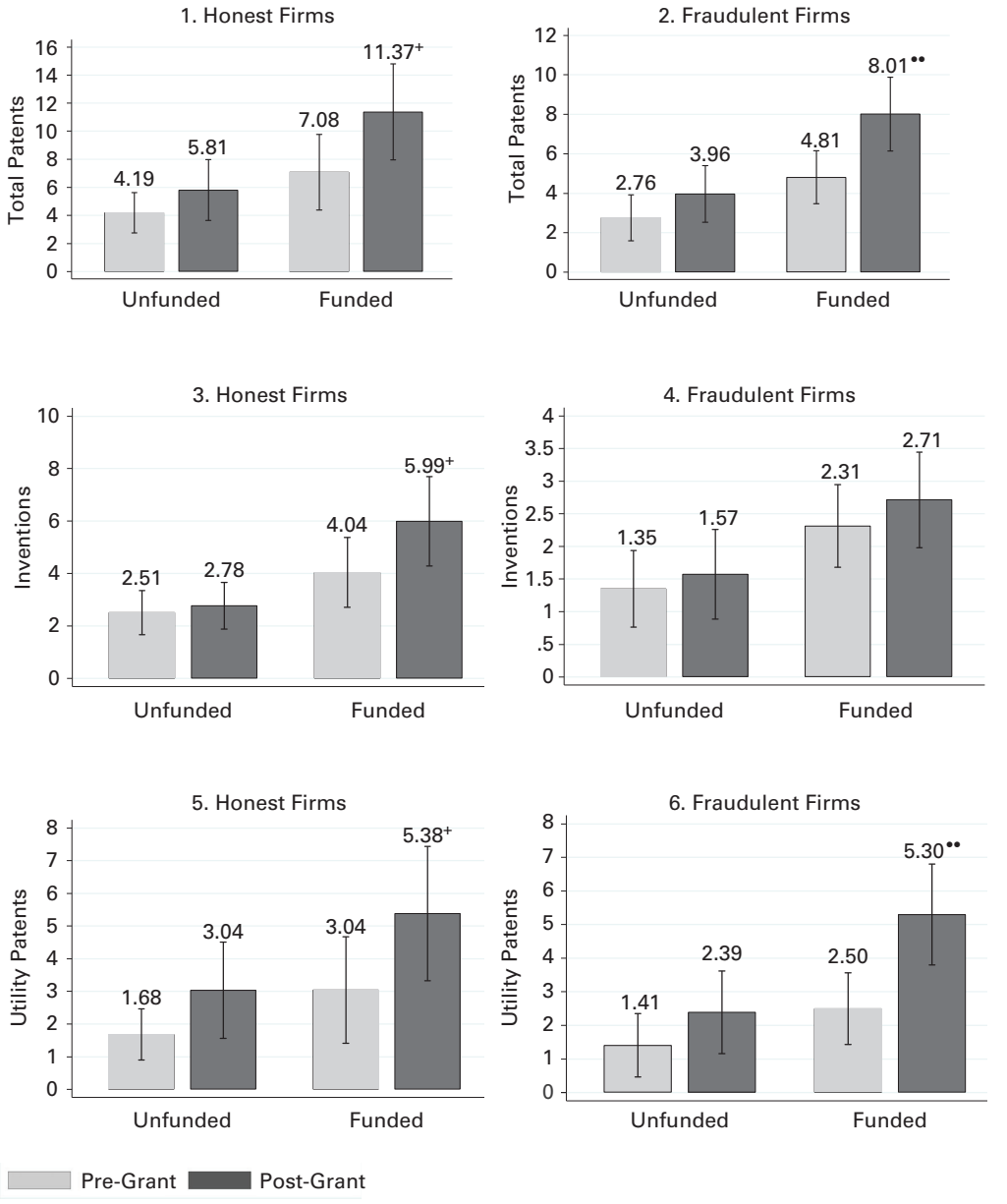


$^{+}p < .10$; $^{\bullet}p < .05$; $^{\bullet\bullet}p < .01$; $^{\bullet\bullet\bullet}p < .001$ for two-tailed t-tests within group across eras.

