Whistleblowing and Regulatory Arbitrage in Labor: Accountants of Foreign Firms in the U.S.

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Abstract

This study investigates how foreign firms in the U.S. strategically allocate accountants to minimize their exposure to whistleblowing risk following the implementation of the 2011 SEC Whistleblower Program. The results show that firms subject to the program reduce their U.S.-based accountants than control foreign firms, supporting the regulatory arbitrage hypothesis. This reduction is not accompanied by changes in non-accounting employees, sales, or investments in the U.S. Instead, firms strategically shrink their U.S. accounting teams to mitigate whistleblowing risks, particularly when they have higher corruption exposure, greater U.S. business reliance, or operate in countries with less regulatory cooperation with U.S. authorities. Further analysis reveals that firms reduce new hiring in the U.S. while retaining more senior accountants and expanding accounting teams in their home countries, particularly in regions with higher corruption and less U.S. regulatory cooperation. Finally, these firms exhibit reduced reporting quality without significantly impacting production efficiency.

Keywords: SEC Whistleblower Program, Information exposure, Regulatory arbitrage, Multinational firms, Accounting human capital

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1 Introduction

The 2011 SEC Whistleblower Program (the Program, here and after), established under the Dodd-Frank Act, is one of the most important regulatory initiatives in combating corporate misconduct. By offering substantial financial rewards and legal protections, it incentivizes individuals to report violations such as fraud and corruption that might otherwise remain concealed (e.g., Bowen et al. 2010, Berger et al. 2017, Berger & Lee 2022, Wiedman & Zhu 2023, Nan et al. 2024).[1] While empirical research has demonstrated the program's effectiveness in deterring accounting fraud among U.S.-headquartered firms (e.g., Berger & Lee 2022, Wiedman & Zhu 2023), recent theoretical studies suggest that self-interested executives may engage in opportunistic manipulation of internal information flows to minimize whistleblowing risk, potentially resulting in undesirable opacity and welfare losses (e.g., Banerjee et al. 2024, Nan & Zheng 2024). We provide the first empirical investigation into how foreign firms (i.e., those headquartered outside the U.S.) with U.S. listing status strategically organize accounting human capital across their global segments, a measurable strategy of internal information/communication manipulation aimed at reducing exposure to whistleblowing risk in response to the Program. This internal information manipulation reflects an important form of regulatory arbitrage, which leads to avoidance of legal investigations into corporate misconduct, highlighting the unintended consequences of the Program.[2]

Accounting personnel (accountants, here and after) offer a unique perspective for analyzing how firms manage internal information flow in the context of whistleblowing.[3] Their specialized knowledge of financial transactions and reporting processes positions them as key insiders with access to sensitive information on corporate misconduct. Unlike other employees, accountants can identify irregularities and discrepancies that serve as red flags for regulators, making them in-

[1] Due to regulatory resource constraints, optimal enforcement requires selective interventions based on incomplete information, placing whistleblowers in a crucial role (Stigler 1971, Dye 1986, Nan et al. 2024).

[2] Regulatory arbitrage involves exploiting differences in regulatory frameworks across jurisdictions to bypass unfavorable rules. Previous studies mainly examine how firms shift activities to regions with more lenient regulations to maximize returns(e.g., Houston et al. 2012, Ongena et al. 2013, Karolyi & Taboada 2015, Delis et al. 2024). However, they barely visit the implications of ethical or corporate governance in the presence of agency conflicts between executives and stakeholders. Arbitrage in response to regulations on corporate misconduct could reduce accountability, facilitate unethical behavior, and harm the interests of shareholders, employees, and the broader community.

[3] Common allegations in SEC whistleblowing cases include manipulation, offering fraud, and corporate disclosures, often involving financial reporting irregularities typically handled by accountants and auditors. The SEC's annual reports indicate that a significant portion are insiders. For example, in FY 2021, 60% of whistleblowers were insiders, with 75% having reported their concerns internally before contacting the SEC (SEC 2021 Annual Whistleblower Program Report to Congress).

valuable whistleblowers in detecting fraud or corruption (Graham et al. 2005, Doyle et al. 2007, Donelson et al. 2017, Gao et al. 2023). This information advantage of accountants, coupled with whistleblowing risk, is evident in many high-profile whistleblowing cases, including those involving Countrywide Financial, Halliburton, and Panasonic.[4] As a result, firms may strategically arrange their accountants to minimize the risk of internal leaks that could trigger regulatory action under the Program. We focus on foreign firms with U.S. operations for several reasons. First, the non-U.S. headquarters can effectively coordinate their global networks, naturally allowing for greater flexibility in sustaining a larger disparity between the scale of business operations and the scale of their accounting departments in the U.S., which enables them to more likely strategically allocate accountants between the U.S. and other jurisdictions.[5] Second, corporate governance norms stemming from regulatory environments in home countries differ significantly between U.S. and non-U.S. firms. The variance in these initial conditions can cause non-U.S. firm managers to have different incentives regarding transparency, potentially leading to different strategies and consequences from those observed in U.S. firms (e.g., Nan & Zheng 2024). Third, the SEC's influence on foreign firms' operations offers insights into how the spillover effects of U.S. regulators in enforcing global business practice standards materialize (e.g., Christensen et al. 2022).[6]

Ex ante, it is unclear whether the Program induces foreign firms to shift their accountants away from the U.S., as firms with different intentions may adopt various strategic responses. On the one hand, firms aimed at enhancing internal corporate governance may increase the number of U.S.-based accountants to deter internal misconduct (e.g., Gao et al. 2023). Placing more accountants in U.S. jurisdictions with stricter regulations helps firms enhance compliance and reduce fraud. This strategy can also minimize the risk of future whistleblowing when more stringent regulatory environment preempts fraud (Berger & Lee 2022, Wiedman & Zhu 2023). On the other hand,

[4]For example, an accountant whistleblower reported that Panasonic was overstating revenue, which led to both SEC and DOJ investigations. The SEC's action resulted in Panasonic paying approximately $143 million in disgorgement and interest, while additional penalties were levied through criminal action by the DOJ.

[5]In contrast, U.S.-headquartered firms are constrained as they must locate most accounting functions in the U.S., and the entire operations are subject to the Program's effects. Therefore, their ability of such type of opportunistic allocation is largely constrained.

[6]Non-U.S. firms are subject to both their home country regulations and U.S. regulations, creating a complex regulatory environment. This dual exposure can lead to unique compliance strategies not observable in U.S. firms. For example, the level of cooperation between U.S. regulatory bodies and their foreign counterparts influences the compliance behavior of non-U.S. firms. Another stream of literature examines the disciplinary effects of PCAOB international program (e.g., Lamoreaux 2016, Fung et al. 2017, Krishnan et al. 2017, Shroff 2020.) These studies emphasize more on the intended treatment effects, while less discuss the particular mechanisms through which U.S regulations induce opportunistic responses by self-interested executives.

self-interested executives may have greater incentives to distort internal communication flows to preempt the risk of information leaks when whistleblowing incentives are stronger (e.g., Banerjee et al. 2024, Nan & Zheng 2024). U.S.-based accountants are more susceptible to pressure from U.S. authorities, as they fall under the jurisdiction of the Department of Justice (DOJ) and the SEC, which possess extensive legal tools to compel testimony, access corporate records, and enforce compliance. In contrast, accountants located abroad are shielded by jurisdictional boundaries and different legal systems, making them less accessible to U.S. authorities. As a result, U.S.-based accountants are more likely to become whistleblowers, either voluntarily through the Program or via regulatory compulsion.

To test how foreign firms allocate accounting human resources in response to the Program, we focus on publicly listed firms headquartered outside the U.S. but with U.S. segments from the Bureau van Dijk (BvD) database for the period 2005 to 2015. Among them, foreign firms registered with the SEC (e.g., Christensen et al. 2022) are defined as treated firms.[7] The remaining foreign firms with U.S. operations that are not registered with the SEC and thus not subject to the Program are classified as control firms. To increase the distributional comparability between treated and control firms, we adopt an entropy balancing approach to enhance co-variate balance (Lee & Fargher 2018, Sanseverino 2023). We rely on Revelio Labs to obtain individual employee information for sample firms and aggregate the individual-level data into firm-year level observations. Relying on the difference-in-differences (DID) estimation with a tight fixed effect structure, we show that treated firms reduce their U.S-based accountants by 7.0% more than control firms following the Program's implementation, consistent with the regulatory arbitrage hypothesis.[8]

One non-mutually exclusive explanation is that treated foreign firms only reduce their U.S.-based accountants but also downsize their entire U.S. operations. To explore this possibility, we first examine changes in the size of other functional areas within these firms around the time of the Program's implementation. Our results indicate that the number of non-accounting employees in the U.S. does not significantly change after the program's introduction. Second, we analyze segment-level sales data and find insignificant impacts on sales in the U.S. or other regions. Third,

[7]We determine the registration status of foreign firms based on their SEC filings. Treated firms are identified as those filing Forms F-1, F-3, F-4, F-6 for the direct registration of their securities with the SEC. In addition, foreign firms are treated if they submit Form 20-F as their annual reports.

[8]We include firm, home country-year and industry-year fixed effects to account for time-invariant or slow-moving heterogeneity across firms, time-varying home country characteristics, and time-varying industry-level factors.

we investigate firms' merger and acquisition (M&A) activities globally and find no evidence that treated firms engage in fewer M&A deals in the U.S. or more elsewhere following the Program's implementation. Collectively, these findings suggest that treated firms do not engage in costly adjustments of their broader operations but instead simply reduce their allocation of accounting human resources in the U.S. to mitigate whistleblowing risk exposure.

To strengthen our inference that firms mitigate whistleblowing risk by reducing the size of their U.S. accounting departments, we explore three relevant international business contexts where the impact of the Program is expected to be more pronounced. We first assess cross-sectional variation in firms' exposure to corruption, a proxy for propensity for firms to engage in misconduct. Self-interested executives of these firms are more concerned about the increased regulatory scrutiny, and thus take more aggressive steps in allocating their U.S.-based accountants. Second, firms heavily reliant on a particular region for income are more vulnerable to regulatory actions from that region (Efing et al. 2023). Therefore, firms with a greater U.S. market exposure would have to engage in more opportunistic allocation of their accountants to mitigate whistleblowing risk in the U.S. Third, we explore the international corporate governance coordination by assessing the level of cooperation between U.S. regulators and the regulatory bodies of the firm's home country. The SEC frequently collaborates with foreign regulatory authorities to collect evidence and enforce compliance (Silvers 2020). One important mechanism for such international coordination is the Memoranda of Understanding (MoUs).[9] Operations in jurisdictions with a strong collaboration with U.S. authorities, in the form of MoUs, may face a similar enforcement risk as those in the U.S. Therefore, opportunistic allocation of accountants to preempt U.S regulators' enforcement actions becomes less feasible when authorities of operating regions have a strong collaboration with U.S. authorities. Our findings support these cross-sectional predictions.

Downsizing accounting functions can be achieved by either reducing new hires (inflow) or increasing departures of existing employees (outflow). In the context of whistleblowing risk, the Program has different implications for inflows and outflows. Hiring new accountants who lack established loyalty to a firm but will possess sensitive information in the near future may increase whistleblowing risk, while firing incumbent accountants, who already possess sensitive information,

[9]MoUs are formal agreements that establish cooperative frameworks to facilitate bilateral activities. A primary function of these MoUs is to exchange critical information.

could significantly elevate whistleblowing risk, as any preexisting loyalty or bonds between the firm and departing employees would be ruined. Therefore, we examine both hiring and departure patterns, which not only provide insight into the specific human capital decisions made by treated firms but also strengthen our causal inference, as alternative explanations are unlikely to account for these contrasting patterns. Consistent with our predictions, we find that treated firms significantly reduce their U.S. hiring activities following the Program. At the same time, these firms are more likely to retain incumbent accountants within their U.S. segments to mitigate whistleblowing risk. However, since the reduction in new hiring activities is more substantial, we observe a net decrease in the overall size of U.S.-based accounting departments.[10]

To further explore this nuanced effect, we categorize accountants based on their seniority: entry-level and non-entry-level (i.e., higher-ranked) accountants. Entry-level or junior accountants, who often lack firm loyalty, could exhibit a higher propensity to whistleblow. In contrast, non-entry-level or senior accountants, who typically have stronger bonding to a firm, exhibit a lower propensity to whistleblow. However, their possession of sensitive information makes the consequences of their departure more severe for the firm if the bonding is broken through job termination. Consistent with these interesting context-specific features, our empirical results show that treated firms significantly reduce U.S. hiring for both junior and senior accountants following the Program's implementation. However, in terms of retention, treated firms focus on retaining more senior accountants, likely to mitigate the risk posed by their sensitive knowledge.

Since the growth of the accounting department should align with overall business expansion, foreign firms that have reduced their U.S. accounting teams often seek substitutes in other regions. A convenient alternative is their home country's accounting teams. We thus test whether these foreign firms increase the size of their home or other countries' accounting teams after downsizing in the U.S. To this end, we shift the unit of analysis to the employee level, allowing for a more precise identification of accountants' locations following the Program's implementation. Our employee-level analysis yields two primary findings: First, the results reaffirm our firm-level findings that treated firms hire fewer new U.S.-based accountants but increase the hiring of non-U.S.-based accountants. Second, treated firms do not show a significant bias towards expanding their home-

[10]We note that a decrease in inflow could also stem from a lack of available accountants in the U.S. labor market. Therefore, we directly investigate the accounting related job posting in U.S. and demonstrate that the effect is indeed driven by demand side effect.

country accounting teams compared to their accounting teams in other foreign countries unless the home countries are with higher exposure to corruption or with weaker collaborative ties with U.S. regulators, as indicated by the lack of MoUs. These individual-level patterns provide additional support for our previous firm-level cross-sectional tests, reinforcing our causal inferences.

