THE IMPACT OF ANTI-CORRUPTION LAWS:
EVIDENCE FROM THE U.K. BRIBERY ACT'S EXTRATERRITORIAL REACH

by

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This manuscript has been read and accepted by the Graduate Faculty in Business in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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ABSTRACT

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Advisor: Professor Donal Byard

I study the impact of anti-corruption laws by introducing a within-country foreign setting that exploits US multinational firms' differential exposure to the extraterritorial jurisdiction of the 2010 United Kingdom Bribery Act (UKBA). I show that adoption of the UKBA, which is stricter than the US Foreign Corrupt Practices Act (FCPA) in several key respects, induces US firms with material UK business to curb their exposure to corrupt countries, relative to US peers without material UK business. The effect is more pronounced for firms with higher enforcement risk and ex ante bribery risk. Following adoption, more US firms stop reporting their UK segment (compared to their German segment), consistent with some firms reducing their exposure to the UK's jurisdiction. Additionally, firms that reduce their UK exposure have relatively high pre-adoption business in corrupt countries. This study, which is the first to provide evidence on the effects of extraterritorial foreign anti-corruption laws on US firms, suggests that stricter anti-corruption laws abroad increase expected costs of bribery for US multinationals and thereby drive real changes to their international business. This study also demonstrates how extraterritoriality, which is increasingly common due to globalization, can be exploited to provide plausibly causal evidence of the impact of new laws and regulations.

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Table of Contents

List of Figures

List of Tables

List of Supplementary Tables

1. Introduction

Foreign bribery, and corruption more generally, has been recognized as a pervasive global problem with substantial economic effects.[1] The worldwide annual cost of bribery is estimated to be between 1.5 to 2 trillion USD, or roughly 2% of global GDP (IMF 2016). Prior studies suggest that corruption has a negative impact on a variety of outcomes, including economic growth, development, and educational attainment (Shleifer and Vishny 1993, Mauro 1995, 1998, Acemoglu and Robinson 2012, Ortiz-Ospina and Roser 2016).

Many countries recently adopted tougher anti-corruption laws in order to combat this problem, yet it remains unclear whether these laws reduce corruption. The few studies that have quantitatively evaluated the effectiveness of anti-corruption reforms yield conflicting evidence (e.g., Hines 1995, Wei 2000, Barassi and Zhou 2012, Christensen, Maffett, and Rauter 2020). Further, there is scant empirical evidence of the effects of extraterritorial jurisdiction in anti-corruption laws on foreign firms.[2] Extraterritorial jurisdiction is an increasingly common tool for targeting transnational issues. While controversial, extraterritoriality is perceived to be key to prosecuting multinational firms, which are major contributors to corruption (e.g., Hock 2017; see Section 2.2). Until recently, the United States (US) Foreign Corrupt Practices Act (FCPA) was the only anti-corruption law with significant extraterritorial implications for multinational firms (see, e.g., Christensen et al. 2020).[3]

This study, to the best of my knowledge, is the first to empirically assess the impact of ex-

[1] A common definition of corruption is "the abuse of entrusted power for private gain" (Transparency International 2019). Depending on the definition, corruption may include, in addition to bribery, activities such as embezzlement and extortion. Because bribery is the focus of both the US Foreign Corrupt Practices Act and the UK Bribery Act, I use the words "corruption" and "bribery" interchangeably. From the UK government's perspective, "Very generally, [bribery] is defined as giving someone a financial or other advantage to encourage that person to perform their functions or activities improperly or to reward that person for having already done so" (Ministry of Justice 2012b, p. 3).

[2] Extraterritorial jurisdiction in, or extraterritorial application of, domestic laws occurs when "a [country] increases its jurisdiction beyond its own boundaries and into another [country]'s territory (Lordi 2012, p. 957). It involves the "broad application and enforcement of national laws to subjects acting beyond the borders of a given country" (Hock 2017, p. 307). Section 2.2 provides further discussion.

[3] Christensen et al. (2020) focus on the extraterritorial enforcement of the US FCPA on non-US firms. Section 4.1 provides a review of the related literature.

traterritorial jurisdiction in a foreign country's domestic legislation on US firms. Specifically, I introduce a within-country foreign setting that focuses on the extraterritorial jurisdiction of the United Kingdom Bribery Act (UKBA) to examine whether the 2010 adoption of the UKBA curbs US multinational firms' (hereafter, "US firms") exposure to corrupt countries, or "corruption exposure."[4] The UKBA's expansive jurisdiction affects not only UK firms but also *non-UK firms that carry on business in the UK*, regardless of where the bribery occurs.[5] Whereas the US is widely regarded as a leader in the enforcement of anti-corruption laws, the effects of extraterritorial jurisdiction on *US firms* is a recent, less understood development. Given the central position of US multinationals in the global economy and the trend toward extraterritorial jurisdiction in anti-corruption and other laws, understanding whether and how non-US laws with extraterritorial jurisdiction affect US firms' business activities is important.

My identification strategy is based upon US firms' differential exposure to the UKBA, i.e., treatment (control) firms plausibly have (do not have) business in the UK and thus are (are not) subject to the UKBA's jurisdiction. Importantly, the UKBA is stricter than the FCPA in a number of ways. These incremental differences, along with plausibly credible enforcement, should increase the expected costs of bribery for treatment firms relative to control firms. A history of cooperation in white-collar criminal enforcement, along with US firms' awareness of the UKBA and its extraterritorial implications, further serve to legitimize the risk of UKBA enforcement against US firms with business in the UK. Sections 2 through 4 discuss details related to these arguments.

However, whether adoption of the UKBA affected US firms' corruption exposure is ultimately an open question. Corruption exposure may be unaffected if US firms do not perceive the UKBA as presenting a significant enforcement risk. It is also possible that differences between the FCPA and UKBA fail to prompt meaningful changes in US firms' internal anti-corruption pro-

[4] Actual corruption by firms is intentionally concealed and ex ante unobservable. I follow prior literature by utilizing a country-level index capturing perceived levels of corruption. For brevity, I refer to *perceivably* corrupt countries simply as "corrupt countries." Section 5 further describes data sources.

[5] Section 5.1 provides additional discussion on the UKBA's extraterritorial jurisdiction.

cedures in practice. Further, even in the case of credible enforcement and significant incremental exposure to the UKBA over and above the FCPA, the UKBA's effect depends upon each firm's calculation of the costs and benefits of paying bribes. For example, firms that do not pay bribes may be unable to compete with bribing peers in countries with lenient anti-corruption regimes (Hines 1995, Cuervo-Cazurra 2008, Darrough 2010, Zeume 2017). Paying bribes can also grease the wheels of business abroad by allowing firms to circumnavigate bureaucratic red tape in corrupt countries (e.g., Leff 1964, Beck and Maher 1986), and can yield positive net-present-value projects even after considering financial sanctions and reputational costs (Karpoff, Lee, and Martin 2017).

The within-country foreign setting used in this study also has several advantages in providing plausibly causal evidence of the effects of anti-corruption laws generally, which is lacking in the literature. For several reasons, empirical identification issues make it difficult to answer whether anti-corruption laws curb corruption. The potential for confounding contemporaneous events in the adopting country necessitates a benchmark against which to assess any observed impact. Identification of such a benchmark, however, is precluded in a single-country domestic setting because anti-corruption laws generally apply to all firms in the adopting country. Most studies therefore focus on cross-country analyses, which are also vulnerable to identification challenges. Isolating the effects of an anti-corruption law using a cross-country setting is difficult because the timing of adoption may coincide with confounding factors that affect firms in the treatment country but not those in control countries. For example, bribery-related scandals in a given country may raise public awareness regarding corruption and thus drive both the adoption of new anti-corruption laws and the curbing of corruption for firms based in the adopting country.[6]

The within-country nature of this setting mitigates concerns regarding concurrent country-wide policy or other institutional changes unrelated to the UKBA because both treatment and control firms are based in the US (and thus subject to similar economic and regulatory shocks). A

[6] Consistent with this idea, the findings in Hail, Tahoun, and Wang (2018) suggest that regulations are strongly reactive to corporate scandals.

within-country setting also avoids confounding changes in, and levels of, country-level characteristics affecting firms' exposure to corrupt countries. The foreign nature of this setting, which exploits the UKBA's extraterritorial jurisdiction to focus on non-UK firms, mitigates concerns related to contemporaneous events and the endogenous timing of adoption in the UK.