(*continued*)

Figure 3. (continued)

Panel B. Patenting



$^{+}p < .10$; $^{\bullet}p < .05$; $^{\bullet\bullet}p < .01$; $^{\bullet\bullet\bullet}p < .001$ for two-tailed t-tests within group across eras.

control group is a valid yardstick for assessing pre–post changes in the treated cases because the trend followed in the control group can be used to difference out the trend line that the treated group would have experienced had it not been treated. General support for the parallel trends assumptions is described in Online Appendix B.

To test our hypotheses, we ran four sets of multivariate regressions. Across all outcome variables, we first ran two-way, difference-in-differences (DD) specifications that compare fraudulent winners with honest winners, fraudulent winners with fraudulent non-winners, and honest winners with honest non-winners. We then ran DDD specifications that simultaneously take into consideration all three sets of differences in the data: pre–post changes, fraudulent–honest firms, and funded–unfunded firms. The DDD regressions are needed to compare honest versus fraudulent firms while also varying grant winner versus loser status. Finally, we report a robustness check that is a DDD implementation of coarsened exact matching (CEM).

Empirical results: Recruitment. We hypothesized that compared with fraudulent companies that win an Innofund grant, honest recipients are more likely to place online recruitment ads in the post-grant period. Table 2, panel A reports two-way DD results. The data include only grant winners, and therefore the comparison is between fraudulent and honest grant winners. Model 1 shows that, on average, an honest Innofund grantee adds 5.38 more job positions than its fraudulent counterparts between the pre- and post-grant periods. Models 2 and 3 differentiate across job types. Once again, the interaction term shows that receiving an Innofund grant has a smaller effect on the change in recruitment for fraudulent winners, whether positions are related to R&D ($\beta_4 = -.78$) or not ($\beta_4 = -4.60$). All three coefficients are highly statistically significant, supporting hypothesis 1.

Hypothesis 2 posits that fraudulent grant winners will not significantly depart from fraudulent losers in hiring practices. In other words, in the post-grant period, we anticipate fraudulent grant winners to invest no more in talent recruitment than fraudulent losers. Table 2, panel B subsets the sample on fraudulent firms only and compares grant winners to non-winners. Across the three regressions on total jobs, R&D-related jobs, and non-R&D jobs, the coefficient (β_5) for the interaction term ($Post_t \times Win_i$) is consistently positive and statistically significant. These results indicate that fraudulent grant winners actually do increase their recruiting effort between the pre-grant and post-grant periods relative to non-winning fraudulent counterparts. These results reject hypothesis 2, although panel C will qualify this interpretation.

Panel C examines only honest companies so we can compare grant winners vs. non-winners, even though we do not specifically hypothesize about this comparison. As in panel B, across the three models on total jobs, R&D-related jobs, and non-R&D jobs, the coefficient (β_5) for the interaction term ($Post_t \times Win_i$) is positive and statistically significant. However, comparing the coefficient (β_5) for the interaction term ($Post_t \times Win_i$) across panels B and C in Table 2, the β_5 parameter estimate consistently is (twice or more) larger, and more precisely estimated, for the honest subsample relative to the fraudulent subsample. This tells us that the infusion of state funding catalyzes a much more animated hiring process at honest firms than it does for fraudulent grant