Last, we address how the strategic allocation of accounting human capital following the Program's implementation affects the corporate informational environment. Several theoretical works propose that the opportunistic executives seek to manipulate the internal information flow and reduce the exposure to US regulatory risk, thereby leading to low financial reporting quality (Banerjee et al. 2024, Nan & Zheng 2024). We provide suggestive empirical evidence that treated firms' financial reporting quality, measured by discretionary accruals and F-score, decreases following the implementation of the Program, particularly among those with larger U.S. operations, which we document with more intensive management of their U.S. accountants. Two recent papers (Berger & Lee 2022, Wiedman & Zhu 2023) find that U.S.-headquartered firms experience an improvement in financial reporting quality after the Program's implementation. As theoretically demonstrated in Nan & Zheng (2024), our findings complement rather than contradict these results, highlighting a different equilibrium: when self-interested executives have the flexibility to act strategically, as in the case of foreign firms with more complex global networks beyond the U.S. than U.S.-headquartered firms, their actions may lead to a more distorted informational environment, undermining the effectiveness of the program.[11]

2 Contribution

Our work is related to the firms' cross-country regulatory arbitrage behaviors (e.g., Houston et al. 2012, Ongena et al. 2013, Dyreng et al. 2015, Karolyi & Taboada 2015, Delis et al. 2024). Much of the literature does not visit the agency conflicts between shareholders and managers and emphasizes how firms seek to maximize their investment returns in response to government regula-

[11] An alternative way to view the issue is through the two potential outcomes modeled in Nan & Zheng (2024). U.S.-headquartered firms, typically with good pre-existing corporate governance, are expected to see a moderate increase in whistleblowing risk following the Program's implementation. These firms would exhibit a positive net treatment effect. In contrast, foreign firms, often with relatively weaker corporate governance, are projected to experience a significant increase in whistleblowing risk and cause extensive managerial manipulation of internal information, thereby having a net negative impact. Our results in Appendix C support this nonlinear relation.

tions.[12] For example, Houston et al. (2012) find that credit flows are reallocated from countries with stricter regulations to those with lenient ones. Ongena et al. (2013) suggest that stricter regulations push firms to lend to riskier borrowers, while Karolyi & Taboada (2015) show that cross-border acquisitions are drawn to markets with fewer regulations. Dyreng et al. (2015) highlight how tax costs affect international investments and Delis et al. (2024) document the global profit shifting driven by maximizing tax savings. In contrast, we focus on self-interested executives who seek to maximize their own interests in a different regulatory context, the Program. This program has significant *ethical* and *corporate governance* implications. The strategic allocations of accountants among geographic segments by multinational corporations to distort internal information flow and avoid legal investigation is a less-explored dimension of cross-country regulatory arbitrage related to obstructing investigations into corporate misconduct. Two recent studies test the effect of the Program on U.S.-headquartered firms, focusing on its intended goal of enhancing reporting quality (Berger & Lee 2022, Wiedman & Zhu 2023). Unlike U.S. domestic firms, which have limited flexibility in strategically allocating their accounting teams and with relatively better pre-existing corporate governance, foreign firms are more likely to be induced to utilize their global networks that offer greater flexibility in accountant allocation and allow for a larger disparity between the scale of their U.S. accounting operations and U.S. business activities. This context enables us to empirically demonstrate the opportunistic actions of self-interested executives and the unintended consequences of the program (which are intended by the executives). This aspect has been largely overlooked in prior empirical studies of whistleblowing, yet theoretically, it has been proven to be significant (Banerjee et al. 2024, Nan & Zheng 2024).

Our study also contributes to the literature on the labor market for accounting professionals. One stream of studies focus on how CPA regulations in the U.S. affect labor supply and service quality. For example, Cascino et al. (2021) examine the impact of CPA Mobility Provisions, while Barrios (2022) and Sutherland et al. (2024) study the effects of the 150-hour rule on the accounting industry. Another group of studies explore the negative consequences of abnormal turnover among accountants in the U.S., such as firm-specific capital, routines, and performance (e.g., Aobdia et al. 2018, Kerckhofs et al. 2024, Lin et al. 2024, Ma et al. 2024). Our study offers a new perspective

[12]The literature focuses on the concern that whether these actions could lead to a *race-to-the-bottom* in regulations. The "race-to-the-bottom" is a concept where jurisdictions compete against each other by lowering regulatory standards to attract businesses, leading to progressively lower standards.

on firms' opportunistic decisions regarding the allocation and retention of accountants to preempt regulatory investigations following the Program's implementation, particularly in international contexts. We highlight a less-explored yet critical aspect of regulatory arbitrage in the labor market for accountants: firms reduce new hires, focus on retaining incumbents, and shift from U.S.-based to home-country-based accountants to mitigate whistleblowing risks.

Last, our study contributes to the literature on the impact of U.S. regulatory enforcement on international business practices. Earlier empirical research on how U.S. regulators shape international corporate governance centers on the "bonding effect" that firms from countries with weaker investor protections "bond" themselves to stronger legal and regulatory environments by cross-listing (e.g., Doidge et al. 2004, Lang et al. 2003, 2012). Recent studies shift the focus to the extraterritorial disciplinary effect and examine the PCAOB international program (e.g., Lamoreaux 2016, Fung et al. 2017, Krishnan et al. 2017, Shroff 2020) or global enforcement of the Foreign Corrupt Practices Act (FCPA) (e.g., Christensen et al. 2022). Our study instead focuses on the opportunistic actions of international firms and examines the *unintended* extraterritorial treatment effects of the Program on the opportunistic allocation of accountants globally by these firms.[13] By examining the incremental effect of the Program, we provide additional insights into how U.S. regulatory frameworks influence foreign firms' corporate governance practices, in the context of whistleblowing risk and corporate transparency.

3 Institutional Background

3.1 The Whistleblower Program

The Dodd-Frank Wall Street Reform and Consumer Protection Act (Dodd-Frank Act) was signed into law on July 21, 2010. This legislation introduced groundbreaking measures to enhance the Whistleblower Program.[14] Specifically, Section 922 of the Dodd-Frank Act introduced new

[13] An interesting aspect of the Program is its intersection with the FCPA, which extends U.S. regulatory reach internationally. Together, they strengthen corporate governance enforcement by deterring foreign firms from engaging in corrupt practices and ensuring consistent compliance. As Andrew Ceresney, former SEC Enforcement Director, noted, FCPA violations became an"*increasingly fertile ground*" for whistleblowers under the Program. While the FCPA's effects predated the program, we use a difference-in-differences framework to isolate the incremental impact of the Program. This combined enforcement mechanism enhances the SEC's role in regulating global business and combating international corporate misconduct.

[14] This program began to take shape in 2009 and became legally effective as part of the Dodd-Frank Act in 2010. We list the important milestones in Appendix E.

Section 21F, titled "Securities Whistleblower Incentives and Protection." Section 924(d) mandated the SEC to create a separate office to oversee the whistleblower bounty program. This program offered financial awards for whistleblower tips and aims to obtain high-quality information on securities law violations to better detect and prevent fraud. Individuals with information regarding potential securities law violations can submit tips to the SEC.[15]

The Program encourages employees to report wrongdoing to external regulators. The Program mandates that the SEC provides financial rewards to whistleblowers who offer original information leading to successful enforcement actions with monetary sanctions exceeding $1 million. The reward amounts range between 10% and 30% of the collected sanctions. Further, the Program explicitly prohibits retaliation by employers against whistleblowers and provides a private cause of action for whistleblowers who face discrimination or termination due to their reporting activities. These provisions represent an unprecedented commitment to protecting and incentivizing whistleblowers, ensuring that individuals who report corporate misconduct receive substantial financial rewards and are shielded from retaliation. This innovative approach aims to encourage more individuals to come forward with critical information, thereby bolstering the SEC's ability to detect and prosecute misconducts effectively.

The SEC prioritizes "original information" that has the potential to trigger investigations. To qualify as original information, it must meet the following criteria: (i) it is derived from the whistleblower's independent knowledge or analysis; (ii) it is not already known to the SEC from any other source; and (iii) it is not solely based on allegations made in a judicial or administrative hearing, a governmental report, hearing, audit, investigation, or from the news media. Accountants typically have close access to information on various corporate activities. Their proximity to daily operations and the inner workings of the organization provides them with insights and data that are not readily available to external parties. This unique position enables accountants to play a critical role in uncovering and whistleblowing misconduct.

[15]The Program came into effect in August 2011, but the whistleblowers were eligible for an award for original information submitted on or after July 22, 2010. Therefore, we use Obama's signing of the Dodd-Frank Act as an exogenous shock for the increase in whistleblowing risk. This marks the point at which firms begin to take action, which corresponds with the dynamic effect observed in our study.

3.2 Challenges of International Enforcement

Note that the Program is not limited to U.S. nationals; rather it also applies to foreign individuals who report violations of U.S. securities laws. This global reach enhances the likelihood of detecting violations occurring outside the U.S. Despite its global reach, its effectiveness faces challenges, such as legal disputes over the scope of protections offered to non-U.S. whistleblowers and the restricted SEC international corporations. These issues can affect the Program's impact on international enforcement (Westbrook 2018).

First, while the Program incentivizes reporting of violations, its protections are limited in scope for employees outside the U.S. Specifically, under the Sarbanes-Oxley Act (SOX), protections against retaliation are provided for whistleblowers, but these protections generally apply only to U.S. citizens or employees within the U.S. or its territories. For instance, non-U.S. citizens working abroad for U.S. entities are not covered by these protections enforced by the Department of Labor (DOL), or U.S. Equal Employment Opportunity (EEO) laws, a typical stance supported by DOL regulations and reinforced by various court rulings.[16] In addition, non-U.S. employees may often face criminal or civil penalties under their home country's laws for sharing sensitive or classified information with foreign entities, such as the SEC, even if it pertains to illegal activities. Information from U.S. employees, however, is more easily validated and acted upon by the SEC to streamline its investigations. Anecdote evidence suggests that the SEC frequently requires U.S.-based employees, more so than their non-U.S.-based counterparts, to cooperate in investigations concerning non-U.S. firms.[17]

Second, cross-border flows of human capital create great challenges for the SEC. The scope of SEC enforcement activities has to expand to encompass a broad range of international players.[18]

[16] Individuals who are not U.S. citizens are not covered by U.S. Equal Employment Opportunity (EEO) laws when employed outside the U.S. or its territories. For more details, visit: https://www.eeoc.gov/laws/guidance/employee-rights-when-working-multinational-employers

[17] For example, in the Magyar Telekom case, the SEC used U.S.-based resources to investigate the illegal activities of the Hungarian telecommunications company, who bribed Macedonian government officials in connection with its business operations. Moreover, in the Siemens case, the SEC repeatedly highlighted the role of U.S.-based individuals in the complaint and discovered that Siemens attempted to avoid U.S. regulation by having its U.S. subsidiaries' bribery activities mediated through its headquarters in Germany.

[18] Section 21(a)(2) of the Securities Exchange Act of 1934 authorizes the SEC to conduct investigations on behalf of foreign securities authorities and compel the production of documents and testimony from any person or entity, whether or not that person or entity is regulated by the SEC. Section 3(a)(50) of the Securities Exchange Act of 1934 defines a "foreign securities authority" as "any foreign government or governmental body or regulatory organization empowered by a foreign government to administer or enforce its laws as they relate to securities matters."

This includes two main categories: (1) non-U.S. firms and their representatives allegedly violating U.S. securities laws domestically, and (2) U.S.-based entities and their executives suspected of securities violations abroad that fall within U.S. jurisdiction. Such investigations present challenges involving jurisdictional boundaries, legal privileges, data privacy considerations, and require extensive collaboration with various stakeholders including corporate entities, foreign securities regulatory bodies, and international law enforcement partners to gather evidence beyond U.S. borders (Erie 2019, Arrieta 2016). When dealing with foreign entities or individuals lacking substantial connections to U.S. jurisdiction, the SEC's subpoena authority is substantially restricted.[19]

4 Empirical Predictions

Due to regulatory resource constraints, optimal enforcement necessitates that regulators engage in selective interventions based on incomplete information (Stigler 1971, Dye 1986). In this context, whistleblowers play a crucial role by not only providing regulators with early warning signals but also supplying detailed and precise evidence that facilitates direct legal action against offending firms (Dyck et al. 2010). However, career risks and personal safety concerns can significantly deter insiders from coming forward. Therefore, an effective governance mechanism through whistleblowing remains a substantial challenge (Dyck et al. 2010, Nan & Zheng 2024).

The Program aimed to this issue and improve fraud detection and prevention by enhancing whistleblowing incentives. However, opportunistic executives may manipulate internal information and communication to minimize whistleblowing risk when they become aware of the increased whistleblowing incentives for lower-level managers (e.g., Banerjee et al. 2024, Nan & Zheng 2024). Access to information is crucial for potential whistleblowers, as successful tips must provide original evidence that supports the SEC's investigation (Dyck et al. 2010). The SEC stresses that tips identifying individuals, detailing fraudulent transactions, or providing non-public evidence are more likely to trigger investigations. Accountants, with their specialized knowledge of transactions and reporting, play a crucial role as whistleblowers in exposing fraud. Therefore, internal information flow and communication manipulations are likely associated with accountants, who are central to financial processes and well-positioned to detect misconduct.

[19] A foreign citizen in a foreign state refused to comply with an investigative subpoena issued by the SEC. Information source: [CFTC v. Nahas, 738 F.2d 487, 491 (D.C. Cir. 1984)]

We expect that, given the crucial role of accountants, opportunistic executives have stronger incentives to reduce accountants' interactions with U.S. regulators. As multinational firms, foreign firms with U.S. operations can leverage their global operational network to strategically allocate accountants in ways that minimize whistleblowing risk. Specifically, extraterritorial enforcement by U.S. regulators is hindered by differing international laws, limited cooperation from foreign entities, and challenges in enforcing subpoenas across borders, which lead to delays and reduced enforcement efficiency (Westbrook 2018). Foreign firms, facing an increase in whistleblowing risk, can exploit this global flexibility by downsizing their U.S.-based accounting teams to reduce regulatory exposure. Therefore, we expect that foreign firms regulated by the SEC are more likely to downsize their accounting operations in the U.S., compared to those that are not regulated by the SEC.

Our prediction, however, is not without tension. If managers are not opportunistic and seek enhance internal corporate governance in response to the increase in whistleblowing risk, they may increase the number of U.S.-based accountants to deter misconduct. Accountants in the U.S. are held to more stringent standards and strict ethical guidelines. This strategic action strengthens internal controls, improves monitoring of financial transactions, and facilitates early detection of irregularities ex post (Gao et al. 2023). As a result, the presence of these accountants under stringent U.S. regulations may act as a deterrent against unethical behavior (DeFond & Francis 2005). With an opposite prediction regarding the allocation of accountants, these firms enhance compliance, reduce fraud, and thereby, minimize future whistleblowing risk. Given the competing forces as discussed, how the SEC Whitsleblower Program affects foreign firms in the U.S. allocate their U.S.-based accountants is an empirical question.

5 Sample and Research Design

5.1 Sample

To construct our sample of foreign firms, we start by retrieving all publicly listed firms headquartered outside the U.S. from the Bureau van Dijk (BvD) Osiris database for the period 2005 to 2015 surrounding the Program. We use 2005 to 2015, which we divide into a pre-period (2005–2010) and a post-period (2011–2015). The Program was enacted under the Dodd-Frank Act on July 21,

2010.[20] We select a sample period of five years before and after this event. As summarized in Panel A of Table 1, we require that the firms have at least one segment in the U.S., resulting in a sample of 7,251 firms. We then match them with the firms jointly covered by Revelio Labs to obtain our sample firms.[21] We exclude firms registered with the SEC after 2010 when the Program was initially enforced and those with no U.S. employee records before 2010.