Using a sample of 5,077 US firm-year observations during 2007–2012 (six years around the 2010 adoption), I exploit a quasi-natural experimental setting that subjects *some* US firms to an additional anti-corruption law. I construct a treatment group of US firms with material business in the UK by identifying firms that disclose a pre-adoption UK geographic segment. A control group of US firms with limited or no exposure to the UKBA's jurisdiction is constructed by identifying firms that do *not* disclose a UK segment during the sample period. I utilize a difference-in-differences (DiD) design combined with the entropy balancing procedure to ensure greater similarity between treatment and control firms, rendering causal inferences more reliable (Hainmueller 2012). Using the 2009 Transparency International Corruption Perceptions Index (CPI), I construct a revenue-weighted firm-year measure of corruption exposure as the dependent variable.

Results of my main analysis, which controls for variables predicted to affect corruption exposure, as well as firm, industry×year, and segment country-year fixed effects, show that, compared to control firms, treatment firms exhibit significantly lower mean corruption exposure following adoption. The estimated treatment effect amounts to a 0.07 point decrease in the relative CPI score of the treatment group. In a falsification test, I replicate my main tests as though US firms with (without) material business in *Germany* should (should not) be affected by the UKBA.[7] As expected, these placebo regressions do not produce significant results.

Results of additional analyses corroborate evidence that adoption of the UKBA drives the main results. Extending the sample period to include five additional years in the post-adoption pe-

[7] Germany serves as a comparable benchmark country for the UK for a number of reasons. Like the UK, Germany has relatively high enforcement strength and low levels of corruption, and the two countries have similar cooperative arrangements with the US with respect to white-collar criminal enforcement. Germany is also a large developed economy in Western Europe, and, after the UK, is the next-most frequent European segment disclosed by US firms.

riod indicates a stronger estimated effect, consistent with increased enforcement under the UKBA increasing US firms' incentive to curb corruption exposure. Next, I test whether the effect of adoption is more pronounced for firms with higher ex ante incremental risk exposure to the UKBA over and above the FCPA. Specifically, unlike under the FCPA, the UKBA prohibits facilitating payments, which differ from bribes in that they are not made to win a business contract but rather to induce public officials to perform or expedite routine actions, such as acquiring permits. I find the estimated treatment effect is substantially larger for firms with a relatively high proportion of pre-adoption revenues from countries where facilitating payments are most prevalent. I also find a stronger estimated effect after conditioning on proxies for bribery and enforcement risk.

In addition, I provide evidence consistent with some treatment firms limiting their exposure to the UKBA's jurisdiction by curbing their UK business presence. I show that, following adoption, the estimated likelihood that a treatment firm stops disclosing a UK segment is uncharacteristically high, relative to the likelihood that a firm with material pre-adoption business in Germany stops disclosing a German segment. Firms that discontinue UK segment disclosure (comprising 15.9% of treatment firms) have relatively high exposure to corrupt countries in the pre-adoption period.

This study, which is the first to empirically assess the impact of extraterritorial jurisdiction in foreign anti-corruption laws on US firms, provides evidence suggesting that such laws curb US firms' business activity in corrupt countries. This evidence is timely given the increasing prevalence of anti-corruption and other laws with extraterritorial application in countries outside of the US (e.g., Arrieta 2016). Implications for US firms are also of interest given the contribution of US-based multinational firms to the global economy, e.g., an estimated 5.3 trillion USD of worldwide value added (Bureau of Economic Analysis 2017). By showing that the UKBA leads to incremental changes in US firms' corruption exposure over and above the FCPA's impact, my findings are consistent with extraterritoriality enhancing the effectiveness of anti-corruption laws. This study thus provides evidence of a potential benefit of extraterritoriality, thereby contributing to debates surrounding its appropriateness (see Section 2.2). My findings complement concurrent research on

the extraterritorial impact of US FCPA enforcement on non-US firms (Christensen et al. 2020).

Further, I provide plausibly causal evidence that anti-corruption laws curb firms' exposure to corrupt countries, thereby contributing to the mixed literature on the effectiveness of anti-corruption laws generally, and to a broad recent literature examining various effects of anti-corruption initiatives on public firms (e.g., Karpoff et al. 2017, Zeume 2017, Griffin, Liu, and Shu 2018, Rauter 2019). This evidence complements findings from studies focusing on whether anti-corruption laws curb corruption in firms based in the adopting country (e.g., Hines 1995, Wei 2000, Zeume 2017). In contrast to these studies, I focus on understanding the impact of extraterritoriality in these laws by using a within-country foreign setting, which has the advantage of mitigating identification issues inherent in cross-country regulatory studies. I also contribute evidence on another outcome of anti-corruption laws: I show that certain US firms discontinue UK segment disclosure following adoption of the UKBA, consistent with anti-corruption laws potentially discouraging foreign firms subject to the law from carrying on business in the adopting country.

Finally, my study demonstrates how extraterritoriality can be exploited to provide plausibly causal evidence of the effects of laws and regulations. Extraterritorial jurisdiction is increasingly used to address transnational issues, introducing promising opportunities for future research.[8] With respect to accounting research, for instance, the UK adopted the Modern Slavery Act in 2015, which requires non-UK firms with business in the UK to provide public disclosures regarding steps taken to avoid labor exploitation in their supply chains. Although causal evidence is a critical input in cost–benefit analyses of regulatory changes, empirical identification challenges often preclude causal inferences (see Leuz and Wysocki 2016). The within-country foreign setting and design utilized in this study alleviates important identification concerns pervasive in regulatory studies, arising from country-level factors and the endogenous timing of new regulation in the adopting country (e.g., see Daske, Hail, Leuz, and Verdi 2008, Christensen, Hail, and Leuz 2013, Isidro,

[8] For example, the 2017 UK Criminal Finances Act, which prohibits firms from failing to prevent tax evasion, has extraterritorial application to non-UK firms with business in the UK. In addition, the European Union (EU) General Data Protection Regulation (GDPR) extends to non-EU firms offering goods and services to EU "data subjects."

Nanda, and Wysocki 2019).[9]

The remainder of this paper is structured as follows. Sections 2 and 3 provide details on institutional background and enforcement of the UKBA, respectively. Section 4 discusses related literature and develops the hypothesis. Section 5 provides details on data, measures, and sample construction. Section 6 presents the main empirical analyses. Sections 7 through 9 present additional and robustness analyses, and Section 10 concludes.

2. Institutional Background

2.1. Overview of the UK Bribery Act

In 1997, member countries of the Organisation for Economic Co-operation and Development (OECD), including the UK and the US, signed the *Convention on Combating Bribery of Foreign Public Officials in International Business Transactions* (hereafter, the "Anti-Bribery Convention"). The Anti-Bribery Convention, which became effective in 1999, was the first international anti-corruption agreement; it establishes legally binding standards for OECD member countries to criminalize the payment of bribes to foreign public officials. The 2010 adoption of the Bribery Act in the United Kingdom (UKBA) was a response to pressure from the OECD Working Group on Bribery, which criticized the UK for failing to implement and enforce anti-corruption legislation in accordance with the Anti-Bribery Convention (OECD 2008). Other countries were also slow to implement and enforce laws in line with the convention, but a number of factors subjected the UK to especially harsh criticism, including significant deficiencies in existing anti-corruption laws, a lack of anti-corruption enforcement, and a major bribery scandal.[10]

The UKBA was drafted in March 2009, received Royal Assent in April 2010, and has been

[9] For instance, policy reforms are frequently bundled with other regulatory or institutional changes. Prior studies highlight this issue by showing, for example, that changes in liquidity around the mandatory adoption of International Financial Reporting Standards (IFRS) are largely attributable to concurrent increases in accounting enforcement (Christensen et al. 2013).

[10] In the buildup to the UKBA, the case of a large UK-Saudi arms deal contracted by BAE Systems generated significant controversy due to the UK's decision to discontinue its long-running investigation into bribery charges (Transparency International 2010).

enforced since July 2011. It is widely recognized as one of the strictest anti-corruption laws internationally. The UKBA makes it illegal to pay bribes to foreign public officials and persons in the private sector, and to receive bribes. Criminal penalties for violating the UKBA include unlimited financial penalties and potential debarment for firms and individuals, and up to ten years imprisonment for liable individuals.

Notably, section 7 of the UKBA establishes an offense for a bribing firm that fails to prevent bribery by its employees, agents, or subsidiaries (hereafter, the "section 7 offense"). The jurisdiction covered by the section 7 offense extends not only to UK firms operating in the UK and abroad, but also to *non-UK firms* carrying on business or part of a business in the UK, even if the bribery has no direct connection to the UK.[11] The section 7 offense applies to the offenses of making bribe payments to foreign public officials and persons in the private sector. The UK government adopts a "common sense approach" in establishing what constitutes carrying on UK business, designating the courts as the final arbiter in any disputes. In Sections 3 and 5.1, I provide further context surrounding the UKBA's applicability to non-UK firms.