Table 2. Difference-in-Differences Analyses across Subgroups

	Total Jobs (1)	R&D Jobs (2)	Non-R&D Jobs (3)	Total Patents (4)	Inventions (5)	Utility Patents (6)
Panel A. Fraudulent winners vs. Honest winners (N = 504)						
Post × Fraud (β_4)	–5.381•••	–.782••	–4.599••	–1.090	–1.551•••	.460
	(1.542)	(.274)	(1.441)	(.919)	(.455)	(.656)
Post (β_1)	11.120•••	1.235•••	9.885•••	4.293•••	1.949•••	2.343•••
	(1.202)	(.213)	(1.123)	(.716)	(.354)	(.512)
Constant	–5.560	–.617	–4.943	–1.146	.025	–1.172
	(6.008)	(1.066)	(5.613)	(3.580)	(1.772)	(2.558)
Log likelihood	–1.8e + 03	–919.596	–1.8e + 03	–1.5e + 03	–1.2e + 03	–1.4e + 03
Panel B. Fraudulent winners vs. Fraudulent non-winners (N = 512)						
Post × Win (β_5)	3.392••	.365•	3.026••	1.999•	.175	1.823••
	(1.153)	(.167)	(1.101)	(.778)	(.346)	(.607)
Post (β_1)	2.347••	.087	2.260••	1.204•	.223	.981•
	(.891)	(.129)	(.851)	(.601)	(.268)	(.469)
Constant	–1.174	–.044	–1.130	–.102	.388	–.490
	(4.545)	(.659)	(4.340)	(3.066)	(1.365)	(2.393)
Log likelihood	–1.7e + 03	–687.985	–1.7e + 03	–1.5e + 03	–1.1e + 03	–1.3e + 03
Panel C. Honest winners vs. Honest non-winners (N = 422)						
Post × Win (β_5)	7.675•••	.976••	6.700•••	2.668••	1.682•••	.986
	(1.521)	(.327)	(1.429)	(.940)	(.507)	(.638)
Post (β_1)	3.445•••	.259	3.186••	1.625•	.268	1.357••
	(1.042)	(.224)	(.979)	(.644)	(.347)	(.437)
Constant	–5.560	–.617	–4.943	–1.146	.025	–1.172
	(5.542)	(1.192)	(5.204)	(3.423)	(1.848)	(2.326)
Log likelihood	–1.5e + 03	–816.968	–1.4e + 03	–1.3e + 03	–1.0e + 03	–1.1e + 03

• $p < .05$; •• $p < .01$; ••• $p < .001$.

winners. In short, fraudulent firms appear to invest a fraction of their grant proceeds in recruiting, but a much smaller fraction than do honest awardees. We test this effect directly in the following table.

Table 3, columns 1–3, reports difference-in-difference-in-differences (DDD) regressions that take into account all subgroups simultaneously, allowing us to investigate whether there are differential effects of state funding on honest grant winners vs. fraudulent grant winners, using the trend lines of non-winners as the benchmark. In the DDD framework, the hypothesis that honest grant winners invest more in recruitment than do fraudulent grant winners is assessed by a Wald test of $(\beta_4 + \beta_7) = 0$, while the hypothesis that fraudulent grant winners do not invest more in human resource acquisition than fraudulent non-grant winners is assessed by a test of $(\beta_5 + \beta_7) = 0$.

Among honest firms, grant winners exhibit a significant pre–post increase in recruitment ads relative to non-winners. Among fraudulent firms, grant winners have a pre–post increase in recruitment ads relative to non-winners, and the increase is statistically significant for total jobs and non-R&D jobs but not for R&D-related jobs. Among grant winners, the test $(\beta_4 + \beta_7 = 0)$ reveals that