To empirically identify our treated firms for the Program in our sample, we focus on those registered with the SEC among the foreign firms. Following Christensen et al. (2022), we determine the registration status of foreign firms based on their SEC filings. Specifically, treated firms are those filing Forms F-1, F-3, or F-4 for the direct registration of their securities with the SEC. Further, treated firms may file Form F-6 for the registration of American Depositary Receipts (ADRs). Lastly, treated firms could submit Form 20-F as their annual reports. The remaining foreign firms of our sample are classified as control firms, as they have segments in the U.S. but are not directly impacted by the Program. Our final sample includes 2,615 firms with 25,845 firm-year observations and 41 million employee-firm-year employment records with 965 treated firms and 1,650 control firms, respectively.

5.2 Research Design

To examine the impact of increased exposure to whistleblowing driven by the Program, we use the 2005-2015 period to compare the pre- and post-periods of the whistleblowing provision enacted in 2010 and estimate the following DID regression model for firms indexed by i, home countries indexed by c, and years indexed by t:

$$US\ Acct_{i,c,t} = \beta_0 + \beta_1(Treated_i \times Post_t) + Controls_{i,t-1} + \gamma_i + \eta_{c,t} + \tau_{s,t} + \varepsilon_{i,c,t}, \quad (1)$$

[20] A whistleblower could be eligible to receive an award if the original information provided to the Commission conforms to the rules for information submitted on or after July 22, 2010. More information available at https://www.sec.gov/spotlight/dodd-frank/whistleblower.shtml and Appendix E.

[21] We match employer names from Revelio Labs with company names in BvD Osiris. Initially, we obtain all firms headquartered outside the U.S. and their current and former names in English and in their home-country languages from BvD Osiris. Next, we match the names from BvD Osiris to Revelio Labs using a fuzzy matching score of 90 or higher. For each employee's position record in Revelio Labs, we identify the best matching firm from our sample firms. If an employee holds multiple positions within a given year, we retain the position with the highest seniority as her primary job. We exclude employees with missing job start dates, ultimately identifying 56 million employee-firm-year records.

whereas our key dependent variable is the size of a firm's accounting department in the U.S. (*US Acct*). We identify employees in a firm's accounting department based on their O*NET job codes.[22] Next, we determine the location of each accountant, whether in the U.S. or abroad, and calculate the ratio of the size of a firm's accounting department in the U.S. to its total number of accounting employees globally. Compared to simply counting the number of accountants in the U.S., this ratio better controls for heterogeneity in firm size across different entities, including that between treated and control firms.[23] *Treated* is an indicator variable that equals one for our treated firms, and zero otherwise. *Post* equals one for the years following the enforcement of the Program. It is important to note that a whistleblower is eligible to receive an award for original information provided to the SEC on or after July 22, 2010, even though the program's official effective year is 2011. Therefore, we consider 2010 as a year impacted by the program as well.

We include three sets of fixed effects in Equation (1): firm (γ_i), home country-year ($\eta_{c,t}$), and industry-year ($\tau_{s,t}$). The firm fixed effects account for time-invariant or slow-moving heterogeneity across firms, such as firm culture, historical strategic choices, or industry reputation. The country-year fixed effects account for time-varying home country characteristics (e.g., law enforcement, economic conditions, and uncertainties in international political relations), recognizing that international enforcement, for example, is dynamic and influenced by political cycle and local conditions (Cohen & Li 2022). The industry-year fixed effects control for time-varying industry-level factors, such as business cycles. The effects of *Treated* and *Post* are absorbed by these effects.

We are interested in β_1, the coefficient on the interaction term between *Treated* and *Post*. It captures the effect of the Program on the changes in the size of treated firms' U.S. accounting departments. We expect it to be negative if a firm tends to downsize its accounting department in

[22] We define the positions with the following O*NET codes as accountants: 13-2011 "Accountants and Auditors", 13-2051 "Financial Analysts", 43-3011 "Bill and Account Collectors", 43-3021 "Billing and Posting Clerks", 43-3031 "Bookkeeping, Accounting, and Auditing Clerks", and 43-3051 "Payroll and Timekeeping Clerks" (Awyong et al. 2024).

[23] One potential concern is that professional profile records may be more incomplete in non-U.S. regions compared to those in the U.S., which could introduce measurement error into the ratio-based variable. However, as this potential bias is unlikely to be correlated with the introduction of the SEC program (treatment vs control as well as pre-period vs post-period), we do not anticipate that it will bias our difference-in-differences estimates. Consistent with this, we find that the distribution of accountants among our sample firms is similar between those located within the U.S. and those located globally. (See Appendix D). Moreover, our results remain robust even when we exclude firms from (home) countries where professional profile data is more likely to be incomplete. We identify these countries based on whether LinkedIn users comprise less than 20% of the labor force with advanced education, given that LinkedIn is one of the most popular online platforms for professional profiles. Our results continue to hold even after excluding firms from Japan, which accounts for the largest number of observations in our sample.

the U.S. following the program.

In our full model, We control for firm characteristics as outlined by Christensen et al. (2022), including profitability (*ROA*), degree of internationalization (*Foreign Biz*), and total revenues (*Sales*). We measure these control variables one year prior to *US Acct*. Appendix A provides more detailed variable definitions. We winsorize all continuous variables at 1% and 99% to mitigate the influence of outliers. Standard errors are clustered at the home country level to account for within-country correlation over time not captured by firm fixed effects alone. For example, firms in the same country face similar tax policies or economic trends that could influence their outcomes similarly. This clustering approach helps ensure that standard errors are not underestimated and mitigates the concern of potential inefficient estimates (Cameron & Miller 2015, Petersen 2008).

6 Empirical Results

6.1 Sample Description

Panel B in Table 1 presents a comparison of the yearly distribution in our final sample with that of those after requiring non-missing values of control variables. Panel C presents a similar comparison but of the industry distribution, and Panel D for a home country distribution. These distributions of our sample largely resemble those of the sample before we consider the U.S. employee record from Revelio Labs, indicating that, overall, our sample reasonably represents most of the other foreign firms with U.S. operations that U.S. employee data available for empirical analysis.

6.2 Univariate Analysis and Entropy Balancing

Table 2 summarizes the distributional properties of variables used in our baseline analysis. Panel A shows that the mean of *US Acct* is 0.228, indicating that accountants located in the U.S. account for about 22.8 percent of all accountants of a foreign firm, representing on average 11.5 accountants per firm (untabulated).[24] The *Post* indicator averages 0.565, representing observations are roughly equally across the pre- and post-event periods.

Panel B calculates univeriate DID of *US Acct*. Consistent with our prediction, *US Acct* of treated firms decreases from 26.3% to 23.6% after the event, representing a 11.8% reduction relative to the mean value of *US Acct* (=[26.3% −23.6%]/22.8%). Importantly, this effect continues to hold

[24]Untabulated results indicate that, on average, there are 25.9 accountants located in the U.S. for treated firms and 2.3 for control firms.

when we use the change in *US Acct* of control firms as the benchmark. Specifically, the decrease in *US Acct* for our treated firms is more than twice as large as that observed in the control firms (=[-0.027]/[-0.013]).

Panel C provides a more detailed comparison of other firm characteristics between our treated and control firms. We note that there exists a systematic pre-period difference between treated and control firms, consistent with the notion that whether a foreign firm is registered with the SEC is not random. As Panel C indicates, treated firms are larger (*Sales*), perform better (*ROA*), and have a larger global network (*Foreign Biz*).[25]

To alleviate the concern about the distributional difference between treated and control firms, we adopt an entropy balancing approach, which allows for co-variate balance in our setting with a binary treatment (Lee & Fargher 2018, McMullin & Schonberger 2022, Sanseverino 2023). Specifically, we estimate the weights for each observation in the control sample within each year prior to 2010 such that the mean, variance, and skewness of the covariate distributions of the weighted control sample are nearly identical to those of the treated sample. Panel D presents the effectiveness of the balancing process.[26] We find the post-entropy balancing distributional properties of treated and control covariates are almost identical. We apply this post-entropy balanced sample for the regression analyses next. Our robustness results presented in Appendix B show that our results are not driven by applying this balancing approach since our results also hold if we use the unweighted sample, or the alternative weighting/matching methods.

6.3 Regression Results

Table 3 presents the estimation results of Equation (1) with entropy balanced weights. Following Jennings et al. (2024), we report results across a variety of fixed effect structures, including specifications that exclude fixed effects and other time-variate control variables altogether (Gormley & Matsa 2014, Imbens & Rubin 2015). Specifically, we begin by estimating a model without any control variables or fixed effects (Column 1). Next, we incorporate firm and home country-by-year fixed effects without control variables (Column 2). In Column 3, we introduce additional

[25] Untabulated results show that firms in our sample tend to be bigger and have a larger employee base than the average foreign firms registered with the SEC. While our findings may not be generalizable to smaller firms, they hold implications for a population of large and economically significant foreign firms with U.S. operations (e.g., Dehaan et al. 2023).

[26] In Panel D, we re-weight the observations within the control sample based on the mean value of the annual-based weights previously obtained for each firm. We use these averaged weights for the remaining regression analyses.

industry-by-year fixed effects, and finally, in Column 4, we estimate the full model, which includes both control variables and the complete set of fixed effects.

We find that the coefficient on *Treated*× *Post* is statistically significant across all columns. More importantly, as fixed effects and control variables are progressively added, the coefficient appears to stabilize. Our findings suggest that firms subject to the Program experience a reduction in *US Acct* following the program's implementation than control firms. This effect is economically meaningful in two respects. Taking the one in Column 4 as an example, first, -0.16 represents a 7.0% reduction relative to the mean value of *US Acct* (=-1.6%/22.8%). Second, if we follow Breuer & DeHaan (2024) and consider the benchmark of the within-fixed effect standard deviation of *US Acct* for treated firms (0.092, untabulated), treated firms reduces their U.S. accounting departments by 17.4% (=-1.6%/9.2%).[27] This result is consistent with our hypothesis that firms facing increased whistleblowing risks may reduce their U.S. accounting presence to limit potential information exposure.

6.4 Parallel Trends Assumption

The critical identification assumption underlying our research design is the trends of firms' U.S. accounting department sizes would be the same in the absence of the Program. To assess this assumption, we introduce year-specific indicators to examine the effect of the Program both before and after its implementation. Specifically, in our primary specification in Equation (1), we replace the interaction of *Treat* and *Post* with interactions between *Treat* and the year-specific indicator variables, using 2009—the year prior to the program's enactment—as the reference year. We then plot the coefficient estimates of these interaction terms along with 90% confidence intervals. Figure 1 illustrates the trend of counterfactual treatment effects on *US Acct* from 2005 to 2015. We find that coefficients on the interactions of *Treat* and the four pre-period year indicators (from 2005 to 2008) are all insignificant. The trends for treated and control firms are parallel before the enforcement of the Program, as indicated by the estimated treatment effects during the pre-treatment period, which are not significantly different from zero, suggesting that there is no significant difference in the time trends of exporting behavior between the treated and control samples before 2010. Thus, the parallel trends assumption, a key assumption underlying our analysis, is unlikely to be violated.

[27]Note that the within-fixed effect standard deviation of *US Acct* is the residualized standard deviation of *US Acct* after considering firm fixed effects, home country x year fixed effects, and industry x year fixed effects.

Further, we observe significant coefficients on interactions of *Treat* and all the six post-period year indicator variables. The shift between the pre- and post-trend indicates that the Program reduces the proportion of U.S. accounting employees within the treated firms. Although the differences between the coefficients of 2010, 2011, and 2012 are not statistically significant at the conventional level, the figure also demonstrates that firms rapidly adjust their accounting department size in the U.S., in particular with a larger effect in 2011. This is consistent with the fact that, although a whistleblower is eligible to receive an award for providing original information to the SEC on or after July 22, 2010, the effect is expected to become more pronounced after the SEC approved the final rules implementing the Whistleblower Program on May 25, 2011, which took effect on August 12, 2011. In sum, the impact of the Program on the size of U.S. accounting departments emerges only after 2010 and persists throughout the post-implementation period.

6.5 Is It A General Downsizing of U.S. Operations?

One may be concerned that our results merely reflect a general downsizing of U.S. operations among treated firms following the program's implementation, with the decrease in accounting department size being a confounding factor. It is worth noting that a general downsizing of U.S. operations could also represent a valid response to concerns over whistleblowing risk. However, such a strategy would be a more drastic business decision, as it might significantly impact the firm's overall business strategy. Nonetheless, we conduct three sets of tests to address this question.

First, we examine the change in the size of other functions within treated firms around the time of the program's implementation. If our baseline results reflect a confounding effect of a general downsizing of U.S. operations, we would expect to observe a similar reduction in the size of the functions other than accounting. To test this, we re-estimate Equation (1) with the dependent variable based on a firm's total number of non-accounting employees in the U.S. scaled by its total global non-accounting employees (*US Others*). Row 1 in Panel A of Table 6 shows that the regression coefficient on *Treated* × *Post* is not statistically significant at conventional levels, suggesting that the size of non-accounting employees in the U.S. does not experience a significant change following the enforcement of the Program. Importantly, Row 2 in Panel A shows that the results of insignificant change in non-accounting employees remain unchanged when we randomly select individuals from non-accounting employees, matching the size of the accounting departments

in each firm prior to the program, and track their turnover throughout the testing period. In sum, our findings suggest that the reduction in accountants in our treated firms does not coincide with the changes in the number of employees in other functions within the firms. This is consistent with the notion that firms perceive accountants to be more exposed to private information and thus more sensitive to whistleblowing risks.

Second, we analyze firms' segment-level sales data from BvD Osiris and assess the distribution of the firms' business activities in the U.S. and other regions. To identify potential shifts in business operations within the U.S. relative to other regions, we use the proportion of U.S. sales to total sales across all regions of a firm (*US Sales*). The results in Row 1 of Panel B in Table 6 show no significant changes in the U.S. sales relative to a firm's total sales. Further, the results in Rows 2 and 3 indicate that the program does not significantly affect sales in either the U.S. or other regions when we decompose total sales into U.S. and non-U.S. sales (*Ln($ US Sales)* and *Ln($ NonUS Sales)*), respectively. Taken together, our findings suggest that the reduction in accountants in the U.S. is unlikely to be driven by a decline in business operations, and the Program does not result in a significant change in overall business operations.

Third, we focus on a more specific investment decision, a firm's merger and acquisition (M&A) activities globally and examine whether our treated firms vary their cross-border investments following the Program's implementation. Empirically, we construct two variables based on the data from BvD Orbis M&A: *US M&A* and *Non-US M&A* represent the number of M&As conducted by a firm, where the target firms are incorporated in the U.S. and outside the U.S., respectively.[28] We re-estimate Equation (1) with these dependent variables. The results of Rows 1 and 2 in Panel C suggest that the treated firms do not engage in more M&A deals in either the U.S. or other regions following the program.

One may argue that the change in the size of the U.S. accounting departments of treated firms could be due to shifts in their risk preferences regarding M&A targets following increased exposure to corruption (Lynch 2023). For example, the treated firms might reduce their accounting departments because they no longer plan to engage in high-risk M&As as before. We follow Lynch (2023) and classify M&As as high- or low-risk based on whether the target firms are in high- or

[28] BvD Orbis M&A only provides the firms' incorporated countries. Therefore, we use the incorporation country address to identify the location of a target firm.

low-risk countries. Rows 3 and 4 in Panel C report the results. Again, we do not find the results are consistent with the notion that the reduction in accountants in the U.S. is not confounded by potential changes in a firm's risk preference in M&A deals, and the Program does not result in a significant change in the firm's risk preference.