2.2. Extraterritorial Jurisdiction

Globalization has led to the increasing prevalence of extraterritorial jurisdiction in domestic laws. Extraterritoriality has especially gained popularity in laws targeting global issues such as corruption, human rights, data privacy, and taxation. A primary goal of extraterritoriality is to influence the behavior of, and more effectively prosecute, multinational firms, whose transactions are major contributors to these global issues (e.g., Hock 2017).

Extraterritoriality can influence multinational firms' behavior by encouraging compliance with multilateral treaties such as the Anti-Bribery Convention. Compliance with the Anti-Bribery Convention has proved difficult for two main reasons (Schuman 2011). First, the Anti-Bribery

[11] A "relevant commercial organisation" under the section 7 offense is "a body or partnership incorporated or formed in the UK irrespective of where it carries on a business, or an incorporated body or partnership which carries on a business or part of a business in the UK irrespective of the place of incorporation or formation" (Ministry of Justice 2012a, p. 15).

Convention does not have its own enforcement mechanism. Second, member countries have incentives to avoid enforcement in order to secure a competitive advantage for domestic firms. That is, without the ability to prosecute bribery globally, as more and more countries enact and enforce anti-corruption laws, incentives increase for other countries to not enact or weakly enforce their own anti-corruption laws. The solution to this "collective action problem" requires countries to adopt laws that can be enforced beyond their borders (e.g., Lestelle 2008).

Prior to adoption of the UKBA, the US FCPA was the only domestic anti-corruption law with significant extraterritorial implications for multinational firms. Scholarly evidence and practitioner viewpoints suggest that extraterritorial FCPA enforcement helps mitigate the collective action problem (e.g., Schuman 2011, Christensen et al. 2020). For one, FCPA enforcement against non-US firms encourages other member countries to pursue investigations and prosecutions in accordance with the Anti-Bribery Convention. The country where an FCPA-violating firm is based often brings its own enforcement action against the firm. FCPA enforcement also pressures non-US firms into adopting anti-corruption compliance systems, thus reducing noncompliance and consequently any competitive advantage for firms in countries failing to address anti-corruption violations (Schuman 2011). In sum, extraterritorial jurisdiction helps achieve a level playing field for domestic firms competing with foreign firms in countries with weak anti-corruption regimes.

Despite its potential benefits, extraterritoriality in national laws is controversial because it can serve national interests and negatively impact international relations, principles of international order, and financial markets (Hock 2017). Some argue that aggressive extraterritorial jurisdiction violates international law by failing to respect the exclusive authority of other countries to govern behavior within their territory (e.g., see Windsor and Getz 2000, Schuman 2011, Lordi 2012). Countries that did not sign the Anti-Bribery Convention, for instance, never agreed to the extraterritorial enforcement of firms headquartered within their borders (Schuman 2011). Lordi (2012) focuses on the UKBA and argues that such far-reaching extraterritoriality in anti-corruption laws is inappropriate in part because the world is not a "global village" with one value system. Im-

posing values on those in countries with incompatible moral constitutions can lead to a variety of issues, including the potential for host country resentment and political conflict. On the other hand, such arguments are arguably weakened by the broad jurisdictional approach endorsed by the Anti-Bribery Convention (see Brewster 2017). In addition, Lestelle (2008) argues that humanitarian issues inherent in foreign bribery justify expansive jurisdiction in anti-corruption laws.

2.3. Comparing the UK Bribery Act and the US Foreign Corrupt Practices Act

The 1977 FCPA in the United States was the first law to explicitly make it illegal to bribe foreign public officials. This study's focus on US firms calls for a summary of the differences between the FCPA and the UKBA (for an in-depth comparison, see Warin, Falconer, and Diamant 2010). From the perspective of US firms subject to the UKBA's jurisdiction, compliance with the UKBA does not necessarily follow from compliance with the FCPA. The UKBA is stricter and broader than the FCPA in several respects, making it necessary for US firms subject to the UKBA to reassess their anti-corruption compliance systems (e.g., Warin et al. 2010, Lippman 2013).

First, unlike the FCPA, the UKBA does not permit facilitating (or "grease") payments. Facilitating payments secure or expedite a routine governmental action that is part of a foreign public official's duties, such as issuing a license or work permit. Facilitating payments are frequently a slippery slope to more serious corruption (e.g., Argandoña 2005). Second, the UKBA applies not only to bribing foreign public officials, but also to the private sector, i.e., firm-to-firm or commercial bribery. Third, the UKBA provides for an affirmative defense against the section 7 offense of failing to prevent bribery. In other words, a firm has a full defense in court if it can prove that it had "adequate" anti-bribery procedures in place to prevent a particular case of bribery from occurring. Fourth, in contrast to the FCPA, the UKBA does not provide for an affirmative defense for reasonable and *bona fide* hospitality expenses provided to government officials. A final difference is that the UKBA criminalizes the receipt of bribes (i.e., passive bribery), whereas the FCPA does not. The offense of receiving bribes, however, is not covered by the section 7 offense and thus its

jurisdiction does not generally extend to US firms with business in the UK.

3. Enforcement of the UK Bribery Act

The discussion in this section demonstrates that the UKBA plausibly presents a credible enforcement risk to US firms. The first subsection includes a brief overview of the pre-UKBA anti-corruption laws, the role of the UKBA in strengthening the UK's anti-corruption regime, and expected enforcement of the UKBA generally. The second subsection discusses settled anti-corruption cases in the UK. The final subsection focuses on additional areas expected to influence US firms' perception of enforcement risk.

3.1. The UK Anti-Corruption Regime

At the time of adoption, whether the UKBA would be strictly enforced was an open question. The patchwork of pre-UKBA anti-corruption statutes and common law, which dated back over a century, frequently failed to support effective enforcement. Deficiencies in the UK's earlier laws on corporate liability and bribing foreign public officials hindered investigations. The earlier anti-corruption regime also lacked clarity in several important respects. For example, it seemed to permit using a non-UK intermediary to bribe foreign public officials so long as the bribe took place outside the UK (OECD 2008).

The UKBA modernized the law to address increasingly sophisticated cross-border bribery and facilitate more effective prosecutions of bribery within the UK and overseas (Ministry of Justice 2009b). Statements from UK Secretary of State for Justice offer insight into the goal of the UKBA and the laws it replaced:

> *Ultimately, the Bribery Act matters for Britain because our existing legislation is out of date. In updating our rules, I say to our international partners that the UK wants to play a leading role in stamping out corruption and supporting trade-led international development.* (Ministry of Justice 2012a, p. 2-3).

> *[The UKBA] will provide the basis for a modern, clear and consolidated law that complements and supports our international efforts and equips our courts and prosecutors to deal effectively with bribery of all kinds, wherever it occurs* (Ministry of Justice 2009a, p. 3).

The UKBA was expected to have a widespread and significant impact if enforced to the full potential of its statutory language (e.g., Lippman 2013). Many legal experts anticipated strict enforcement (e.g., Bonneau 2010, Warin et al. 2010, Jordan 2011). In combination with the UK's strong system of legal enforcement (e.g., La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1998), the modernized law thus presented a nontrivial enforcement risk. The UK Serious Fraud Office (SFO), which prosecutes serious and complex cases of financial crime, is the main agency responsible for enforcing the UKBA. Public criticism of the pre-UKBA anti-corruption regime likely heightened enforcement risk by placing the SFO under substantial pressure to effectively enforce the new law.

3.2. Anti-Bribery Cases in the UK

Over the period 2008 through 2018, there were 86 (31) concluded total (corporate) anti-bribery cases in the UK. Of the 86 (31) cases, 80 (27) were concluded in the post-adoption period, i.e., after 2009 (EY 2019).[12] Most of these cases were brought under pre-UKBA anti-corruption statutes or common law because the UKBA is not retroactive. During the keynote address at the FCPA Conference in December 2018, the director of the SFO stated there were "dozens of bribery and corruption cases in the investigative pipeline" (Osofsky 2018), several of which have been categorized by the OECD Working Group on Bribery as "high profile, multijurisdictional, high value and complex" (OECD 2017, p. 50).

UKBA cases accounted for 16 (5) of the 86 (31) concluded cases through 2018. As discussed in Section 2.1, the jurisdiction of the UKBA's section 7 offense extends to non-UK firms with business in the UK. Expected enforcement of the section 7 offense should undoubtedly influence whether and to what extent non-UK firms adjust their business activities in corrupt countries following adoption of the UKBA. Figure 1 displays the count of UKBA enforcement actions by year; section 7 enforcement actions are presented separately in dark shading.