Table 3. Difference-in-Difference-in-Differences Analyses*

	Total Jobs (1)	R&D Jobs (2)	Non-R&D Jobs (3)	Total Patents (4)	Inventions (5)	Utility Patents (6)
Post × Fraud × Win (β_7)	–4.284•	–.611 +	–3.673•	–.669	–1.506•	.837
	(1.876)	(.351)	(1.775)	(1.208)	(.598)	(.883)
Post × Win (β_5)	7.675•••	.976•••	6.700•••	2.668••	1.682•••	.986
	(1.378)	(.258)	(1.304)	(.887)	(.440)	(.649)
Post × Fraud (β_4)	–1.097	–.172	–.926	–.421	–.045	–.377
	(1.364)	(.255)	(1.290)	(.878)	(.435)	(.642)
Post (β_1)	3.445•••	.259	3.186•••	1.625••	.268	1.357••
	(.944)	(.176)	(.893)	(.608)	(.301)	(.444)
Constant	–5.560	–.617	–4.943	–1.146	.025	–1.172
	(5.020)	(.938)	(4.750)	(3.232)	(1.601)	(2.363)
Log likelihood	–3.2e + 03	–1.6e + 03	–3.1e + 03	–2.7e + 03	–2.1e + 03	–2.4e + 03
1. Honest winners post-grant vs. honest winners pre-grant: ($\beta_1 + \beta_5$)	+•••	+•••	+•••	+•••	+•••	+•••
2. Fraudulent winners post-grant vs. fraudulent winners pre-grant: ($\beta_1 + \beta_4 + \beta_5 + \beta_7$)	+•••	+••	+•••	+•••	+	+•••
3. Honest non-winners post-grant vs. honest non-winners pre-grant: β_1	+•••	+	+•••	+••	+	+••
4. Fraudulent non-winners post-grant vs. fraudulent non-winners pre-grant: ($\beta_1 + \beta_4$)	+•	+	+•	+ ^	+	+•
5. Difference in pre–post changes between honest winners and honest non-winners: (β_5)	+•••	+•••	+•••	+••	+•••	+
6. Difference in pre–post changes between fraudulent winners and fraudulent non-winners: ($\beta_5 + \beta_7$)	+•••	+	+•	+•	+	+••
7. Difference in pre–post changes between fraudulent winners and honest winners: ($\beta_4 + \beta_7$)	–•••	–••	–•••	–	–•••	+

+ $p < .10$; •$p < .05$; ••$p < .01$; •••$p < .001$.

* N = 934 for 467 firms.

fraudulent winners are associated with a lower level of pre–post increase than honest winners across all three measures of online job posts. Finally, the coefficient for the three-way interaction provides the critical evidence that accounting for the counterfactual of not receiving any capital infusion from the Innofund, grant-winning fraudulent firms are associated with a lower level of pre-to-post increase in job postings than honest grant winners. The coefficient magnitudes imply a negative difference of 62 percent for R&D-related jobs ($\beta_7 = -.61$ vs. $\beta_5 = .97$ in model 2) and of 55 percent for non-R&D jobs ($\beta_7 = -3.67$ vs. $\beta_5 = 6.70$ in model 3). Overall, the DDD specification accords with the subgroup DD results and the descriptive statistics. The one exception is H2, which is rejected in the DDs but finds limited support for R&D job postings in the more precise DDD regressions.

Empirical results on patents. We now turn to the patent-derived dependent variables. For this, we first refer to Table 2 models 4–6, which explore the DDs for firms' patenting outcomes for each of the subsamples. Panels A–C, respectively, compare fraudulent grant winners with honest winners, fraudulent winners with fraudulent losers, and honest winners with honest losers. Model 4 in panels B and C shows that grant winners are associated with a higher pre–post increase in total patents than losers, and this pattern holds for both fraudulent firms (panel B) and honest firms (panel C). However, Panel A suggests no difference in the pre–post change in total patents between fraudulent and honest grant winners ($\beta_4 = -1.09$, $p > .10$).