Taken together, our results in Table 6 suggest that although a general downsizing of U.S. operations could represent a valid response to concerns over whistleblowing risk, our treated firms do not alter their real business strategies, including the number of non-accounting employees, the size of operations, or M&A decisions.

6.6 Cross-Sectional Analyses

To strengthen our inference that foreign firms mitigate whistleblowing risk by reducing U.S. accounting department size, we examine three contexts where the Program's impact is more pronounced: firms' corruption exposure, U.S. market importance, and international collaborations. These cross-sectional tests provide supporting evidence for the mechanism linking firms' incentives to reduce exposure risk and their decision to downsize U.S. accounting departments, while addressing concerns about omitted variables or alternative explanations(Angrist & Krueger 2001). Table 4 reports the results.

First, since the international businesses are particularly sensitive to corruption, we conduct a cross-sectional analysis of firms' corruption exposure levels as a proxy for their propensity to engage in misconduct. Firms with higher corruption exposure are more motivated to reduce whistleblowing risk. We measure corruption using the Corruption Perceptions Index (CPI) for regions where the firm operates, sourced from Transparency International. Following Sanseverino (2023), we weight the CPI by the firm's sales across these regions.[29] Consequently, regions contributing a larger share of sales have a greater influence on the firm's overall corruption exposure. We define firms with values above the median as having high exposure to corruption. ($Corruption^{High}$ = 1), and firms with values below the median as having low corruption exposure ($Corruption^{High}$ = 0) otherwise. We then re-estimate Equation (1) by further including the interaction terms, $Post \times Corruption^{High}$, and $Treated \times Post \times Corruption^{High}$.[30] Column 1 of Table 4 shows that

[29] We measure a firm's sales across its operating regions for 2009, and similarly, we use the CPI index for the same year.

[30] $Treated \times Corruption^{High}$ is omitted from the regression because the values of *Treated* and $Corruption^{High}$ remain constant within a firm over time.

the coefficient of $Treated \times Post \times Corruption^{High}$ is significantly negative, suggesting that firms with higher corruption risk significantly reduce the size of their U.S. accounting departments to a larger extent after the Program's implementation than other firms.

Second, resource dependence theory suggests that firms heavily reliant on a specific region for income are more sensitive to and vulnerable to local regulatory actions(Efing et al. 2023). This dependence can heighten the scrutiny they face, as their operational success is closely tied to market-specific regulations, thereby increasing the pressure to comply and avoid penalties. The heightened regulatory enforcement creates a stronger incentive for firms to adopt measures and manage their exposure carefully (Call et al. 2016, Yost 2023). In our context, firms with a significant portion of their sales derived from the U.S. would particularly aim to mitigate the risk of attracting regulatory investigations due to their increased exposure to whistleblowing, more so than other firms. Empirically, we construct an indicator variable, US^{High}, which takes a value of one if a firm's pre-event U.S. sales, scaled by its global sales, are above the median, and zero otherwise. We then incorporate the interaction terms between *Treat*, *Post*, and US^{High} into Equation (1). The regression coefficient on $Treated \times Post \times US^{High}$ in Column 2 of Table 4 is significantly negative, suggesting that firms with more sales from the U.S. experience a greater reduction in the size of accounting departments in the U.S. compared to other treated firms.

Third, the regulation outside the U.S. inherently requires extensive international cooperation. The SEC frequently collaborates with foreign governments and regulatory authorities to collect evidence and enforce compliance (Silvers 2020, Chy et al. 2024). A network of Memoranda of Understanding (MoUs) with foreign securities regulators, have been instrumental in delineating the terms of information-sharing between and among MoU signatories, create a framework for regular and predictable cooperation in securities law enforcement, and hence overcoming jurisdictional hurdles. U.S. regulatory actions become more effective in gathering evidence after the signing of MoUs for enforcement cooperation with foreign countries (McLean 2011). These MoUs enable the exchange of critical information, enhancing U.S. agencies' ability to monitor and enforce laws like the FCPA. As a result, firms are less likely to downsize U.S. accounting departments to mitigate whistleblowing risks if MoUs exist with their home country or other regions they operate in. In the absence of MoUs, U.S. whistleblower information becomes crucial, making downsizing more effective. Therefore, we expect firms to reduce the size of their U.S. accounting departments when

they are not exposed to countries with MoUs with the U.S.

To test this prediction, we construct an indicator variable, MoU^{NA}, which takes the value of one if a firm's home country has not signed MoUs with the U.S. and the firm does not operate in countries that have MoUs with the U.S. in 2009, and zero otherwise. Again, we incorporate the interaction terms in relation to MoU^{NA} into Equation (1). The regression coefficient on $Treated \times Post \times MoU^{NA}$ in Column 3 of Table 4 is significantly negative, suggesting that firms without exposure to the countries that have MoUs with the U.S. experience a greater reduction in the size of accounting departments in the U.S. compared to other treated firms.

6.7 Employee Turnover by Seniority

Our baseline results indicate that treated firms downsize their U.S. accounting functions following the program's implementation. The change in the size of an accounting department can be attributed to two main factors: the inflow and outflow rates of accountants. Specifically, a decrease in department size may result from (1) reduced hiring, (2) increased departures, or (3) a disproportionately greater reduction in hiring compared to departures. In this section, we investigate the hiring and departure patterns, aiming to better understand the specific human capital decisions made by treated firms to reduce the number of their accountants in the U.S.

Empirically, we create two new variables to capture the accountant inflow and outflow rates: $USAcct^{Inflow}$ represents the number of accountants who newly join a firm's U.S. segments in a given year, while $USAcct^{Outflow}$ represents the number of accountants who depart from the firm's U.S. segments during that same year. We scale both variables by the respective global inflow and outflow. We then re-estimate Equation (1) by replacing our baseline dependent variable *US Acct* with the new variables. Table 5 reports the results. Consistent with our baseline results, we find the coefficient on *Treated* × *Post* in Column 1 is significantly negative (-0.021), suggesting that our treated firms hire fewer new accountants in the U.S. than control firms following the program. This effect is economically meaningful: -0.021 represents a 11.0% reduction relative to the mean value of $USAcct^{Inflow}$ (=-2.1%/19.9%, untabulated).

Interestingly, the coefficient on *Treated*×*Post* in Column 4 is significantly negative (-0.013) when the dependent variable is $USAcct^{Outflow}$. This result suggests that treated firms are more likely to retain incumbent accountants in their U.S. segments rather than to dismiss them, even when

aiming for a net downsizing of the U.S.-based accounting team.[31] This effect is also economically meaningful: -0.013 represents a 6.4% reduction relative to the mean value of $USAcct^{Outflow}$ (=-1.3%/20.3%, untabulated). However, compared to the magnitude of hiring reduction shown in Column 1, this effect is approximately 50% smaller (=6.4%/11.0%). Untabulated results show that the magnitude of the coefficient on $Treated \times Post$ is significantly larger for the $USAcct^{Inflow}$ specification than that in the $USAcct^{Outflow}$ specification. Therefore, our results suggest that, while the main driver of the reduction in accounting department sizes is the reduced hiring rate, this impact is partially counterbalanced by a higher retention rate of existing accountants, though this retention is less significant compared to the reduction in hiring.

In the further analysis, we partition our accountants joining or leaving a firm based on whether they are entry-level or non-entry-level (i.e., ranked higher than entry-level) accountants. Appendix D presents the distribution of accountants across different ranks in our sample, where entry-level accountants comprise more than 65% of the total. On the on hand, we find that the reduction in inflow is generally observed across both groups (Columns 2 and 3). This finding is consistent with the notion that reducing hiring is a strategy uniformly employed by treated firms across various ranks of accountants. Such a reduction in hiring decreases the size of the accounting department and, thus, lowers the whistleblowing risks posed by new hires. On the other hand, the reduction in outflow is only statistically significant among non-entry-level accountants (Column 6), but not for entry-level accountants (Column 5). Given that higher-ranked employees are more prone to managing execution (Aobdia et al. 2023) and engaging in wrongdoing (Bowen et al. 2010), this finding aligns with the notion that higher-ranked employees hold more private information and are therefore more sensitive to whistleblowing risks. Firms seek to manage the risk by retaining higher-ranked accountants (e.g., providing additional compensation to incumbent ones) rather than terminating them. However, the effect in Column 6 is not significantly greater than that for entry-level accountants in Column 5, possibly due to lower testing power.

Taken together, the reduction in the size of U.S. accounting departments is primarily due to

[31] Our results are consistent with the anecdote evidence. For example, in the foreign bribery case of Anheuser-Busch InBev (AB InBev), a firm from Belgium listed in the U.S. through ADR, it has agreed that within 60 days from the date the SEC enters the Order, it will make reasonable efforts to contact the *former* employees of AB InBev's U.S. entities previously identified by the SEC staff, and provide them with a copy of this Order and a statement that AB InBev does not prohibit *former* employees from contacting the SEC regarding possible violations of federal law or regulation. See more details at https://www.sec.gov/enforcement-litigation/whistleblower-program/notice-covered-actions/award-claim-2016-138.

decreased hiring across all levels of accountants, along with a noticeable decline in departures among higher-ranked accountants. This strategy could effectively minimize whistleblowing risk exposure by limiting the inflow of new accountants, who are less familiar to the firm yet soon to handle sensitive information, and by reducing the risk of potential retaliation from departing employees already privy to the firm's private information.

7 Further Analyses

7.1 Locations of New Accountants

In this section, we conduct an employee-level analysis on the change in new individual accountants' location around the enforcement of the Program to provide additional inferences. We first examine the potential of "home bias" in hiring accounting employees. Specifically, in reducing the accounting department size of U.S. segments, foreign firms could hire fewer new accountants in the U.S., and meanwhile hire more accountants in other locations, in particular, their home countries, if they continue to maintain a similar overall size of accounting team. The home country is generally considered to be a more familiar and information-controlled environment (Coval & Moskowitz 1999, Belderbos et al. 2013). Also, firms tend to have home bias in their business activities to reduce the risk of information dissipation abroad (Belderbos et al. 2013).

We conduct our analysis at the employee level using a Poisson Pseudo Maximum Likelihood (PPML) regression based on all accountants of a certain foreign firm in our sample. The dependent variable, *Enter*, indicates whether an accountant is newly hired or relocated within the same firm to a new location in a given year, and zero otherwise. The regression model incorporates the same fixed effects and firm characteristics as Equation (1). We further include employee characteristics such as gender, race, and seniority, sourced from Revelio Labs. We also construct three indicator variables, *Home*, *NonHome*, and *US*, to represent the countries in which new accountants begin their roles: the home country, a non-home country (outside U.S.), and the U.S., respectively. We include these variables, along with their interaction terms with *Treated* and *Post*, in the regression. Our key interest lies in the three interaction terms: $Treated \times Post \times Home$, $Treated \times Post \times NonHome$, and $Treated \times Post \times US$. They represent the likelihood of a new accountant being located in the home country of a treated firm, other countries outside the U.S., and the U.S., respectively,

after the program compared to control firms.[32] Figure 2a illustrates the regression results of the coefficients on the three key interaction terms. Consistent with our baseline results, we find evidence that treated firms hire significantly fewer new accountants in the U.S. We find some weak evidence suggesting that treated firms hire more accountants outside the U.S. Further, we do not find statistically significant differences among home countries and non-home countries.

Next, we examine the countries where accountants begin working, by considering the country-level of corruption exposure and whether a country has signed MoUs with the SEC, as in our cross-sectional tests. As discussed, firms located in the countries with high corruption levels are more likely to engage in unethical practices (Fisman & Miguel 2007). Conversely, countries with MoUs have established mechanisms for enforcement cooperation and information sharing with the SEC (Silvers 2020), thereby posing fewer challenges for regulatory oversight. Consistent with this, our cross-sectional results indicate that firms operating in countries with higher corruption risk and those without MoUs with the SEC reduce the size of their U.S. accounting departments to a greater extent following the Program's implementation compared to other firms.

To empirically test the effect of corruption exposure, we decompose *Home* and *NonHome* into four new indicator variables by incorporating the country-level corruption exposure. For example, in the case of *Home*, $Home^{HighCorruption}$ and $Home^{LowCorruption}$ indicate scenarios where a new accountant starts working in her firm's home country with high and low corruption exposure, respectively. We then repeat the previous employee-level regression after replacing *Home* and *NonHome* with the new four indicator variables. Figure 2b illustrates the regression results. We find that treated firms significantly more prefer hiring new accountants in their home countries with higher corruption exposure than other locations. We also repeat the analysis by considering whether a country has signed MoUs with the SEC. Figure 2c illustrates the results. We find that treated firms significantly more prefer hiring new accountants in their home countries without MoUs with the SEC than other locations. Collectively, our treated firms exhibit some home bias when adjusting their accounting departments, and their decisions are strongly affected by institutional factors related to whistleblowing risks and the effectiveness of relocation strategies in mitigating these risks (Porta et al. 1998, Chan et al. 2005).

[32] Some terms in Equation (1) are absorbed by including the interaction terms in relation to the baseline case (US). For example, $Treated \times Post$ is absorbed after including $Treated \times Post \times US$.

7.2 U.S. Accounting Job Postings

Our analysis of inflow and outflow data has demonstrated that the downsizing of the U.S. accounting team is due to reduced inflow—specifically, fewer hiring decisions—rather than an increase in the termination of existing accountants. One limitation of this approach is that a decrease in inflow could also stem from a lack of available accountants in the U.S. labor market. To further investigate this decrease in hiring activity and mitigate the effects from the demand side, we analyze changes in accounting-related job postings, which could directly reflect a firm's demand for accountants. Job postings serve as a proxy for the demand for specific occupations within a firm and provide good measures on immediate labor requirements (Aobdia et al. 2024, Gao et al. 2023, Forsythe et al. 2020). However, due to limitations in data coverage, our analysis can only be based on job posting information within the U.S. from the year 2010. As a result, we cannot capture the reallocation of demand for accountants in other countries (e.g., the potential expansion of accounting teams in non-U.S operations or an overall increase in accountant hiring), which may lead to an underestimation of the treatment effect. Also, the constraints in data availability may prevent us from constructing a sample that covers earlier years with a testing power.

We obtain the job posting data in the U.S. from Burning Glass Technologies. Since data before 2010 is not available, we rely on data during 2010 and 2011 to conduct the dynamic effect at firm-quarter level. The dependent variable is $USAcct^{JobPost}$, calculated as the number of job postings for a firm's accounting positions in the U.S.[33] We run the Poisson Pseudo Maximum Likelihood (PPML) regression and control for the same variables and fixed effects as baseline specification Equation (1). As a part of firm-quarter-level analysis, We further control for the potential seasonality of recruitment by including the year-quarter fixed effects.[34] As we discussed earlier, whistleblowers became eligible for bounties starting from July 22, 2010. Consequently, we use the quarter immediately before July as the benchmark. Figure 3 displays the coefficient estimates for the interaction between *Treated* and quarterly indicators. We observe that, following the announcement of the Act's implementation at the beginning of the third quarter of 2010, the number of U.S. accounting job postings among firms in the treated group significantly decreases in

[33] We apply the same O*NET classifications as used in screening Revelio Labs data for accounting roles.