[INCLUDE FIGURE 1 ABOUT HERE]

[12] 2008 was the first year of sustained anti-corruption enforcement in the UK; the most recent convictions prior to 2008 occurred in 2002 (EY 2012).

A limited number of enforcement actions in the early years of a new anti-corruption law is hardly surprising and not necessarily reflective of future enforcement strength. In any enforcement area, settlements and convictions frequently lag the adoption of a new law because investigations take time to complete. The complexity involved in anti-corruption cases, where wrongdoing often takes place overseas, is especially conducive to lengthy investigation periods. For example, the average length of an FCPA investigation is over 3 years (Stanford Law School 2019). In the case of the FCPA, despite a shift in policy toward enforcement in 1997, a sustained increase in FCPA settlements did not materialize until the early- to mid-2000s (see, e.g., Brewster 2017, Christensen et al. 2020). Adjusting to a new law also involves a learning curve, for firms subject to the law and for regulators and authorities charged with the law's enforcement. Legal experts therefore predicted that the UKBA's most immediate effect would be to encourage compliance (e.g., Koehler 2011).

With respect to cases involving the section 7 offense, SFO statements indicate a commitment to prosecuting non-UK firms in violation of the UKBA.[13] The ongoing investigation into the UK subsidiaries of US firm KBR, Inc. also demonstrates a commitment to prosecuting non-UK firms.[14] Further, a recent judgment concerning Airbus SE, a Netherlands parent company with headquarters in France, reflects a strict interpretation of extraterritorial reach. Although the corruption in this case occurred entirely outside the UK, Airbus SE had continuously carried out business in the UK. The judgment does not make reference to the proportion of Airbus SE's UK revenues nor to the potential improper behavior of its UK subsidiaries, suggesting a broad interpretation of the criteria subjecting a foreign firm to prosecution under the UKBA's extraterritorial jurisdiction (Breen 2020).

Through 2018, one of the five settled section 7 enforcement actions in Figure 1 involved a non-UK subsidiary of a UK firm: Rolls-Royce Energy Systems Inc. Specifically, the UK led a criminal investigation during 2013–2017 into bribes paid by Rolls-Royce plc (a UK firm) and Rolls-

[13] See, for example, https://www.theguardian.com/law/2011/mar/25/serious-fraud-office-overseas-firms-bribery-act and https://www.sfo.gov.uk/2016/10/27/gain-2016-serious-fraud-offices-current-direction-enforcement-priorities (accessed December 2019).

[14] The KBR Inc. investigation announcement can be found here: https://www.sfo.gov.uk/cases/kbr-inc/ (accessed December 2019).

Royce Energy Systems Inc. (its US subsidiary). The investigation culminated in financial penalties of over 628 million USD in the UK, 170 million USD in the US (for violating the FCPA), and 25 million USD in Brazil.

3.3. US Firm Awareness of the UKBA and UK-US Cooperation

This subsection discusses two important components of US firms' perception of UKBA enforcement risk (note that a complete discussion of the expected costs imposed by the UKBA on US firms is relegated to Section 4.2). The first of the two components regards US firms' awareness of the UKBA and its extraterritorial implications. Bribery-related disclosures in 10-K filings suggest that, around adoption of the UKBA, US firms had knowledge of the new law and anticipated potentially significant costs. For example, an excerpt from Atmel Corporation's 2010 10-K Item 1A: Risk Factors disclosure is provided below. Additional examples of 10-K disclosures related to the UKBA can be found in Appendix A.2.

> *The United Kingdom, where we have operations, has recently adopted, but not yet implemented, the U.K. Bribery Act that could impose significant oversight obligations on us and could have application to our operations outside of the United Kingdom. The costs for complying with these and similar laws may be significant and could reasonably be expected to require significant management time and focus. Any violation of these or similar laws, intentional or unintentional, could have a material adverse effect on our business, financial condition or results of operations.*[15]

US firms also disclose UKBA-related information in Form 8-Ks. In 2011, the Wall Street Journal published an article describing a change in Lockheed Martin Corporation's internal anti-corruption policy in response to the UKBA. Prior to adoption of the UKBA, Lockheed Martin's internal policy did not prohibit facilitating payments, though they were discouraged and limited to 100 USD. The pre-UKBA policy also did not make mention of commercial bribery, which is illegal under the UKBA but not the FCPA. Finally, the revised policy requires the firm to conduct risk-based anti-corruption due diligence of all international partners (e.g., suppliers and consultants).[16] A web

[15] See https://www.sec.gov/Archives/edgar/data/872448/000087244816000043/atml-201510k.htm (p.19; accessed October 2019).

[16] See https://blogs.wsj.com/corruption-currents/2011/06/08/lockheed-martin-gets-into-step-with-uk-bribery-act-with-new-policy/ (accessed April 2020).

search also suggests a high volume of communication from law firms regarding US multinational firms' exposure to the UKBA.[17]

The second of the two components is the degree of cooperation between UK and US regulators in anti-corruption enforcement. The importance of international cooperation in anti-corruption enforcement is well-established (e.g., Brewster and Buell 2017). A primary challenge in foreign bribery cases is obtaining evidence and receiving mutual legal assistance from certain jurisdictions (e.g., OECD 2017). For example, descriptive findings from Christensen et al. (2020) indicate that over 50% of FCPA cases against non-US firms involve foreign cooperation.

The US has an established pre-adoption history of cooperating with the UK in investigating and prosecuting white-collar crime. Cooperation between the UK and US is facilitated by multilateral treaties such as the Anti-Bribery Convention, which requires OECD member countries to provide legal assistance to other member countries in foreign bribery investigations, and the 2005 United Nations Convention Against Corruption. The International Organization of Securities Commissions (IOSCO) Multilateral Memorandum of Understanding (MMoU) between the two countries also facilitates cooperation through information sharing among securities regulators (Lang, Maffett, Omartian, and Silvers 2019, Silvers 2016, 2019).[18] FCPA cases also illustrate the strong cooperative relationship between the UK and US. FCPA enforcement actions against UK firms are the second-most frequent among all enforcement actions against non-US firms based in countries that signed the Anti-Bribery Convention (Christensen et al. 2020, Table 2). In general, countries

[17] For example, see https://www.reedsmith.com/-/media/files/perspectives/2010/07/what-the-new-uk-bribery-act-2010-means-for-us-comp/files/what-the-new-uk-bribery-act-2010-means-for-us-comp/fileattachment/what-the-new-uk-bribery-act-2010-means-for-us-comp.pdf and https://www.paulweiss.com/media/110677/ukbriberyactr10withcover.pdf (accessed December 2019).

[18] There is also anecdotal evidence of the UK-US cooperative relationship. The former head of the Department of Justice (DOJ) FCPA Enforcement Unit stated, "[...] in many respects [the UK is] our closest law enforcement partner [...] Many of [the cases under UK investigation] are cases that we are working on as well, in cooperation with the Serious Fraud Office and the Metropolitan Police" (Mendelsohn 2009, transcribed interview). The International Development Committee of the UK House of Commons have also referred to the DOJ as the SFO's "closest partner," and states that the agencies "have a number of joint investigations and work together very closely in this area" (House of Commons 2011, p. 65). Additionally, the 2003 UK-US Extradition Treaty strengthened the ability of the two countries to extradite white-collar criminal offenders.

that signed the Anti-Bribery Convention (like the US) have a significantly higher chance of being targeted by the UKBA; in the FCPA context, nearly all cases against non-US firms involve firms headquartered in these countries (Christensen et al. 2020, Figure 1e)

Importantly, the UK-US cooperative relationship in white-collar criminal enforcement predates the UKBA, thereby limiting the possibility that any observed effect is driven by a concurrent increase in FCPA enforcement cooperation between the two countries. For both countries, the Anti-Bribery Convention entry-into-force date was February 15, 1999, a decade prior to passage of the UKBA. Formal changes in UK-US bilateral information-sharing arrangements occurred prior to 2004 (see Friedman, Jacobs, and Macel 2002), and there is no evidence of agreements after that time that would facilitate a disproportionate increase in FCPA enforcement cooperation between the US and UK, relative to that between the US and other countries.