Recall that ''total patents'' is an obfuscated measure because the variable fails to distinguish important invention patents from more trivial utility patents. Therefore we are more interested in models 5 and 6, which separately analyze invention and utility patent counts. We find a comparison of these outcome variables in panels B and C to be particularly revealing. Panel B examines only fraudulent firms and shows that fraudulent grant winners increase post-grant utility patent filings more than do fraudulent losers ($\beta_5 = 1.82$, model 6); however, there is no increase in the rate of substantively meaningful inventions for fraudulent winners ($\beta_5 = .18$, model 5). These patterns are reversed for honest firms, among which grant winners exhibit a much larger, statistically significant, pre–post increase in invention patents ($\beta_5 = 1.62$, model 5), but there is no discernible difference in post-grant utility patent filings for honest winners relative to losers ($\beta_5 = .99$, model 6). Comparisons across types of patents and firms provide strong empirical support to hypotheses 3 and 4: fraudulent and honest firms use Innofund grants differently, and only honest grant winners convert Innofund-invested capital into technically more meaningful innovations.

Models 4–6 in Table 3 report the triple difference (DDD) specifications for the patenting outcomes in the full sample. Once again, fraudulent winners exhibit a smaller increase in invention patents relative to honest grant winners ($\beta_4 + \beta_7 < 0$, model 5), but there is no difference between these two groups in utility patents ($\beta_4 + \beta_7 > 0$, model 6). Honest grant winners increase invention patent filings more than honest losers ($\beta_5 > 0$, model 5), but there is no difference in utility patent filings between honest winners and losers. Fraudulent grant winners, conversely, significantly increase utility patent filings relative to fraudulent losers ($\beta_5 + \beta_7 > 0$, model 6), but there is no difference in invention patent filings across fraudulent winners and losers. Finally, β_7, the coefficient for the three-way interaction effect, shows that compared with the counterfactual of not having received a capital infusion from Innofund, honest grant winners exhibit a much larger pre–post increase in filings of invention patents than do fraudulent winners. Further, the fact that β_7 is of comparable magnitude as β_5 but with the opposite sign (–1.51 vs. 1.68, model 5) shows that fraudulent grant winners do not parlay Innofund grants into invention patents.

Coarsened exact matching. As a final check on the results, we employed coarsened exact matching (CEM) to address covariate balance among time-changing, observable factors. CEM is a method of pruning the sample to reduce any covariate imbalances between the treated and control groups (Iacus, King, and Porro, 2012). CEM creates a balanced sample based on observable characteristics, and it also enables us to create a sample in which

the parallel trends assumption of the DDD is fully upheld. Depending on the regression, we match on a number of dimensions, including (1) fraud or not; (2) year of Innofund grant application; (3) number of R&D positions posted (numbers of inventions filed) during the pre-grant era; (4) number of non-R&D positions posted (number of utility patents filed) during the pre-grant era; (5) a firm's geographic location at the provincial level; (6) firm age at the time of grant application; (7) broad industrial sectors as defined in the Innofund application material; (8) founder's education; (9) firm's ownership structure; and (10) political connections. We report DDD CEM results in Online Appendix C, Table C1. For brevity, we simply note that hypotheses 1, 3, and 4 continue to receive support in these estimations. This is exactly as we find in the descriptive statistic, the two-way DDs with fixed effects, the DDD with fixed effects, and the CEM estimates of the DDD. We consider the entirety of the findings to offer strong support for these three predictions.

Political connections. Previous work has established that politically connected firms are more likely to perpetrate fraud. This relationship likely exists for two reasons: political connections may shield firms from the scrutiny of regulators, and conditional on being detected, punishment may be less severe for those connected to state officials. Following prior work (Stuart and Wang, 2016), we define a firm to be politically connected when one or more of the company founders previously worked in the Chinese government (army included) or once held membership in the People's Congress or the Chinese People's Political Consultative Conference. Under this definition, 102 firms in our sample have political connections, and the remaining 365 do not. This split means that we cannot precisely estimate the DDD coefficients in the politically connected sample, but we can rerun the analyses for non-politically connected firms to verify that this source of firm heterogeneity does not account for the effects. These results appear in Online Appendix D, Table D1 and establish that the findings hold in the subsample of non-connected firms.