[34] The entropy balancing weights are calculated using the values of the controls (*ROA*, *Foreign Biz*, and *Sales*) for the year 2009.

compared to the control group. This downward trend tends to largely persist over the subsequent quarters. The dynamic effect of U.S. accounting job postings provides support for the notion that managers from treated firms strategically reduce the demand for accounting employees in the U.S. in response to the Program's implementation.[35]

7.3 SEC Investigations

Our hypothesis is developed based on an assumption that managers of treated firms believe allocating fewer accountants in the U.S. can reduce the whistleblowing risk. Several empirical challenges hinder us from directly examining the validity of this assumption. For example, we could attempt to explore the issue by examining whether firms subject to the Program, those on average allocate fewer accountants in the U.S. after the Program, are less likely to be involved in SEC investigations then (e.g., Nan et al. 2024). However, an opportunistic manager's ex ante intention to distort internal information flow and the ex post observed realization of reduced whistleblowing risk could be disconnected. First, several factors, such as behavioral biases, information asymmetries, unexpected external shocks, and implementation challenges, could render it ineffective in reducing whistleblowing risk ex post, even if managers believe ex ante that it would be effective (Chetty 2015). In other words, we expect managers to adjust their accountant allocation in response to the *perceived* increase in whistleblowing risk and the *anticipated* reduction in such risk through this allocation. However, this incentive does not necessarily result in an actual reduction in whistleblowing risk. Second, empirically isolating the reduction in whistleblowing risk attributable to accountant allocation from the overall change in whistleblowing risk is challenging. Specifically, managers may endogenously adjust their unobservable misconduct or hidden opportunistic behaviors following accountant allocation. As a result, the observed whistleblowing risk could decrease, remain unchanged, or even increase, even if the component of whistleblowing risk directly influenced by accountant allocation decreases. Third, the intensity of SEC investigations may not solely be driven by managers' actions but also by the SEC's efforts and incentives following the Program's implementation. Therefore, an observed reduction in actual SEC investigations is not necessarily sufficient to validate our assumption. Nevertheless, to provide more confidence on the

[35] The relatively weaker statistical significance of our estimates may be attributed to limited testing power, stemming from constraints in the sample period and an inability to account for changes in the size of global accounting teams due to data limitations.

validity of our assumption, we rely on a list of ex-post and observable SEC investigations to explore whether managers achieve their intended goals by manipulating internal accounting capital flows or information flows.

That said, using this approach introduces additional empirical difficulties, particularly when attempting a DID regression similar to our baseline analysis. First, since our control firms are not subject to SEC oversight, they cannot face SEC investigations and, therefore, cannot serve as control groups in this context. Second, the binary and infrequent nature of our SEC investigation measure, particularly among the treated firms, does not allow us to capture nuanced variations in whistleblowing risk. This limitation restricts the generalizability of our findings and our ability to detect both cross-sectional and temporal variations in whistleblowing risk, should we maintain the same fixed effects structure as our baseline model. As a result, we focus on providing suggestive evidence through a univariate comparison of SEC investigation incidence in treated firms before and after the Program's implementation.

In Figure 4, we plot the frequency of SEC investigations for the treated firms during our sample period. The investigation data encompasses both the firms themselves and those of their U.S. subsidiaries. Prior to the Program's implementation, 2.05% of the firm-years were subject to the investigations. After the implementation, this decreased to 0.96%, representing a roughly 53% reduction (=(0.96%-2.05%)/2.05%) that is both economically and statistically significant (with a t-statistic of 78.98, untabulated). This result is consistent with the notion that treated firms are less likely to be investigated by the SEC after the implementation. we further investigate the change in SEC investigations across different contexts using the partition variables in cross-sectional tests (Table 4). This approach helps to establish a benchmark group similar to the DID method, aimed at mitigating the omitted variable bias. Figure 4 shows that, following the Program's implementation, there is a more marked decrease in investigations, particularly in firms that substantially reduce the size of their accounting departments in the U.S.: when firms have higher U.S. sales ($US^{High} = 1$) and when firms do not have exposure to countries with MoUs with the U.S. ($MoU^{NA} = 1$). These decreases represent approximately 68% (=-0.70/1.03) and 9% (=-0.18/1.98) relative to firms with lower U.S. sales ($US^{High} = 0$) and with exposure to countries that have MoUs with the U.S. ($MoU^{NA} = 0$) prior to the Program, respectively. Taken together, our evidence is consistent with firms subject to the Program and reducing more U.S. accountants

are less likely to be involved in SEC investigations after the Program.[36] It is important to note that these findings should be interpreted with caution. For instance, the observed reduction could align with a decrease in whistleblowing risk due to potential internal information manipulation by firms, or it could alternatively reflect the improvement in corporate governance of treated firms prompted by a deterrence effect (e.g., Berger & Lee 2022, Wiedman & Zhu 2023). We explore this question further in Table 8.

7.4 Alternative Explanation: SEC Specialization

On January 13, 2010, the SEC announced a reorganization of its Division of Enforcement (DOE), creating five Specialized Units (SUs), each of which focused on specific areas of securities law requiring heightened scrutiny.[37] One of these is the FCPA Unit, which concentrates on enforcing laws and regulations that prohibit corporate bribery of foreign officials. The most significant change in the reorganization involves transitioning some staff from generalist roles to specialist positions. Given our focus on foreign firms, which are likely involved in corruption with foreign officials, one may be concerned that the downsizing of U.S. accounting departments could be driven by specialized reorganizations within the SEC, which coincided with the enactment of the Program.

We conduct two sets of tests to address this issue. First, we control for the effect of FCPA-related specialized reorganizations within the SEC in the baseline model. Specifically, we create an indicator variable ($FCPA\ Specialization^{D}$) and interact it with *Post* in the Equation (1). $FCPA\ Specialization^{D}$ is equal to 1 if a firm has segments located in states where the SEC's FCPA investigative specialists operate, and 0 otherwise. We obtain the geographic distributions of the SEC's regulators specializing in FCPA enforcement from Herrmann et al. (2024).[38] Table 7 shows the regression results. In Column 1, we find our baseline results remain unchanged after controlling for $FCPA\ Specialization^{D} \times Post$. To further highlight the intensity of FCPA specialization varying across firms, we replace $FCPA\ Specialization^{D}$ with $FCPA\ Specialization^{\#}$, calculated as the number of a firm's segments located in FCPA specialized states. Column 2 shows that the coefficient on the interaction term between $FCPA\ Specialization^{\#}$ and *Post* is statistically insignificant from zero

[36] We acknowledge that the reduction in SEC investigations is nearly negligible when comparing firms with low corruption exposure ($Corruption^{High} = 0$) to those with high corruption exposure ($Corruption^{High} = 1$).

[37] For more details, please see https://www.sec.gov/news/speech/2010/spch011310rsk.htm.

[38] According to Herrmann et al. (2024), the states that have FCPA specialists including Utah, Washington, Oregon, California, Nevada, Idaho, Alaska, Hawaii, Montana, Texas, Louisiana, Mississippi, Arizona, Florida, Oklahoma, Vermont, Maine, Kansas, Arkansas, New Hampshire, Massachusetts, Rhode Island and Connecticut.

and our baseline effect continues to hold.

Second, we check the robustness of our baseline findings by excluding firms with segments in states with FCPA specialists from the SEC. In Column 3, we exclude 147 firms with segments in Utah, the state with the highest percentage of FCPA investigative staff from the SEC. While in Column 4, we exclude 1,183 firms, the number of whose segments in the sates with SEC FCPA specialists is greater than the median value based on Herrmann et al. (2024). The results in both columns show that the coefficient on *Treated* × *Post* is still significantly negative. Therefore, our results in Columns 1 to 4 of Table 7 suggest that the alternative explanation of the SEC's specialization in FCPA enforcement do not drive our inference.[39]

7.5 Alternative Explanation: Jumpstart Our Business Startups (JOBS) Act

The U.S. Jumpstart Our Business Startups (JOBS) Act of 2012 was introduced with the aim of stimulating job growth and easing regulatory burdens for smaller companies, particularly those going public. One of the key provisions of the Act was to reduce financial reporting and disclosure requirements for emerging growth companies (EGCs), which are defined as companies with less than $ 1 billion in total annual gross revenues in their most recent fiscal year.[40] These EGCs were granted relief from certain regulatory and disclosure obligations, which could trigger the small foreign issuers to reduce their demand to have a certain size of accountants in the U.S. as before.

To mitigate the potential impact of the JOBS Act on our findings, we conduct a robustness check by excluding firms that qualify as EGCs from our testing sample. Specifically, we first identify 303 treated firms (or in total 3,096 firm-years in 2012 and onwards) that ever had gross annual revenue of less than 1 billion starting from the year prior to the enactment of the Act.[41] Column 5 of Table 7 shows that, after excluding the treated firms that met the criteria before and after the enactment of the JOBS Act, our baseline results remain robust. In Column 6, we further exclude 977 control firms with 9,659 firm-years that qualify as EGCs based on the revenue test. Our results do not

[39] We also consider other specialized units, including the Asset Management Unit, Market Abuse Unit, Structured and New Products Unit, and Municipal Securities and Public Pension Unit, respectively. Our results hold when we control for the indicators or degree of these specializations, or drop the firms with more segments in the states with a certain type of SEC specialists (untabulated).

[40] For more details of EGCs, please see https://www.sec.gov/rules-regulations/staff-guidance/compliance-disclosure-interpretations/jumpstart-our-business-startups-act-frequently-asked-questions.

[41] Under the JOBS Act, a company that meets the criteria for an EGC can submit an application in any year after the enactment of the Act, as long as its total annual gross revenues in the most recent fiscal year are below $ 1 billion and it meets other relevant requirements.

change. Note that the results in Columns 5 and 6 of Table 7 also indicate that the implementation of the JOBS Act do not significantly increase additional reallocation of accountants.

7.6 Reporting Quality

Cross-listing in the SEC subjects a foreign firm to various provisions of U.S. securities laws, including compliance with U.S. Generally Accepted Accounting Principles (GAAP). This regulatory environment is seen as more stringent and transparent, and it has been documented that cross-listing in the U.S. can act as a bonding mechanism to U.S. regulations, signaling a more transparent information environment to investors and stakeholders (Reese Jr & Weisbach 2002). Given this regulatory context, we examine whether the Program, which leads to a reduction in the size of a firm's accounting department in the U.S. affects the overall quality of the firm's financial reporting.

To assess the impact on financial reporting, we first employ the modified Jones model as proposed by Dechow et al. (1995), which is widely used to estimate the discretionary accruals to capture the level of accrual earnings management within firms. This model allows us to create a proxy for the transparency and reliability of financial reporting. Columns 1 in Table 8 capture the overall effect of changes in discretionary accruals around the Program's implementation for treated and control firms. Second, Berger & Lee (2022) and Wiedman & Zhu (2023) use the F-score proposed by Dechow et al. (2011) to examine the possibility of financial fraud after the enforcement of Program for the U.S. firms. We also investigate this effect in the context of foreign firms with reduced U.S. accounting employees. Column 2 shows the overall effect of changes in F-score. Our findings indicate that treated firms experience a noticeable decline in the quality of their financial information, which consequently increases the likelihood of earnings management, relative to control firms after the Program comes into effect.

To enhance the credibility of our findings, we further analyze the impact of the significance of U.S. business operations on a firm's financial reporting quality in Columns 3 and 4. Given that a firm's U.S. operations are only a part of its entire business, we expect that the negative impact of the Program would be more pronounced when the firm's U.S. operations constitute a larger proportion of its total operations, if the increases in discretionary accruals and F-score are indeed driven by the program's implementation. Consistent with our prediction, we find that the decline in financial reporting quality is more pronounced for those with significant business presence in the

U.S. Our findings suggest that a firm's reduction in its U.S. accounting department is associated with a deterioration in the quality of a firm's financial information, with this adverse effect being particularly pronounced in firms for which U.S. operations represents a substantial component of the overall business activities.

7.7 Production Efficiency

Banerjee et al. (2024) propose that firms experience a decline in real efficiency due to distorted internal transparency stemming from the increased regulatory threats associated with whistleblower programs. To further investigate this possibility, we assess production efficiency using total factor productivity (TFP) as outlined by Schoar (2002). Since the input of capital, labors and material can vary by country, industry, and year. We calculate TFP for each country-year (*TFP1*) and country-industry-year (*TFP2*). *TFP1* captures a firm's deviation from the factor productivity within its home country in a given year. *TFP2* captures a firm's deviation from the factor productivity within its home country and industry in a given year. In columns 1 and 2 of Table 9, we present the overall effects on TFP, while columns 3 and 4 analyze the incremental impact of reliance on the U.S. market. Our results do not provide strong evidence that firms experience a significant decrease in total factor productivity. Furthermore, firms with greater U.S. business reliance do not exhibit a significant trend in declining production efficiency either. These findings suggest that the distortion in information flow does not readily translate into a lower productivity.

8 Conclusion

This study examines how foreign firms in the U.S. stratigically allocate accountants to minimize their exposure to whistleblowing risks following the implementation of the 2011 SEC Whistleblower Program. Departing from prior studies (e.g., Berger & Lee 2022, Wiedman & Zhu 2023) focusing on the disciplinary effects of the program on U.S.-headquartered firms, we shift the attention to regulatory arbitrage of foreign firms subjected to the program, with an emphasis on their manipulations of allocating the U.S.-based accountants.

Relying on a sample of foreign firms registered with the SEC, our DID estimation indicates that they downsize their U.S.-based accounting teams, following the Program's implementation. They achieve this by reducing new U.S. hiring while retaining more senior accountants. At the same time,

treated firms expand teams in their home countries, especially in regions with higher corruption risks and without MoUs with the SEC. These actions taken by treated firms are consistent with the hypothesis that self-interested executives deliberately distort internal information flow and communication to avoid regulatory scrutiny.

We further demonstrate that the regulatory arbitrage behaviors by these foreign firms, in the form of manipulating internal information flow, also manifests in a decline in reporting quality. We show an increase in reporting opacity proxied by abnormal accruals and F-Score, suggesting that the possibility of opportunistic behaviors by foreign firms undermines the effectiveness of the Program. Our findings on the regulatory arbitrage of foreign firms, combined with prior empirical studies on disciplinary effects on U.S.-headquartered firms, provide a comprehensive view of the two potential equilibrial resulting from enhanced whistleblowing incentives: one leading to desirable improvements in regulatory efficiency and the other potentially causing unintended reductions in transparency due to opportunistic managerial manipulation (e.g., Banerjee et al. 2024, Nan & Zheng 2024).