4. Related Literature and Hypothesis Development

4.1. Related Literature

There are virtually no empirical studies examining the extraterritorial impact of anti-corruption (or other) laws. An exception is Christensen et al. (2020), who examine the effect of extraterritorial FCPA enforcement on non-US firms. Specifically, Christensen et al. (2020) show that an increase in FCPA enforcement reduces foreign direct investment in corrupt countries by US firms and non-US firms based in countries that agree to cooperate with US regulators (by ratifying the Anti-Bribery Convention). The distinction between whether the extraterritorial reach of US laws affects non-US firms and whether the extraterritorial reach of non-US laws affects US firms is not trivial. The US is recognized as a leader in anti-corruption enforcement of non-US firms (e.g., Brewster 2017). As the world becomes more globalized, however, other countries are beginning to adopt laws with extraterritorial reach (e.g., Sapin II in France)—we lack evidence on the effects of these laws. Examining the effect of extraterritorial jurisdiction on US firms is of particular interest given the major role of the US in the global economy (Bureau of Economic Analysis 2017).

The remainder of the literature discussed focuses on the effect of anti-corruption laws on domestic subjects, i.e., not on their extraterritorial effect. In general, much of the extant research examining whether anti-corruption laws curb corruption does not examine laws that criminalize corrupt acts, e.g., the UKBA and the FCPA, but rather on information transparency laws, e.g., freedom of information laws and mandated financial and conflict of interest disclosures by politicians (Johnsøn, Taxell, and Zaum 2012). These studies produce conflicting findings (e.g., Tavares 2007, Djankov, La Porta, Lopez-de-Silanes, and Shleifer 2010).

The few studies that quantitatively examine the FCPA's impact on business activity in corrupt countries yield mixed evidence and focus on aggregate rather than firm-level outcomes. Hines (1995) documents a decline in US business activity (i.e., foreign direct investment, joint venture activity, capital/labor ratios, and aircraft exports) in corrupt countries following FCPA adoption. In contrast, Wei (2000) and Smarzynska and Wei (2000) find that, after adoption of the FCPA (but before the Anti-Bribery Convention), US investors are not more sensitive to investing in corrupt countries as compared to investors from other OECD countries that did not have laws explicitly banning bribery abroad. Studies examining the impact of the FCPA on US exports is also mixed: evidence from Beck, Maher, and Tschoegl (1991) suggests that US export market share declined in corrupt countries overall, particularly in non-Latin American countries, where US firms lack regional advantages, while evidence from Graham (1984) suggests no decline in US exports to corrupt countries overall.

Empirical studies focusing on anti-corruption laws adopted in countries outside of the US are rare. Scant research exists on the impact of the UKBA; an exception is Zeume (2017), who focuses on UK firms and finds: (1) a decline in firm value for UK firms operating in corrupt countries; (2) an increase in firm value for non-UK competitors of UK firms; and (3) a decline in sales, M&A activity, and the expansion of subsidiaries in non-OECD countries for UK firms, relative to non-UK competitors. In contrast to this study, the findings in Zeume (2017) do not focus on extraterritorial effects of anti-corruption laws. The analyses in this study are also less vulnerable to

identification challenges relative to cross-country analyses, as discussed previously. With respect to anti-corruption policies in China, evidence from Griffin et al. (2018) suggests that the Chinese anti-corruption campaign had little impact on corporate corruption culture.

Studies focusing on legally binding international anti-corruption treaties also produce conflicting evidence. Cuervo-Cazurra (2008) finds that US investors subject to the FCPA reduce their investment in corrupt countries only after the Anti-Bribery Convention became effective in 1999, suggesting that international anti-corruption instruments facilitate a level competitive playing field. Findings from D'Souza (2012) and Blundell-Wignall and Roulet (2017) are also consistent with the Anti-Bribery Convention curbing investment in corrupt countries. On the other hand, Barassi and Zhou (2012) find that the Anti-Bribery Convention only minimally reduced multinational firms' investment in corrupt countries, and Mungiu-Pippidi (2011) finds no evidence that ratification of the United Nations Convention against Corruption curbs corruption.

4.2. Hypothesis Development

Prior studies demonstrate that bribery is pervasive and not limited to firms based in poor, developing countries (e.g., Shleifer and Vishny 1993). For example, Enterprise Survey data from the World Bank suggest that approximately 32% of multinational firms pay bribes; further, 14% of multinational firms headquartered in OECD countries report bribing, with the average bribe paid amounting to around 5.63% of the contract value (D'Souza and Kaufmann 2013). In deterring corruption, governmental monitoring and enforcement plays a significant role (e.g., Olken 2007).

The strength and transparency of the economic and political institutions of countries where firms do business are important determinants of firms' propensity to bribe (e.g., Shleifer and Vishny 1993, D'Souza and Kaufmann 2013). By raising the cost of doing business in countries where corruption is prevalent, the UKBA could induce US firms under its jurisdiction to curb their business activity in such countries.

Litigation risk for bribing US firms is determined by the probability of detection, expected

penalties, and exposure to incremental UKBA provisions not covered by the FCPA. The probability of detecting UKBA violations is at least as large as in the pre-adoption period because the UKBA introduces potential monitoring by UK regulators on top of FCPA monitoring by US regulators. Expected penalties increase following adoption of the UKBA since US firms convicted of a section 7 offense are subject to unlimited fines. The possibility of joint investigations also leads to higher expected penalties in the post-adoption period. For example, the UKBA permits UK regulators to open their own investigation into a US firm with business in the UK following announcement of an FCPA investigation of said firm, or to collaborate with US regulators in a joint settlement. Prior to UKBA adoption, the UK could not prosecute a US firm merely for conducting business in the UK. Finally, the UKBA is stricter than the FCPA in several respects (see Section 2.3), increasing the possibility that US firms' business activities in corrupt countries constitutes an anti-corruption violation. For these reasons, US firms subject to the UKBA's jurisdiction plausibly face greater anti-corruption litigation risk in the post-adoption period relative to the pre-adoption period.

The UKBA could also impose additional compliance costs on US firms under its jurisdiction. US firms may implement or improve anti-corruption compliance procedures to cover the incremental prohibitions of the UKBA over the FCPA (e.g., see the Lockheed Martin discussion in Section 3.3). Compliance costs may also increase as a result of higher audit fees induced by exposure to the UKBA (Gutiérrez Urtiaga, Hadjigavriel, and Gago Rodríguez 2020). Even firms that do not contemplate bribery could face greater compliance costs under the UKBA because statutory ambiguities in anti-corruption laws make it possible for such firms to nevertheless be held liable, leading to overcompliance (Lippitt 2013). Apart from litigation and compliance costs, UKBA violations could also reduce firm value due to lost future contracts, the cost of legal settlements and internal investigations, and the reputational impact of accusations and investigations (e.g., Nichols 2012, Zeume 2017, Sampath, Gardberg, and Rahman 2018).

Based upon the discussion provided in Section 3, the UKBA plausibly presented a credible enforcement threat to US firms. Further, evidence that US firms have awareness of the UKBA,

as well as the UK-US cooperative relationship in white-collar criminal enforcement, increase the likelihood that adoption of the UKBA raises US firms' cost of doing business in corrupt countries (see Section 3.3). For the aforementioned reasons, I predict that, following adoption of the UKBA, US firms with material business activity in the UK (i.e., treatment firms) curb their exposure to corrupt countries, relative to US firms without material business activity in the UK.

However, whether adoption of the UKBA leads treatment firms to curb their exposure to corrupt countries is ultimately an open question. At a high level, corruption is an outcome of political, economic, and cultural institutions (e.g., Svensson 2005, Acemoglu and Robinson 2012) and whether anti-corruption laws "work" remains unclear (e.g., Johnsøn et al. 2012). US firms may not curb corruption exposure if the UKBA is not perceived to be an effective deterrent, or if statutory provisions of the UKBA over and above the FCPA do not manifest in significant differences in practice (e.g., see Warin et al. 2010). For example, if most US firms voluntarily implement pre-adoption internal policies prohibiting facilitating payments, the UKBA's effect may be minimal.

Even if the UKBA is viewed as an effective deterrent and US firms are significantly exposed to the UKBA's incremental provisions, the UKBA's impact depends upon each firm's calculation of the costs relative to the benefits of paying bribes. For some firms, the post-adoption benefits of bribing may still exceed the costs. Firms that comply with anti-corruption laws may be at a competitive disadvantage, particularly when competitors are based in countries with lax anti-corruption regimes (e.g., Hines 1995, Cuervo-Cazurra 2008, Darrough 2010, Zeume 2017). Paying bribes allows firms to work around rigid economic regulation and administrative hurdles in corrupt countries (e.g., Leff 1964, Beck and Maher 1986), and can generate positive net-present-value projects despite potential penalties and reputational costs (Karpoff et al. 2017). A number of factors will contribute to

heterogeneity in any treatment effect, including firms' risk of bribery and enforcement.