DISCUSSION AND CONCLUSION

A considerable body of work has established that organizations may experience a reputational loss, a stock market penalty, or legal liability if fraudulent behavior is exposed (Jensen, 2006; Greve, Palmer, and Pozner, 2010; Yenkey, 2018). Yet we have almost no understanding of how fraud affects organizations' investment choices and competitive positions when, in the vast majority of cases, fraudulent behaviors escape detection. To our knowledge, this paper is the most direct empirical examination to date of how fraudulently acquired resources are deployed differently than resources acquired through honest means. We find that fraudulent winners of state-sponsored innovation grants are much less likely than honest winners to invest grant awards in expanding the talent base of the company or to develop commercially and technically significant inventions. Our work presents evidence that fraudulent companies allocate state-supplied resources to purposes that are different from those that were broadly intended by the grant-giving entity.

Findings of this nature are essential to establish the societal cost of fraud. It seems undeniable that successful acts of fraud result in a non-meritocratic

distribution of resources across actors. Importantly, if fraud is common but virtually never detected, we risk an equilibrium in which ''fraud pays'': undetected fraud provides perpetrators with undeserved resources. But what of the allocation of these resources? We empirically demonstrate the twofold cost of fraud. First, it undermines merit in the resource acquisition process. Why put in the hard work to achieve a track record of success if you can simply present a fraudulent one? Second, fraud is associated with the misallocation of resources relative to the stated intentions of resource providers.

We employed a novel method of fraud identification. By comparing two sets of financial statements that are required by law to contain identical information, we could directly observe discrepancies that indicate manipulation of financial statements to increase the odds of acquiring a state grant or to avoid tax liabilities. This method allowed us to empirically examine how fraud is linked to investments in R&D and innovation outcomes, without the confounds that are introduced in data in cases of caught-fraud.

A few shortcomings of our work merit discussion. First, we have no knowledge of a firm's actual profit, even an honest firm's. Given that most firms in the sample have an incentive to over-report their profit to one government entity and to under-report it to another, we are confident that the majority of firms that submit the same figures in both sets of books are honest in their financial reporting. Nonetheless, even for cases of identical books, we have no way to verify the accuracy of the profit figure in the two financial reports.

A larger challenge is that the firms we label fraudulent definitely misstated one but could have misstated both of their financials. We cannot know whether the firms in our sample defrauded the tax authorities, the granting agency, or both state organizations. We consider it likely that firms that perpetrate fraud in one context (e.g., they report a deflated level of profit to the tax authority) also are more likely to be misleading in another context (e.g., they will inflate profit reports to the granting agency because it is advantageous to do so). This said, some fraction of the firms that we label as fraudulent may have been honest in the books they submitted to one of the agencies. While our empirical results report the correct overall incidence of fraud in the sample, we cannot know the extent to which fraudulent firms received additional state resources in the form of an innovation grant or received additional state resources in the form of tax relief that they were not entitled to.

We began the paper with a distinction between two types of firms: fraudulent-trait companies that commence operations expressly to defraud resource holders and fraudulent-state companies that opportunistically deceive resource holders but still pursue lawful business objectives. Which type of firm is more prevalent? The preponderance of the evidence points to the latter. First, the results are based on fixed effects estimations, so we know that any (time-constant) trait is subsumed in the firm fixed effects in the regressions, yet we still find a very robust pattern of results. Second, firms that commit fraud are on roughly parallel pre-trend lines with honest firms in the sample with regard to the dependent variables we analyze. While there are some level (versus trend) differences in the pre-grant period between honest and fraudulent firms, we believe they are small enough to suggest that fraudulent-state firms are the dominant type in our data.