Our findings may provide implications for regulators regarding multinational firms' opportunistic responses to SEC's investigations: We find that they allocate accountants by exploiting regulatory barriers between segments to minimize regulatory scrutiny. A lack of international regulatory cooperation can aggravate this problem. Our study hence contributes to understanding how U.S. regulations affect foreign entities and the international business landscape, which is increasingly relevant in today's globalized economy. This helps in understanding the reach and impact of U.S. laws on global commerce and compliance.

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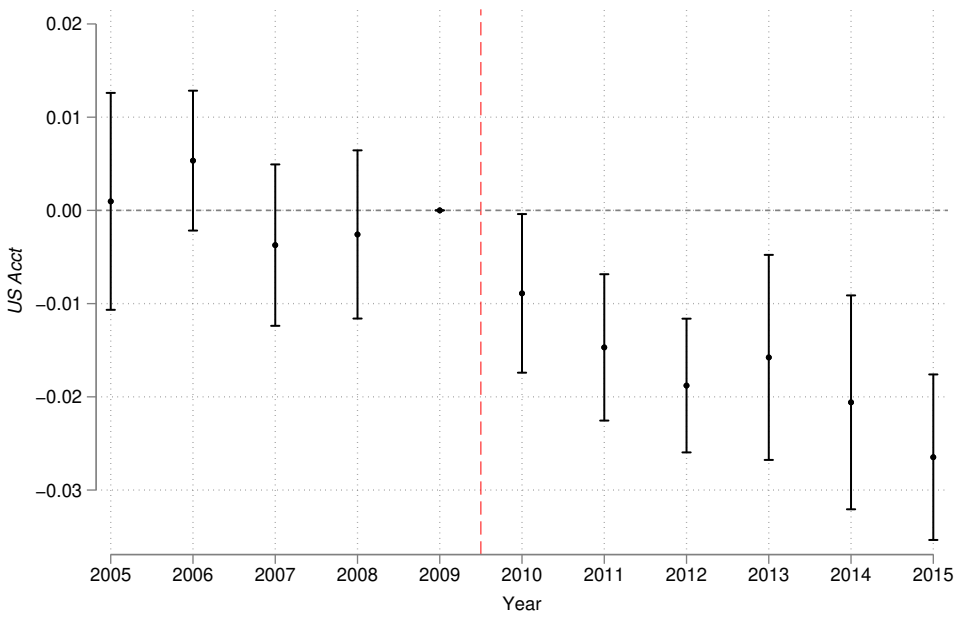
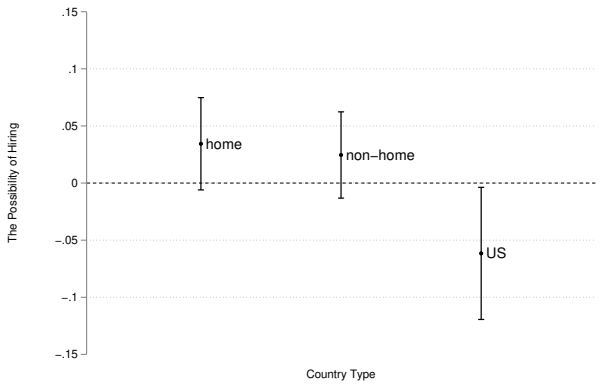
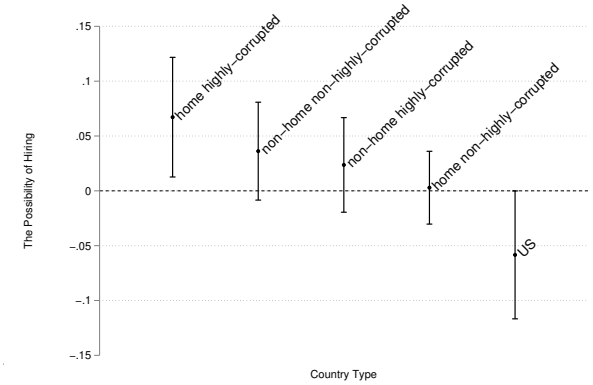


Figure 1. Dynamic Effect

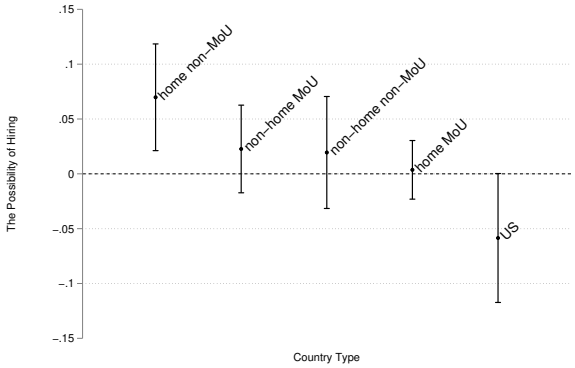
This figure maps out the trend of the counterfactual treatment effects on *US Acct* during the sample period of 2005-2015. Using our main specification in Table 3, Column 4, we replace the *Post* indicator with separate indicators for each year, except for 2009 (base year), which is the year immediately preceding passage of the SEC Whistleblower Program. This figure displays coefficient estimates on the interaction between *Treated* and the indicators for each year and their 90% confidence intervals based on standard errors clustered by home country.



(a)



(b)



(c)

Figure 2. Locations of New Accounting Employees

This figure displays the locations of accountants newly joining firms in our sample. We estimate an employee-year level analysis based on Equation (1) with the dependent variable of *Enter*. We further include the interaction between *Treated*, *Post*, and the indicators for different country types. All variables are defined in Appendix A. In Figure (a), we show the hiring preference in home countries of firms, non-home countries (outside the U.S.), and the U.S. In Figure (b), we further divide the home countries and non-home countries (outside the U.S.) based on whether they are highly-corrupted country. In Figure (c), we categorize the home countries and non-home countries (outside the U.S.) based on whether they have signed MoUs with the SEC. This figure displays coefficient estimates on the interaction between *Treated*, *Post*, and the location indicators as well as their 90% confidence intervals based on standard errors clustered by home country.

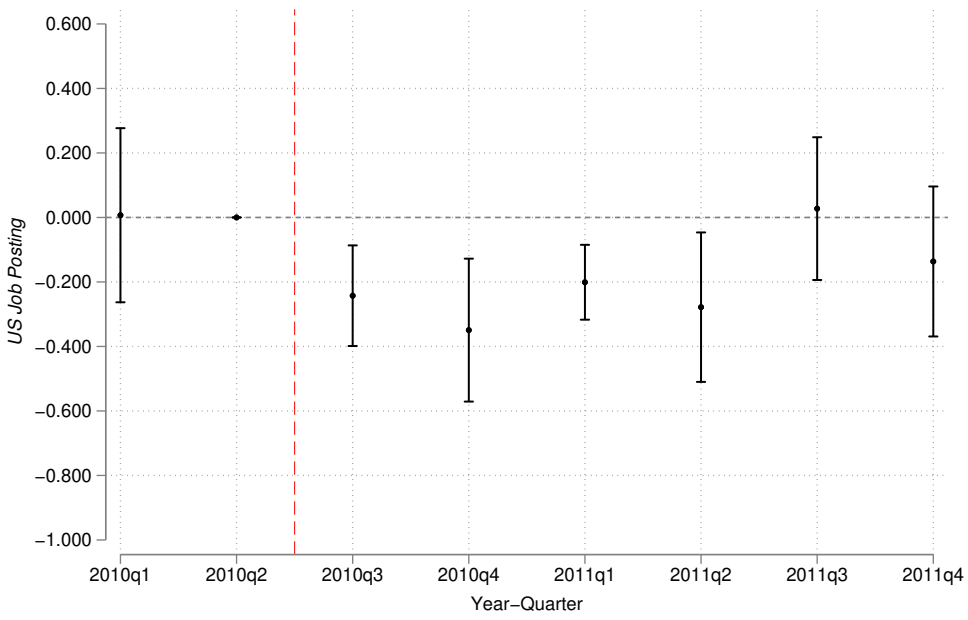


Figure 3. Dynamic Effect of U.S. Accounting Job Postings

This figure illustrates the dynamic effect on job postings of accountants in the U.S. ($USAcct^{JobPost}$) for the year 2010. The analysis is based on firm-quarter level job posting records using a PPML regression, whereas we consider the same variables and fixed effects as baseline specification Equation (1), and further control for year-quarter fixed effects. The entropy balancing weights are calculated using the values of the control variables for the year 2009. The whistleblowers became eligible for bounties starting from July 22, 2010, we use the quarter immediately before July (i.e., 2010q2) as the benchmark. The figure plots the coefficient estimates for the interaction between *Treated* and quarterly indicators, along with their 90% confidence intervals, with standard errors clustered by home country.

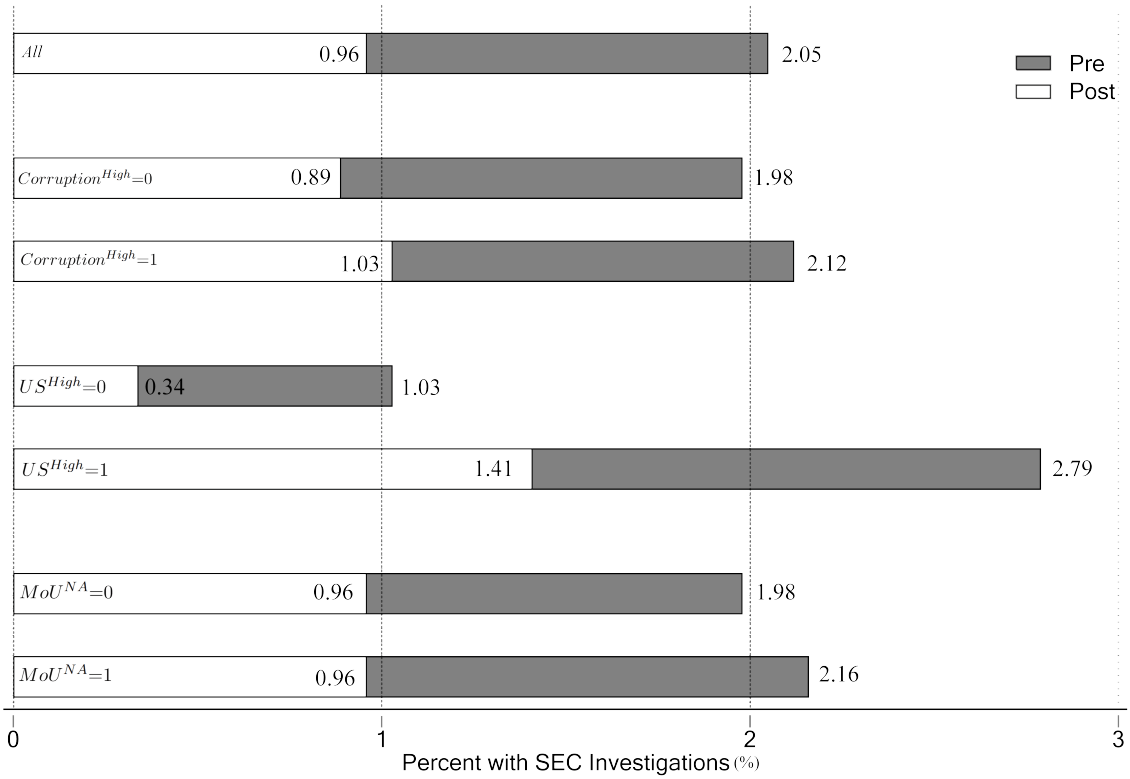


Figure 4. SEC Investigations

This figure plots the observed proportions of SEC investigations among the treated firms. During the sample period, there were a total of 145 SEC investigations, with 91 occurring prior to the Program's implementation and 54 following its implementation. "All" represents the SEC investigation towards the treated firms, using the percentage of firm-years investigated by the SEC. We also consider the percentage of SEC investigations across different contexts using variables in cross-sectional tests (Table 4). Gray in the legend denotes the pre-Program period, while white represents the post-Program period.

Table 1. Sample Selection and Distribution

This table illustrates the sample selection process and the distributions across years, industries, and home countries. The final sample includes 25,845 firm-years between 2005 and 2015. The full sample is defined as the one only requiring non-missing values of control variables but not for employee data.

Panel A: Sample selection procedure

Sample period: 2005-2015	Removed	# Firms	# Firm-years
Foreign firms in the U.S. included in BvD Osiris with employee records		6,842	72,294
Drop firms listed in the U.S. after 2010	676	6,166	65,028
Retain firms with U.S. employee records both before and after 2010	2,241	3,925	42,376
Drop firms with missing data of control variables	872	3,053	27,526
Drop observations for countries with only one firm	439	2,615	25,845
Treated		965	10,059
Control		1,650	15,786

Panel B: Yearly distribution

Year	# Treated Firms	# Control Firms
2005	809	1,176
2006	863	1,270
2007	899	1,368
2008	924	1,467
2009	935	1,519
2010	942	1,502
2011	943	1,495
2012	936	1,495
2013	930	1,503
2014	939	1,487
2015	939	1,504
Total	10,059	15,786

Panel C: Industry distribution

Sector	Full sample		Final sample		# Treated firms		# Control firms		# Treated firm-years		# Control firm-years	
0100-0999	48	0.82%	18	0.69%	4	0.41%	14	0.85%	44	0.44%	135	0.86%
1000-1499	426	7.27%	117	4.47%	80	8.29%	37	2.24%	815	8.10%	333	2.11%
1500-1799	107	1.83%	56	2.14%	27	2.80%	29	1.76%	290	2.88%	289	1.83%
2000-3999	3218	54.90%	1562	59.73%	535	55.44%	1027	62.24%	5597	55.64%	9852	62.41%
4000-4999	475	8.10%	254	9.71%	134	13.89%	120	7.27%	1402	13.94%	1164	7.37%
5000-5199	242	4.13%	110	4.21%	27	2.80%	83	5.03%	284	2.82%	737	4.67%
5200-5999	154	2.63%	50	1.91%	18	1.87%	32	1.94%	178	1.77%	303	1.92%
6000-6799	268	4.57%	111	4.24%	34	3.52%	77	4.67%	352	3.50%	734	4.65%
7000-8999	924	15.76%	337	12.89%	106	10.98%	231	14.00%	1097	10.91%	2239	14.18%
Total	5862	100.00%	2615	100.00%	965	100.00%	1650	100.00%	10059	100.00%	15786	100.00%