5. Data, Sample Selection, and Measures

5.1. Treatment and Control Firms

The UK uses a "common sense approach" to assess whether a non-UK firm is carrying on business in the UK and therefore subject to the UKBA's jurisdiction (Ministry of Justice 2012a, p.15). In conversations with the OECD Working Group on Bribery, the SFO explains that "carrying on business" in the UK "should be understood to be buying and selling in the UK" (OECD 2012, p.16). Official guidance indicates that jurisdiction based solely on a UK stock listing or the presence of a UK subsidiary is unlikely (Ministry of Justice 2012a, OECD 2012). Appendix A.3 provides statements from the UK regarding the UKBA's extraterritorial jurisdiction under section 7. These statements motivate the study's methodology in constructing treatment and control groups.

Given this approach, US firms with material revenues in the UK are plausibly regarded as carrying on business in the UK and thus subject to the UKBA's jurisdiction. Under Accounting Standards Codification (ASC) 280 *Segment Reporting*, US filers must separately disclose material revenues from customers located in an individual foreign country.[19,20] I therefore use geographic segment data to partition US sample firms into two groups: firms plausibly exposed to the UKBA (treatment firms), and firms not plausibly exposed to the UKBA (control firms).

[19] Paragraph 280-10-50-41 of ASC 280 states that public entities must report, unless it is impracticable to do so, "revenues from external customers attributed to the public entity's country of domicile and attributed to all foreign countries in total from which the public entity derives revenues. *If revenues from external customers attributed to an individual foreign country are material, those revenues shall be disclosed separately"* (emphasis added). The Financial Accounting Standards Board (FASB) states, "an item of segment information that, if omitted, would change a user's decision about that segment so significantly that it would change the user's decision about the enterprise as a whole is material even though an item of a similar magnitude might not be considered material if it were omitted from the consolidated financial statements" (FASB 1997, p. 27).

[20] Relying on pre-adoption UK segment disclosures is plausibly more in line with the common sense approach relative to alternative proxies, such as a UK listing or subsidiary, neither of which, according to the UK Ministry of Justice (2012a, p. 15-16), is a sufficient criterion in itself to designate a firm as carrying on business in the UK. Nevertheless, because segment disclosures are subject to materiality thresholds, this proxy is also subject to limitations. Namely, US firms that disclose (do not disclose) a UK segment might not be (might be) exposed to the UKBA. This scenario, however, should bias against finding a significantly treatment effect as it entails the inclusion of some unexposed (UKBA-exposed) firms in the treatment (control) group.

To construct the treatment group, I identify US firms that disclose a UK segment in at least one sample year during the three-year period prior to adoption. The control group consists of US firms that do *not* disclose a UK segment in *any* sample year. To assure comparability between treatment and control firms in having material pre-adoption exposure to at least one foreign country, I require control firms to disclose at least one non-US country-level segment in at least one pre-adoption year. An advantage of using segment data is that firms are required to disclose only country-level segments considered material, thus mitigating the potential for classifying firms with trivial (substantial) UK business activities as treatment (control) firms.[21]

5.2. Sample

Sample firms consist of public companies incorporated in the US to ensure that both treatment and control firms are subject to the FCPA and other US regulatory and economic shocks prior to UKBA adoption in 2010. The baseline sample period includes years 2007 through 2012 to allow three years before and after adoption. In additional tests, I perform regressions based upon an "extended sample," in which the post-adoption period encompasses an additional five years (Section 6.5 provides additional details).

I begin the sample selection process by manually cleaning and standardizing geographic segment names reported in the Compustat Segment data. This process assures that country-level segment names are properly mapped to the correct countries in Compustat; I resolve any discrepancies by manually coding the correct country per the firm's geographic segment name. This process also assures consistency across variations in abbreviations, spelling, or other aspects of a given country name (e.g., a firm-year-segment named "Ivory Coast" is treated the same as a firm-year-segment named "Côte d'Ivoire"). I exclude firm-years in which only regional segments are disclosed (e.g.,

[21] Utilizing disclosures involving materiality thresholds may raise concerns about selection bias. For instance, disclosures for sample treatment firms may be more transparent, and disclosure transparency could be correlated with a lower propensity to bribe. I alleviate this concern in several ways. First, I require that all sample firms disclose at least one non-US country-level segment in the pre-adoption period, thereby ensuring a baseline level of geographic reporting transparency. Second, I include the number of geographic segments disclosed as a conditioning variable in the entropy balancing procedure (see Section 6.1). Finally, regressions control for the number of geographic segments.

Latin America as a whole), thereby retaining all firm-years with at least one country-level segment. I also exclude firm-years in which total revenue from country-level segments is equal to zero.

After merging Compustat segment data with Compustat North America Fundamentals data, I construct treatment and control groups as described in Section 5.1. Firms that do not meet the criteria for inclusion in either the treatment or control group are not included in the sample. I exclude firms that do not have at least one observation in both the pre-adoption and post-adoption periods, as well as firm-years missing data necessary for control variables. This results in a baseline sample of 5,077 firm-years with 251 treatment firms and 694 control firms.[22]

5.3. Measures

A limitation of corruption research in general is that, for obvious reasons, firms do not voluntarily disclose their foreign bribe payments. However, because firms operating in corrupt countries are more likely to engage in corruption (e.g., Shleifer and Vishny 1993, D'Souza and Kaufmann 2013), the extent of firms' exposure to corrupt countries is a reasonable proxy for the extent of firms' corrupt activities. The net benefits of bribing are larger in corrupt countries with weak economic and political institutions relative to other countries; in some countries, doing business without some form of bribing is extremely difficult or, in certain cases, near impossible (e.g., Beck et al. 1991).

I construct a dependent variable that captures firms' exposure to (perceivably) corrupt countries by using Transparency International's Corruption Perceptions Index (CPI)—the index most often used in corruption studies (e.g., Cuervo-Cazurra 2008, Zeume 2017, Christensen et al. 2020).[23] For firm i in year t with segment revenue disclosed for J individual countries, the dependent variable CE_{it} (Corruption Exposure) is calculated as follows:

[22] The Equation 2 model (and variations thereof) is estimated after dropping 7 singleton groups, i.e., groups with one observation, resulting in 5,070 observations. Retaining singleton groups in regressions with multiple levels of fixed effects is computationally inefficient and overstates the statistical significance of the coefficient estimates. Further details can be found in Correia (2016).

[23] The CPI is a composite index which ranks 180 countries by their perceived levels of public corruption. CPI scores are based upon various surveys of country experts and business leaders conducted by international organizations, including the World Bank and Freedom House. Additional information is available here: https://www.transparency.org/research/cpi/cpi_2009/0.

$$CE_{it} = \sum_{j=1}^{J} \left((10 - CPI_j) \times \frac{Revenue_{ijt}}{\sum_{j=1}^{J} Revenue_{ijt}} \right) \quad (1)$$

where $Revenue_{ijt}$ is the revenue from country j disclosed by firm i in year t and CPI_j is Transparency International's Corruption Perceptions Index (CPI) score for country j in 2009, the year preceding adoption.[24] The CPI score is held fixed to ensure results are not driven by changes in the index. Because the CPI score ranges from 0 (highly corrupt) to 10 (uncorrupt), CE_{it} is increasing in corruption exposure. Controls for firm size ($Size_{it}$), profitability (ROA_{it}), and growth opportunities ($logMB_{it}$) are retrieved from the Compustat North America Fundamentals Annual database. Measures used in additional analyses are described in later sections; Appendix A.1 provides variable definitions and data sources.

6. Main Empirical Analysis

6.1. Research Design and Entropy Balancing Procedure

My main empirical tests are based upon the difference-in-differences (DiD) model in Equation 2, where β_1 represents the mean change in corruption exposure from before to after adoption of the UKBA for treatment firms relative to control firms. The DiD approach mitigates the potential for biases resulting from fundamental differences between treatment and control firms. It also mitigates the potential for biases resulting from time trends in treatment firm corruption exposure unrelated to adoption. The potential for any remaining confounding factors is further mitigated by weighting control firm observations using the entropy balancing procedure described shortly.

A negative and statistically significant estimate of β_1 suggests that, relative to control firms, adoption of the UKBA induced treatment firms to curb their mean exposure to corrupt countries.

$$CE_{it} = \beta_0 + \beta_1 Treat_i \times Post_t + \mathbf{\Pi X}_{it} + \tau_i + \varepsilon_{it} \quad (2)$$

[24] In robustness tests, the CPI score is held fixed as of 2007, as well as 2008 (see Section 9).