Assuming that most fraudulent organizations fall into the fraudulent-state category, this raises a host of important theoretical and empirical questions. Do

successful versus unsuccessful first attempts at fraud lead to very different long-term organizational dynamics? Does the simple existence of a willingness to fraudulently mislead constituents permanently alter organizational practices, or does true change occur only if a fraudulent attempt at resource acquisition succeeds? How specifically does fraudulent conduct filter into organizational routines and stakeholders' aspiration levels? How quickly can we expect to observe an emergence of internal expertise in fraud, which may then cause the skills and discipline to invest in legitimate means of organizational development to atrophy? The point is that early acts of fraud may permeate many dimensions of organizational evolution through a variety of theoretical mechanisms.

These kinds of questions imply many testable extensions of this study. For instance, how does organizational age moderate the relationship between fraud and resource allocation? Life-cycle theories posit that in the formative stages of organizational development, top executives' power to influence vision and values is greatest, because no incumbent history exists to impede the emergence of a new culture and value system (Dickson et al., 2001). Plus, younger firms are generally less routinized and less bureaucratized (Stinchcombe, 1965; Baron, Burton, and Hannan, 1999; Sørensen and Stuart, 2000), and unethical behaviors in one part of the organization (i.e., the top management team) might well spill over to other parts of the organization (i.e., the R&D department). In contrast, when organizations age and grow, they require a more routinized division of labor and increased delegation of authority to conduct their affairs. As a result, top executives' influence on the organization's daily operations, resource allocation, and even strategic orientation is attenuated in older organizations. Thus the relationship between fraud and organizational investment decisions may be stronger for younger firms than for more mature organizations.

Another implication of our work is that in contexts in which fraud affects the acquisition of resources, it is essential to rethink the relationship between slack and innovation. A small but important set of studies has established that the presence of slack resources influences innovation, generally in a positive direction (Nohria and Gulati, 1996; Greve, 2003). Our findings show that the source of slack matters. When slack resources are fraudulently acquired, their presence clearly skews leaders' perspectives on aspiration-triggered search and, more generally, their choices regarding whether to invest or to extract these resources. This has both theoretical and empirical implications. The broad implication is that the theory of slack could be enhanced with greater attention to how slack resources are acquired. Fraud is certainly one example of a means of resource acquisition that influences investment decisions. Other conditions are pertinent as well. For example, in the recent bull market in valuations of private technology companies, young companies acquired resources at a historically unprecedented scale, and with greater ease, than in the past. Presumably, the slack resources created in this climate influence subsequent investment decisions in a manner that differs from less munificent periods.

A third direction for future work, which we believe to be of particular importance given recent examples like Theranos and WeWork, concerns the incidence of fraud in the entrepreneurial sector in Western economies. It may seem easy to compartmentalize the evidence in this paper to the case of China. After all, China is known for a high degree of corruption at this stage of market development. Although systematic data do not exist, we believe that

the market for investment in private, high potential, technology-based companies in the West is similarly replete with misrepresentations of facts and overstatements of projected financial futures to attract outside capital. In particular, entrepreneurs routinely present overly sanguine, unrealistic scenarios so that their ventures attract investment dollars (e.g., Crichton, 2015; Zaleski, Waldman, and Huet, 2016). In extreme situations, these nascent organizations may be fraudulent-trait firms, as now appears to be the case with Theranos.

While fraudulent-trait firms may be rare in advanced economies, we suspect that fraudulent-state organizations have become the norm in certain contexts, especially the market for high-potential companies that pursue institutional venture capital. To justify high valuations for their companies, entrepreneurs commonly inflate financial projections. Because exaggerated forecasts are prevalent, investors naturally respond by applying discount factors to the financial models they review. Once these norms are established in the market, honest entrepreneurs are trapped in a system in which the truthful representation of merit is penalized, because the projected returns to honest projects are compared against dishonestly overstated ones.

We know that various forms of fraudulent corporate conduct are prevalent throughout the world. The questions of how acts of fraud lead to the acquisition and allocation of resources are of truly vital social and economic importance. At a time when fundamental innovation is desperately needed to address pressing global challenges, the risk of state and market resources being wasted on firms that promise innovation and provide only window dressing is too great to ignore.

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Supplemental Material

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