Panel D: Home country distribution

Home country	Full sample		Final sample		# Treated firms		# Control firms		# Treated firm-years		# Control firm-years	
Australia	298	5.08%	110	4.21%	44	4.56%	66	4.00%	441	4.38%	588	3.72%
Belgium	45	0.77%	28	1.07%	12	1.24%	16	0.97%	119	1.18%	172	1.09%
Bermuda	101	1.72%	54	2.07%	18	1.87%	36	2.18%	174	1.73%	342	2.17%
Brazil	48	0.82%	29	1.11%	13	1.35%	16	0.97%	139	1.38%	157	0.99%
Canada	557	9.50%	175	6.69%	103	10.67%	72	4.36%	1058	10.52%	704	4.46%
Cayman Islands	133	2.27%	44	1.68%	18	1.87%	26	1.58%	179	1.78%	237	1.50%
China	358	6.11%	87	3.33%	16	1.66%	71	4.30%	153	1.52%	569	3.60%
Denmark	54	0.92%	31	1.19%	17	1.76%	14	0.85%	184	1.83%	141	0.89%
Finland	52	0.89%	40	1.53%	15	1.55%	25	1.52%	162	1.61%	271	1.72%
France	234	3.99%	137	5.24%	62	6.42%	75	4.55%	669	6.65%	750	4.75%
Germany	183	3.12%	112	4.28%	50	5.18%	62	3.76%	534	5.31%	599	3.79%
India	399	6.81%	198	7.57%	34	3.52%	164	9.94%	360	3.58%	1662	10.53%
Ireland	29	0.49%	21	0.80%	20	2.07%	1	0.06%	203	2.02%	11	0.07%
Israel	115	1.96%	41	1.57%	24	2.49%	17	1.03%	243	2.42%	161	1.02%
Italy	79	1.35%	40	1.53%	13	1.35%	27	1.64%	136	1.35%	238	1.51%
Japan	1122	19.14%	563	21.53%	186	19.27%	377	22.85%	1966	19.54%	3502	22.18%
Malaysia	47	0.80%	22	0.84%	2	0.21%	20	1.21%	22	0.22%	192	1.22%
Netherlands	53	0.90%	31	1.19%	18	1.87%	13	0.79%	198	1.97%	133	0.84%
Singapore	67	1.14%	38	1.45%	17	1.76%	21	1.27%	179	1.78%	188	1.19%
South Korea	194	3.31%	78	2.98%	14	1.45%	64	3.88%	149	1.48%	639	4.05%
Spain	51	0.87%	34	1.30%	16	1.66%	18	1.09%	174	1.73%	184	1.17%
Sweden	154	2.63%	65	2.49%	28	2.90%	37	2.24%	296	2.94%	374	2.37%
Switzerland	90	1.54%	62	2.37%	26	2.69%	36	2.18%	281	2.79%	355	2.25%
Taiwan	460	7.85%	183	7.00%	23	2.38%	160	9.70%	237	2.36%	1482	9.39%
United Kingdom	364	6.21%	167	6.39%	85	8.81%	82	4.97%	855	8.50%	816	5.17%
Others	575	9.81%	225	8.60%	91	9.43%	134	8.12%	948	9.42%	1319	8.36%
Total	5862	100.00%	2615	100.00%	965	100.00%	1650	100.00%	10059	100.00%	15786	100.00%

Table 2. Summary Statistics

This table presents the pre- and post entropy balancing sample statistics. Panel A presents the results of univariate difference-in-differences for *US Acct*. Panels B and C present co-variates balance before and after entropy balancing procedure, respectively. All variables are defined in Appendix A.

Panel A: Pre-entropy balancing summary statistics

Variable (N=25,845)	Mean	Std Dev	P25	P50	P75
US Acct	0.228	0.281	0	0.120	0.364
Treat	0.389	0.488	0	0	1
Post	0.565	0.496	0	1	1
ROA	0.061	0.077	0.022	0.058	0.104
Foreign Biz	22.98	35.30	3	8	23
Sales	3.417	6.025	0.163	0.682	3.213

Panel B: Pre-entropy balancing univariate analysis of physical information exposure

US Acct	Treated	Control	Diff (=T-C)	*t*-statistics
Pre (2005-2009)	0.263	0.223	0.040	(6.99)
Post (2010-2015)	0.236	0.210	0.026	(5.85)
Diff (=Post-Pre)	-0.027	-0.013	-0.014	(2.46)
t-statistics	(5.53)	(2.81)		

Panel C: Pre-entropy balancing comparison

	Treated (N=10,059)			Control (N=15,786)			Diff in (=T-C)	
Variable	Mean	Std Dev	P50	Mean	Std Dev	P50	Mean	*t*-stat.
US Acct	0.248	0.241	0.189	0.216	0.303	0.049	0.03	9.11
ROA	0.065	0.077	0.061	0.058	0.077	0.056	0.01	7.26
Foreign Biz	42.46	47.19	20	10.57	15.00	6	31.90	78.91
Sales	6.654	7.831	2.927	1.354	3.069	0.352	5.30	76.34

Panel D: Post-entropy balancing covariate balance

	Mean		Variance		Skewness	
Variable	Treated	Control	Treated	Control	Treated	Control
ROA	0.077	0.077	0.006	0.006	0.050	0.051
Foreign Biz	41.08	41.01	2299.80	2297.40	1.15	1.15
Sales	5.727	5.718	46.354	46.324	1.083	1.086

Table 3. Baseline Results

This table reports the results of estimating Equation(1), which models the effect of the Program on *US Acct*. All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	Dependent Variable: *US Acct*			
	(1)	(2)	(3)	(4)
Treated	0.069***			
	(3.74)			
Post	-0.005			
	(-0.54)			
Treated*×*Post	**-0.022****	**-0.011***	**-0.015*****	**-0.016*****
	(-2.59)	**(-1.86)**	**(-2.77)**	**(-2.85)**
ROA				-0.031
				(-1.31)
Foreign Biz				0.000
				(0.24)
Sales				0.000
				(0.36)
Firm FE	No	Yes	Yes	Yes
Home Country×Year FE	No	Yes	Yes	Yes
Industry×Year FE	No	No	Yes	Yes
N	25,845	25,845	25,845	25,845
Adjusted R^2	0.014	0.833	0.834	0.834

Table 4. Cross-Sectional Tests

This table reports the results of cross-sectional analyses based on Equation (1) with the inclusion of *Treated*×*Post*×*ParVar* and *Post*×*ParVar*. *ParVar* represents four partition variables: the level of corruption exposure ($Corruption^{High}$), the importance of the U.S. segment to the firm (US^{High}), the existence of international collaboration between the U.S. and either a firm's home country or its other operating countries (MoU^{NA}). All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and *t*-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	Dependent Variable: *US Acct*		
	(1)	(2)	(3)
Partition:	Corruption exposure	US importance	International collaboration
ParVar =	$Corruption^{High}$	US^{High}	MoU^{NA}
Treated* ×*Post*×*ParVar	**-0.033*** **	**-0.034** **	**-0.023** **
	(-2.96)	**(-2.06)**	**(-2.02)**
Treated×*Post*	0.005	-0.010*	-0.005
	(0.50)	(-1.90)	(-0.56)
Post×*ParVar*	0.013	0.022	0.023*
	(0.96)	(1.50)	(1.93)
ROA	-0.031	-0.035	-0.029
	(-1.30)	(-1.42)	(-1.27)
Foreign Biz	0.000	0.000	0.000
	(0.20)	(0.01)	(0.52)
Sales	0.000	0.001	0.001
	(0.41)	(0.67)	(0.89)
Firm FE	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	YES
Industry×Year FE	Yes	Yes	YES
N	25,845	25,845	25,845
Adjusted R^2	0.834	0.837	0.831

Table 5. Accountant Inflow and Outflow: Entry vs Non-Entry Level Positions

This table reports the results of accountant turnover tests. The dependent variables are either accountant inflow ($USAcct^{Inflow}$) or accountant outflow ($USAcct^{Outflow}$). The regression results conditioned on the seniority of accountants, based on Equation (1). Appendix D presents the distribution of accountants by seniority among our sample firms. All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Dependent Variable:	$US\ Acct^{Inflow}$			$US\ Acct^{Outflow}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	All	Entry Level	Non-Entry Level	All	Entry Level	Non-Entry Level
Treated*×*Post	**-0.021****	**-0.017***	**-0.018***	**-0.013***	**-0.010**	**-0.011****
	(-2.43)	**(-1.83)**	**(-1.84)**	**(-2.00)**	**(-1.14)**	**(-2.27)**
ROA	-0.024	0.013	0.007	-0.006	0.043	-0.095**
	(-0.64)	(0.32)	(0.23)	(-0.16)	(0.97)	(-2.52)
Foreign Biz	0.000	0.000	-0.001	0.000	0.000	0.000
	(0.37)	(0.77)	(-1.38)	(0.28)	(0.60)	(0.31)
Sales	0.001	0.000	0.001	-0.001	-0.000	-0.001*
	(0.74)	(0.12)	(0.82)	(-0.51)	(-0.26)	(-1.80)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,845	25,845	25,845	25,845	25,845	25,845
Adjusted R^2	0.350	0.349	0.270	0.359	0.341	0.240

Table 6. Is It A General Downsizing of U.S. Operations?

This table presents the results of the effect of the Program on the other job functions, the size of business operations, and M&A decisions. Panel A reports the results of actual and randomly selected non-accounting employees. Panel B reports the results of sales from the U.S. and other geographic segments. Panel C reports the results of merger and acquisition. All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include the control variables, intercepts, and fixed effects based on Equation (1) but do not report them for brevity. Standard errors are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	Dep. Var	Coeff. on *Treated*×*Post*	t-statistic	N	Adjusted R^2
Panel A: Actual and randomly selected non-accounting employees					
(1)	*US Others*	-0.004	(-1.06)	25,845	0.963
(2)	$US\ Others^{Random}$	-0.005	(-0.90)	25,845	0.900
Panel B: Sales in the U.S. and other geographic segments					
(1)	*US Sales*	-0.003	(-0.55)	25,845	0.823
(2)	*Ln($US Sales)*	0.037	(0.29)	25,845	0.837
(3)	*Ln($Non-US Sales)*	-0.009	(-0.31)	25,845	0.963
Panel C: Merger and acquisition decisions					
(1)	*US M&A*	-0.001	(-0.06)	25,845	0.287
(2)	*Non-US M&A*	-0.039	(-1.35)	25,845	0.492
(3)	*High Risk M&A*	-0.004	(-0.34)	25,845	0.318
(4)	*Low Risk M&A*	0.010	(0.65)	25,845	0.424

Table 7. Alternative Explanations

This table presents the results after considering some alternative explanations based on Equation (1). The dependent variables is *US Acct*. Columns (1)-(4) present the results after excluding the influence of the SEC's specialized enforcement units responsible for the Foreign Corrupt Practices Act. Columns 1 and 2 control for the impact of FCPA specialists using an interaction term between an indicator variable (*FCPA Specialization*^{D}) or a degree measure (*FCPA Specialization*$^{\#}$) and *Post*. Column 3 excludes firms with segments in Utah, the state with the highest percentage of FCPA investigative staff in the SEC. Column 4 excludes firms with the number of segments in the states with SEC FCPA specialists greater than the median value. Columns 5 and 6 present the results after excluding the influence of the Jumpstart Our Business Startups (JOBS) Act. Column 5 excludes the treated firms with less than 1 billion total annual gross revenues in any year after the Act was enacted, while Column 6 excludes both the treated and control firms with less than 1 billion total annual gross revenues in any year after the Act was enacted. All variables are defined in Appendix A. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	Dependent Variable: *US Acct*					
	(1)	(2)	(3)	(4)	(5)	(6)
Alternative Explanations:	SEC Specialization				2012 JOBS Act	
Specifications and Samples:	Dummy	Degree	Exclude Firms with UT Segments	Exclude Firms with More Exposure	Exclude Treated EGCs	Exclude All EGCs
Treated*×*Post	**-0.015*** **	**-0.015*** **	**-0.014*** **	**-0.023** **	**-0.015** **	**-0.015*** **
	(-2.73)	**(-2.67)**	**(-2.81)**	**(-2.17)**	**(-2.42)**	**(-2.79)**
FCPA Specialization^{D}×*Post*	0.008					
	(1.05)					
FCPA Specialization$^{\#}$×*Post*		-0.000				
		(-1.31)				
ROA	-0.031	-0.031	-0.027	-0.033	-0.014	-0.047**
	(-1.31)	(-1.30)	(-1.14)	(-0.89)	(-0.60)	(-2.26)
Foreign Biz	0.000	-0.000	-0.000	-0.000	0.000	0.000
	(0.29)	(-0.00)	(-0.10)	(-0.47)	(0.07)	(0.54)
Sales	0.000	0.000	0.000	0.001	0.000	0.000
	(0.40)	(0.43)	(0.44)	(0.69)	(0.39)	(0.05)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	25,845	25,845	24,311	13,638	22,749	13,090
Adjusted R^2	0.834	0.834	0.820	0.800	0.851	0.878

Table 8. Financial Reporting Quality

This table presents the effect on financial reporting quality. Columns 1 and 2 reports the baseline results for the dependent variable of the firm's discretionary accruals ($DACC$) and the predicted possibility ofearnings misstatement ($F\text{-}Score$). Columns 3 and 4 further include the interaction term between US^{High} and $Treated \times Post$. All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)
Dependent Variables:	*DACC*	*F-Score*	*DACC*	*F-Score*
Treated*×*Post	**0.004*** **	**0.032** **	**-0.001**	**-0.027**
	(2.79)	**(2.01)**	**(-0.39)**	**(-1.25)**
$Post \times US^{High}$			-0.007***	-0.081***
			(-2.69)	(-2.92)
$\boldsymbol{Treated \times Post \times US^{High}}$			**0.008*** **	**0.129*** **
			(2.76)	**(4.44)**
ROA	0.027***	1.259***	0.028***	1.262***
	(4.47)	(11.23)	(4.59)	(11.38)
Foreign Biz	-0.000	0.002	-0.000	0.002
	(-0.58)	(0.86)	(-0.64)	(0.87)
Sales	-0.000	-0.022***	-0.000	-0.022***
	(-1.62)	(-6.00)	(-1.55)	(-5.99)
Firm FE	Yes	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
N	25,448	25,144	25,448	25,144
Adjusted R^2	0.444	0.409	0.444	0.409

Table 9. Production Efficiency

This table presents the effect on firm production efficiency base on Equation (1) with the dependent variable, the total factor productivity examined in country-year level (*TFP1*) and country-industry-year level (*TFP2*). Columns 3 and 4 further include the interaction term between US^{High} and *Treated*×*Post*. All variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and *t*-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)	(4)
Dependent Variables:	*TFP1*	*TFP2*	*TFP1*	*TFP2*
Treated*×*Post	**-0.029**	**0.003**	**0.030**	**0.021**
	(-0.43)	**(0.11)**	**(0.53)**	**(0.65)**
Post×US^{High}			0.065	0.034
			(1.20)	(0.89)
Treated*×*Post×US^{High}			**-0.128**	**-0.039**
			(-1.52)	**(-0.83)**
ROA	0.689***	0.295***	0.686***	0.294***
	(6.20)	(3.23)	(6.15)	(3.25)
Foreign Biz	-0.002	-0.001	-0.002	-0.001
	(-0.80)	(-0.75)	(-0.79)	(-0.74)
Sales	-0.010	-0.010**	-0.010	-0.010**
	(-1.57)	(-2.50)	(-1.58)	(-2.51)
Firm FE	Yes	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
N	25,845	21,652	25,845	21,652
Adjusted R^2	0.571	0.277	0.572	0.277