$Treat_i$ is an indicator variable equal to one if firm i is a treatment firm and equal to zero if firm i is a control firm (see Section 5.1). $Post_t$ is equal to one if year t is 2010 or later, and zero otherwise. A vector of controls for other potential factors influencing corruption exposure, represented by $\mathbf{X}_{it}$, includes the natural logarithm of assets ($Size_{it}$), profitability (ROA_{it}), growth opportunities ($logMB_{it}$), the number of geographic segments ($logSegCount_{it}$), industry×year fixed effects, and country-year fixed effects. Industry×year fixed effects capture common effects on corruption exposure in a particular year and industry. Country-year fixed effects, which control for common factors associated with material business in a particular country and year, are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise.[25] Firm fixed effects, denoted by τ_i, control for time-invariant firm factors. Standard errors are adjusted for heteroskedasticity and clustered by firm. A full list of variable definitions and data sources is provided in Appendix A.1.

I perform entropy-balanced DiD regressions in addition to unweighted DiD regressions because treatment and control firms exhibit dissimilar pre-adoption traits which could potentially influence changes in corruption exposure. Further, unweighted tests of relative trends in corruption exposure display a time trend, indicating the potential for confounding factors related to fundamental differences in treatment and control firms (see Section 6.2). Entropy balancing is a reweighting method that produces an improved counterfactual, resulting in estimated treatment effects that are less sensitive to sample composition and model choices (Hainmueller 2012). The entropy balancing procedure identifies continuous weights for the control sample to ensure nearly identical "covariate balance," i.e., the overlap in the pre-adoption distributions of relevant variables. Entropy balancing, which has been recently introduced to the accounting literature (e.g., Ferri, Zheng, and Zou 2018), offers a number of advantages over propensity score matching (McMullin and Schonberger 2020).[26]

[25] As an example, for Exxon Mobil Corp. with reported geographic segments for 13 individual countries in 2009, indicator variables in 2009 are equal to one for these 13 countries and equal to zero for all other countries.

[26] For example, unlike propensity score matching (PSM), entropy balancing preserves the size of the control sample and assures improved balance in the higher-order moments of the covariate distributions. Entropy balancing also involves less researcher discretion than PSM because PSM requires a first-stage predictive model of the treatment

Pre-adoption control firm observations are reweighted to match the distributional properties of pre-adoption treatment firm observations. Entropy weights are constructed at the firm level, i.e., the same weight is applied to all firm-years of a given control firm. Specifically, I reweight control firm observations to match the first and second moments of the mean pre-adoption values of $Size$, ROA, $logMB$, $logSegCount$, and the natural logarithm of 1 plus total foreign segment revenues ($logForeignRev$).[27] I also reweight control firm observations to match the mean treatment industries (according to the Fama and French 48-Industry Classification) and the pre-adoption annual values of CE.[28]

6.2. Descriptive Statistics, Parallel Trends, and Covariate Balance

Table 1 tabulates the industry frequency of treatment and control firms before entropy balancing. Business Services is the most represented industry in the treatment group, comprising 53 of 251 treatment firms, and the second-most represented industry in the control group, comprising 55 of 694 control firms. Control firms are most frequently in the Electronic Equipment industry (87 control firms). Differences in industry representation between treatment and control firms motivate the inclusion of industry as a conditioning variable in the entropy balancing procedure.

[INCLUDE TABLE 1 ABOUT HERE]

Mean corruption exposure for the sample before and after entropy balancing is tabulated by year in Table 2. Consistent with growth in emerging markets and the findings in Zeume (2017), mean CE generally increases over time. The univariate difference in mean unweighted CE between treatment and control firms is statistically significant at the 1% level in each of the pre-adoption years (untabulated). In contrast, the univariate difference in mean entropy-balanced CE is not

assignment. Because this first-stage model is typically unknown, researchers must often manually search for a model that improves balance. In contrast, the entropy balancing procedure requires only that researchers specify a tolerance level for convergence of the algorithm, where a smaller tolerance level indicates a higher required degree of covariate balance.

[27] $logForeignRev$ is a conditioning variable in the entropy balancing procedure but is not included as a control variable in the regressions because foreign revenue is endogenous, i.e., it is a potential outcome of the UKBA adoption.

[28] The mean value of CE over the pre-adoption period is used in cases where annual CE is missing.

statistically different from zero in any pre-adoption year, consistent with the entropy balancing procedure achieving an improved counterfactual.

[INCLUDE TABLE 2 ABOUT HERE]

Panels A and B of Table 3 present summary statistics before and after entropy balancing for the baseline sample of 5,077 firm-year observations. Mean CE in Panel B (2.69) is less than in Panel A (3.15) because the entropy balancing procedure weights control firm observations to match the distributional properties of the treatment firm observations. A smaller mean CE after entropy balancing is consistent with the idea that US firms with material business in the UK tend to be less exposed to corrupt countries (as compared to other US multinational firms with material business in at least one foreign country). Mean CE, both before and after entropy balancing, is on the low end of the CE range, suggesting that sample US firms are not especially predisposed to conduct material business in highly corrupt countries. Mean assets, ROA, and the number of geographic segments ($SegCount$) are greater after entropy balancing. Firms in the entropy-balanced sample have less growth opportunities on average compared to the sample before entropy balancing. For variables other than assets and $SegCount$, the standard deviation decreases after entropy balancing.

Correlation tables for the unweighted and entropy-balanced regression variables are provided in Panels C and D, respectively. The entropy-balanced correlations in Panel D demonstrate that control variables are positively correlated with CE. The correlations in Panels C and D generally differ in sign and magnitude because weighting the control group, by construction, produces relationships between variables reflective of those in the treatment group. Panel E presents a frequency table of segment disclosures and 2009 CPI scores by country. Unsurprisingly, sample firms disclose US segments most often, accounting for 4,816 of 16,800 total firm-year-segments. The UK is the fourth-most disclosed country-level segment (1,195 firm-year-segments).

[INCLUDE TABLE 3 ABOUT HERE]

The validity of DiD analyses relies on the parallel trends assumption, i.e., the assumption that, had the UKBA not been adopted, relative trends in corruption exposure would have been the same for treatment and control firms. Put differently, the change in corruption exposure from before to after the adoption in the control group is assumed to be a suitable proxy for the counterfactual change in corruption exposure of the treatment group had the adoption not occurred. The parallel trends assumption cannot be directly tested because the counterfactual is unobservable; however, comparing pre-adoption outcome trends for treatment firms versus control firms can provide indirect evidence that it holds. Results of tests of pre-adoption parallel trends for the baseline sample before entropy balancing, presented in Figure 2 and in Column (1) of Table 6, suggest common trends in the pre-adoption period.

However, though not statistically significant, the downward trend in relative pre-adoption corruption exposure is somewhat concerning. Using an entropy-balanced sample alleviates inherent biases influencing differences in post-adoption trends between treatment and control firms. Figure 3 and Column (2) of Table 6 present results of tests of pre-adoption parallel trends after entropy balancing. These results are comforting: not only do they suggest common trends in the pre-adoption period, but there is no evidence of monotonically decreasing relative trends in corruption exposure throughout the sample period. At the same time, it is assuring that results of tests using the unweighted and entropy-balanced baseline samples do not dramatically differ. Additional details regarding tests of parallel trends are discussed in Section 6.3.

To gain further understanding as to why entropy balancing should lead to more credible inferences despite satisfying tests of the parallel trends assumption for the unweighted sample, the remainder of this subsection examines pre-adoption differences in treatment and control firm factors before and after entropy balancing. Because pre-adoption parallel trends do not guarantee that parallel trends would have continued if the UKBA had not been adopted, it is important to understand how differences in pre-adoption variables between treatment and control firms affect the plausibility of a common counterfactual trend (Kahn-Lang and Lang 2019). DiD estimates can be confounded

if regressions include variables associated with the treatment assignment and the effect of such variables on the outcome varies over time. As an example, firms with material business in the UK tend to be larger and may therefore be better equipped than control firms to identify investment projects in corrupt countries. Such growth opportunities may increase over time, resulting in increased corruption exposure for treatment firms relative to control firms and, consequently, downward-biased DiD estimates. If treatment and control firms are dissimilar across variables that might affect corruption exposure, a reweighting technique can produce a counterfactual that more closely depicts randomized experimental conditions, thereby mitigating the potential for confounding factors.

For these reasons, evaluating the covariate balance and adjusting for concerning differences is an important step in establishing causal inferences (e.g., Imbens and Rubin 2015, Atanasov and Black 2016). Panel A of Table 4 presents the pre-adoption mean and variance of treatment and control group variables before entropy balancing. Compared to control firms, treatment firms are larger, have higher profitability and foreign revenues, and disclose more geographic segments. Treatment firms are also less exposed to corrupt countries, in part due to the UK's relatively high CPI score.