Appendix A Variable Definitions

Variable	Definition
Variables used for the baseline analysis	
US Acct	The proportion of a firm's accounting employees in the U.S. relative to its total global accounting employees. [Revelio Labs]
Treated	An indicator set to 1 for a firm with stocks, bonds, or ADRs registered with the SEC, and 0 otherwise. [SEC filings]
Post	An indicator variable set to 1 from the year 2010 onwards, and 0 otherwise.
ROA	A firm's return on assets (ROA), calculated as the ratio of profit before tax to total assets. [BvD Osiris]
Foreign Biz	A firm's total number of subsidiaries and branches outside its home country. [BvD Osiris]
Sales	A firm's total operating revenue from global operations, measured in billions of US dollars. [BvD Osiris]
Variables used for the cross-sectional analysis	
$Corruption^{High}$	An indicator variable set to 1 if a firm's corruption exposure (CE) in 2009 is greater than the annual sample median, and 0 otherwise. Corruption exposure (CE) is calculated as the weighted average of the Corruption Perceptions Index across the firm's business regions, weighted by each region's sales in 2009. [BvD Osiris, Transparency International]
US^{High}	An indicator variable set to 1 if a firm's U.S. sales in 2009, as a proportion of its total global sales, are greater than the annual sample median, and 0 otherwise. [BvD Osiris]
MoU^{NA}	An indicator variable set to 1 if a firm's home country does not have Memoranda of Understanding (MoUs) on enforcement cooperation with the U.S., and the firm did not operate in any country with MoUs with the U.S. before 2010, and 0 otherwise. [SEC website, BvD Osiris]
Variables used for the other tests	
$US\ Acct^{Inflow}$	The number of accounting employees newly hired by a firm in the U.S or relocated to the U.S. within the same firm, scaled by the number of the firm's total global inflow of accounting employees. [Revelio Labs]
$US\ Acct^{Outflow}$	The number of accounting employees departed from a firm in the U.S. or moved to other countries within the same firm, scaled by the number of the firm's total global outflow of accounting employees. [Revelio Labs]

(Continued)

Variable	Definition
US Others	The proportion of a firm's non-accounting employees in the U.S. to its total global non-accounting employees. [Revelio Labs]
$US\ Others^{Random}$	The proportion of a firm's randomly selected non-accounting employees in the U.S. to its total global randomly selected non-accounting employees. We randomly select a group of non-accounting employees of a firm in the U.S., equal in size to the firm's accounting employees in the U.S. for each year prior to 2010. We then track their turnover throughout the sample period. We conduct the same random selection and turnover tracking the firm's non-accounting employees outside the U.S. [Revelio Labs]
US Sales	A firm's U.S. sales, as a proportion of its total global sales. [BvD Osiris]
Ln($US Sales)	The natural logarithm of a firm's sales in the U.S., measured in millions of US dollars. [BvD Osiris]
Ln($Non-US Sales)	The natural logarithm of a firm's sales outside the U.S., measured in millions of US dollars. [BvD Osiris]
US M&A	The natural logarithm of one plus the number of merger and acquisition (M&A) conducted by a firm, where the target firm is incorporated in the U.S. [BvD Orbis M&A]
Non-US M&A	The natural logarithm of one plus the number of merger and acquisition (M&A) conducted by a firm, where the target firm is incorporated outside the U.S. [BvD Orbis M&A]
High-Risk M&A	The natural logarithm of one plus the number of merger and acquisition (M&A) conducted by a firm, where the target firm is incorporated in highly corrupt countries. A country is classified as highly corrupt if it scored 5 or below (out of 10) on Transparency International's Corruption Perceptions Index in 2009. [BvD Orbis M&A]
Low-Risk M&A	The natural logarithm of one plus the number of cross-border merger and acquisition (M&A) conducted by a firm, where the target firm is not incorporated in a highly corrupt country in 2009. [BvD Orbis M&A]
Enter	An indicator variable set to 1 if an accountant newly joins a firm of a certain location in a given year, and 0 otherwise. [Revelio Labs]
Home	An indicator variable set to 1 if an accountant is working in a firm's home country in a given year, and 0 otherwise. [Revelio Labs, BvD Osiris]

(Continued)

Variable	Definition
NonHome	An indicator variable set to 1 if an accountant is working outside a firm's home country and outside the U.S. in a given year, and 0 otherwise. [Revelio Labs, BvD Osiris]
US	An indicator variable set to 1 if an accountant is working in the U.S. in a given year, and 0 otherwise. [Revelio Labs, BvD Osiris]
$Home^{HighCorruption}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has a higher corruption exposure, and 0 otherwise. Corruption exposure is defined as higher if a country's Corruption Perceptions Index is below 5 in 2009 [Revelio Labs, BvD Osiris, Transparency International]
$Home^{LowCorruption}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has a lower corruption exposure, and 0 otherwise. Corruption exposure is defined as lower if a country's Corruption Perceptions Index is above 5 in 2009 [Revelio Labs, BvD Osiris, Transparency International]
$NonHome^{HighCorruption}$	An indicator variable is set to 1 if *Non-Home* equals 1 and the country in which the accountant is working has a higher corruption exposure, and 0 otherwise. [Revelio Labs, BvD Osiris, Transparency International]
$NonHome^{LowCorruption}$	An indicator variable is set to 1 if *Non-Home* equals 1 and the country in which the accountant is working has a lower corruption exposure, and 0 otherwise. [Revelio Labs, BvD Osiris, Transparency International]
$Home^{MoU}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has MoUs with the SEC in a year, and 0 otherwise. [Revelio Labs, BvD Osiris, SEC Website]
$Home^{MoU}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has no MoUs with the SEC in a year, and 0 otherwise. [Revelio Labs, BvD Osiris, SEC Website]
$NonHome^{MoU}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has MoUs with the SEC in a year, and 0 otherwise. [Revelio Labs, BvD Osiris, SEC Website]
$NonHome^{MoU}$	An indicator variable is set to 1 if *Home* equals 1 and the country in which the accountant is working has no MoUs with the SEC in a year, and 0 otherwise. [Revelio Labs, BvD Osiris, SEC Website]

(Continued)

Variable	Definition
$USAcct^{JobPost}$	The number of accountant job postings by the firm in the U.S. for a given quarter. [Burning Glass Technologies]
$FCPA\ Specialization^{D}$	An indicator variable set to 1 if a firm has segments located in the states with SEC's FCPA investigative specialists, and 0 otherwise. The states with SEC's FCPA investigative specialists are Utah, Washington, Oregon, California, Nevada, Idaho, Alaska, Hawaii, Montana, Texas, Louisiana, Mississippi, Arizona, Florida, Oklahoma, Vermont, Maine, Kansas, Arkansas, New Hampshire, Massachusetts, Rhode Island and Connecticut.[Herrmann et al. (2024)]
$FCPA\ Specialization^{\#}$	The number of segments operating in states with SEC FCPA specialists. [Herrmann et al. (2024)]
DACC	The discretionary accruals, calculated by sector and year using the Modified Jones model Dechow et al. (1995), without a constant term. [BvD Osiris]
F-Score	The predicted probability of financial misstatement for a firm in a given year, developed in Dechow et al. (2011). [BvD Osiris]
TFP1	A firm's total factor productivity (TFP), calculated following the methodology in Schoar (2002). For all firms in a country-year, we regress the natural logarithm of sales on the natural logarithms of total assets, the natural logarithms of total number of employees and the natural logarithm of the cost for material. The firm's TFP is the residual from this regression. [BvD Osiris]
TFP2	A firm's total factor productivity (TFP) estimated at the country-industry-year level. Industry is defined at the level of two-digit SIC codes. [BvD Osiris]

Appendix B Robustness Tests

This table reports the results of robustness tests using Equation (1), which models the effect of the Program on the size of U.S. accounting departments. *US Acct* is the current number of U.S. accounting employees scaled by the firm's total global accounting employees. $USAcct^{Cum}$ is the number of accountants for a firm in the U.S., including those who work as accountants during the sample period but later move to other roles within the same firm, scaled by the firm's corresponding total global accountants, including those who work as accountants during the sample period but later move to other roles within the same firm. Other variables are defined in Appendix A. The testing sample includes 25,845 firm-years in the EB sample, and 24,154 in the PSM sample between 2005 and 2015. The PSM uses a logistic regression model with the dependent variable indicating registration with the SEC for a given year (*Treated*). The model controls for *ROA*, *Foreign Biz*, and *Sales* to estimate propensity scores, followed by 1:2 matching without replacement, where each treated firm is expected to matched with two control firms within the same sector classification (using US SIC codes). We include but do not report the intercepts and fixed effects for brevity. Standard error are clustered by home country, and *t*-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Dependent Variable:	*US Acct*			$US\ Acct^{Cum}$
	(1)	(2)	(3)	(4)
Method:	EB Unweighted	EB Pre-Period Weighted	PSM OLS	EB OLS
Treated*×*Post	**-0.012****	**-0.014****	**-0.015****	**-0.018*****
	(-2.02)	**(-2.66)**	**(-2.12)**	**(-3.39)**
ROA	0.001	-0.032	-0.000	-0.027
	(0.04)	(-1.33)	(-0.01)	(-1.14)
Foreign Biz	0.000	0.000	0.000	0.000
	(0.14)	(0.57)	(0.40)	(0.50)
Sales	0.001	0.000	0.000	0.000
	(1.59)	(0.49)	(0.56)	(0.19)
Firm FE	Yes	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
N	25,845	25,845	24,154	25,845
Adjusted R^2	0.801	0.831	0.815	0.833

Appendix C The Heterogeneity of Corporate Governance Quality

This table reports the results after considering the nonlinear relation between the Program and information efficiency, based on Equation (1) with the inclusion of $Treated \times HomeCG^{Top}$, $Post \times HomeCG^{Top}$, and $Treated \times Post \times HomeCG^{Top}$. $HomeCG^{Top}$ is equal to one for firms from countries with better corporate governance quality based on the level of "Regulatory Quality" in the database of Worldwide Governance Indicators (Kaufmann & Kraay (2024)), and zero otherwise. We define a country with better corporate governance quality if its "Regulatory Quality" is in the top 20% of the sample distribution in 2009. All other variables are defined in Appendix A. The testing sample includes 25,845 firm-years between 2005 and 2015. We include but do not report the intercepts and fixed effects for brevity. $Treated \times HomeCG^{Top}$ and $Post \times HomeCG^{Top}$ are absorbed after including the fixed effects. Standard error are clustered by home country, and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

	(1)	(2)	(3)
Dependent Variables:	*US Acct*	*DACC*	*F-Score*
$Treated \times Post \times HomeCG^{Top}$	**0.020***	**-0.008****	**-0.065***
	(1.74)	**(-2.47)**	**(-1.70)**
$Treated \times Post$	-0.020***	0.009***	0.025
	(-4.29)	(4.37)	(1.38)
ROA	-0.033	0.036***	1.180***
	(-1.31)	(4.77)	(12.24)
Foreign Biz	0.000	-0.000	-0.000
	(0.26)	(-0.66)	(-0.17)
Sales	0.000	-0.000	-0.024***
	(0.49)	(-1.09)	(-5.35)
Firm FE	Yes	Yes	Yes
Home Country×Year FE	Yes	Yes	YES
Industry×Year FE	Yes	Yes	YES
N	25,845	25,448	25,144
Adjusted R^2	0.833	0.366	0.411

Appendix D Distribution of Firms' Accountants

This appendix presents the global distribution of accountants, as well as those based in the U.S., within our sample firms. If an accountant joined a firm before 2010, we use her latest seniority level prior to 2010. If an accountant joined the firm in 2010 or later, we use the seniority from her first year of joining.

Seniority	Category	Example Titles	Global				U.S			
			# Accountants		# Accountant-years		# Accountants		# Accountant-years	
			Entry-Level Accountants							
1	Intern	Accounting Intern, Temporary Assistant Accountant	122,227	27.10%	418,701	24.68%	21,414	26.06%	78,765	23.87%
2	Junior	Account Receivable Bookkeeper, Accounts Payable Analyst	179,420	39.78%	646,076	38.08%	30,046	36.56%	115,831	35.11%
			Non-Entry-Level Accountants							
3	Associate	Senior Tax Accountant, Account Executive	48,640	10.78%	191,676	11.30%	8,540	10.39%	35,153	10.65%
4	Manager	Account Manager, Account Supervisor	52,577	11.66%	222,188	13.10%	10,188	12.40%	44,680	13.54%
5	Director	Chief of Accountants, Accounts Receivable Director	42,391	9.40%	190,678	11.24%	10,317	12.55%	47,501	14.40%
6	Executive	Director of Revenue, Accounting Director	4,390	0.97%	20,554	1.21%	1,371	1.67%	6,523	1.98%
7	Senior Executive	CFO	1,431	0.32%	6,860	0.40%	301	0.37%	1,468	0.44%
	Total		451,076	100.00%	1,696,733	100.00%	82,177	100.00%	329,921	100.00%

Appendix E Key Events of the SEC Whistleblower Program

Date	Description
26/03/2009	SEC Chair May Schapior, in her testimony before the Congress, revealed that the SEC will request for authority to compensate whistleblowers who bring the SEC well-documented evidence of fraudulent activity. [Testimony Concerning Enhancing Investor Protection and Regulation of the Securities Markets]
14/07/2009	SEC Chair Mary Schapiro, in her testimony before the Congress, revealed that the SEC is considering a legislative change to authorize a Whistleblower Program. This program would incentivize insiders to provide information that could lead to the successful enforcement of federal securities laws. This initiative laid the groundwork for the whistleblower provisions eventually included in the Dodd-Frank Act. [Testimony Concerning SEC Oversight]
21/07/2010	President Obama signed the Dodd-Frank Wall Street Reform and Consumer Protection Act into law, which includes Section 922 addressing whistleblower protections. Under the new regulations, whistleblowers who voluntarily expose inappropriate behavior in public corporations can receive substantial compensation and job protection. With the passage of the Dodd-Frank Act, Congress significantly expanded the SEC's authority to compensate individuals who provide information about any securities law violation. [Public Law 111–203]
22/07/2010	A whistleblower would be eligible to receive an award for original information provided to the SEC on or after July 22, 2010, but before the whistleblower rules become effective, so long as the whistleblower complies with all such rules once effective. [SEC]
3/11/2010	The SEC released proposed rules outlining the program's structure, detailing eligibility criteria for whistleblowers and the methods for compensation. [SEC 2010-213]
25/05/2011	The SEC finalized the rules governing the Program, defining how whistleblowers would be compensated and setting procedures for submitting information to the SEC. [SEC 2011-116]
12/08/2011	The final rules took effect, enabling whistleblowers to officially begin receiving rewards for providing original information that leads to successful enforcement actions by the SEC. [SEC 2011-167]