I assess the statistical significance of these differences by calculating normalized mean differences and performing t-tests of raw mean differences. Normalized differences are preferred in assessing covariate balance because they are unaffected by sample size (Imbens and Rubin 2015); prior research suggests that normalized differences between -0.1 and 0.1 indicate negligible differences (e.g., Austin 2011). Both normalized differences and t-tests indicate significant pre-adoption mean differences. Treatment and control firms also differ in the variance of pre-adoption variables (a variance ratio between 0.80 and 1.25 is ideal according to the criteria in Rubin 2001). Such differences can confound DiD estimates.[29] Collectively, Panel A suggests the potential presence of confounding factors in unweighted regression analyses.

[29] For example, increased global anti-corruption awareness over time (induced partially by the UKBA) may affect control firms to a greater extent than treatment firms because control firms are more exposed to corrupt countries to begin with. This scenario would result in larger observed post-adoption declines in corruption exposure for control firms compared to treatment firms, leading to downward-biased DiD estimates of the UKBA treatment effect.

Panel B of Table 4 presents covariate balance after entropy balancing. Applying the entropy balancing procedure substantially improves similarity in the pre-adoption means of treatment and control firm variables. Mean differences using t-tests are not statistically different from zero and normalized differences are all within acceptable thresholds. The average normalized difference across all variables is merely 0.01. Variance ratios are close to one for all variables other than profitability, though the actual difference is smaller than in the unweighted sample.[30]

[INCLUDE TABLE 4 ABOUT HERE]

Overall, preprocessing the data using entropy balancing makes treatment and control firms substantially more similar across variables that could potentially affect inferences; identifying assumptions of the DiD analyses are thus more likely to be satisfied after entropy balancing.

6.3. Main Regressions: Impact of the UKBA on US Firms' Business in Corrupt Countries

Table 5 presents results of unweighted and entropy-balanced regressions estimating the treatment effect of UKBA adoption for the baseline sample. Columns (1) and (2) present results of estimating Equation 2 with year fixed effects rather than industry$\times$year fixed effects. Columns (3) and (4) present results of estimating Equation 2.

Estimated treatment effects before entropy balancing in Columns (1) and (3) are negative and statistically significant at the 1% level, suggesting that the UKBA induces treatment firms to curb mean corruption exposure relative to control firms. After performing the entropy balancing procedure to achieve a more comparable control sample, estimated coefficients in Columns (2) and (4) remain negative and statistically significant at the 5% and 1% level, respectively. The estimated effect across all specifications is similar in magnitude. For context, a decline of 0.1 in the 2009 CPI score equates to a difference in perceived corruption between, for example, Hungary (CPI=5.1) and Jordan (CPI=5.0). The estimated treatment effect of -0.0694 in Column (4) amounts to an approximate 0.07 point decrease in relative CPI score of the treatment group, or 19% of the interquartile

[30] Pre-adoption covariate values are (nearly) perfectly balanced at the firm level. Entropy balancing does not produce differences of exactly zero in Panel B because weights are determined at the firm level rather than the firm-year level.

range in the pre-adoption sample distribution of corruption exposure. These results support the hypothesis that adoption of the UKBA curbs the mean corruption exposure of US firms that conduct material business in the UK relative to similar US firms that do not conduct material business in the UK.

[INCLUDE TABLE 5 ABOUT HERE]

Returning to tests of parallel pre-adoption trends, I estimate the model in Equation 2 after replacing $Post$ with indicators for each sample year and present the results in Table 6. The interaction between $Treat$ and 2009, the year prior to adoption, serves as the benchmark. Estimated coefficients on $Treat \times 2007$ and $Treat \times 2008$ are not statistically different from zero and a joint F test fails to reject the null hypothesis that the pre-adoption coefficient estimates jointly equal zero. The findings of this analysis suggest that the parallel trends assumption is satisfied.

Estimated coefficients in the unweighted regressions are statistically significant only in 2012, suggesting that US firms only respond to the UKBA after it becomes enforceable in 2011. Estimated coefficients in the entropy-balanced regressions of -0.0531, -0.0496, and -0.0579 for the years 2010, 2011, and 2012 are statistically significant at the 10%, 5%, and 10% levels, respectively. In the entropy-balanced regression, the estimated treatment effect does not become more pronounced after the UKBA became enforceable, suggesting that US firms anticipate enforcement upon adoption (consistent with the discussion in Section 3). An untabulated test fails to reject the null hypothesis that the three post-adoption coefficient estimates are equal, suggesting that, on average, treatment firms do not fully react to the UKBA immediately, perhaps because changes in internal procedures and operations take time to implement. Figures 2 and 3 plot coefficients from the unweighted and entropy-balanced regressions with 90% confidence intervals. Additional details regarding tests of parallel trends are discussed in Section 6.2.

[INCLUDE TABLE 6, FIGURE 2, AND FIGURE 3 ABOUT HERE]

6.4. Placebo Regressions: "Treatment" Assigned to Germany-Exposed Firms

As a falsification test, I replicate the main analyses after constructing treatment and control groups as though US firms that carry on (do not carry on) material business in *Germany* are treatment (control) firms.[31] I focus on Germany because Germany and the UK are similar in several key respects, but carrying on business in Germany does not expose US firms to the UKBA. For one, Germany and the UK are both large, developed economies and trading nations in Western Europe. US firms also conduct substantial business in Germany; after the UK, Germany is the second-most frequent European country-level segment disclosed by US firms (see Table 3, Panel E). Germany, like the UK, has high law enforcement quality (La Porta et al. 1998) and relatively low levels of perceived corruption (Transparency International 2009). Both countries ratified multilateral anti-corruption agreements, such as the Anti-Bribery Convention, and thus have similar levels of cooperation with US authorities in anti-corruption enforcement. Consistent with the preceding results being attributable to adoption of the UKBA, the placebo regressions do not yield significant results (see Table 7).[32]

[INCLUDE TABLE 7 ABOUT HERE]

6.5. Main Regressions Using Extended Sample Period

As my baseline sample spans just three post-adoption years, and investigations and prosecutions take time to carry out (especially in the case of a brand new law), my results suggest that, at the time of adoption, US firms anticipated future enforcement of the UKBA (refer to Section 3). However, the realization of enforcement actions should further influence foreign firms to curb corruption (e.g., Christensen et al. 2020). Because the majority of UKBA enforcement actions occur in years after 2012 (see Figure 1), I estimate the main specifications after expanding the post-adoption

[31] Sample selection procedures for the falsification test are identical to those described in Section 5.2. That is, treatment firms are US firms that disclose a segment for Germany in at least one pre-adoption year, and control firms are US firms that do not disclose a segment for Germany in any sample year, but disclose at least one non-US country-level segment in at least one pre-adoption year.

[32] A positive correlation between disclosing a German segment and disclosing a UK segment biases the falsification test in favor of a significant treatment effect.

period to include five additional years (i.e., through 2017). For this extended sample, the methodology used to construct treatment and control groups and sample selection procedures are the same as those described for the baseline sample. Descriptive statistics for the extended sample are provided in Tables S.1.1 through S.1.4 of the Supplementary Appendix.

Regression results are tabulated in Table 8. The estimated coefficients of interest (statistically significant at the 1% level) are greater in magnitude than those in the Table 5 baseline regressions. These results are therefore consistent with heightened enforcement leading to greater incentives for US firms to curb bribery, although part of the effect in later years could occur because some anti-corruption compliance measures take time to implement. The evidence presented in Table 9 suggests parallel relative trends in pre-adoption corruption exposure. As with the baseline sample, the unweighted regressions display a downward trend in relative pre-adoption corruption exposure (Figure 4), suggesting that entropy balancing provides for more credible inferences (Figure 5).

Event-time plots of the Table 9 coefficient estimates, presented in Figures 4 and 5, are also consistent with greater treatment effects in later post-adoption years. Focusing on the coefficient estimates in Figure 5, I perform a series of F tests of the null hypothesis that the coefficient estimate for a given year is equal to the coefficient estimate for the preceding year. These tests allow me to identify any years with statistically significant annual declines in corruption exposure for treatment firms relative to control firms. Results of these tests (untabulated) indicate a statistically significant effect for the years 2013 ($F = 3.34$, p-value $= 0.0679$), 2015 ($F = 3.39$, p-value $= 0.0657$), and 2017 ($F = 3.01$, p-value $= 0.0832$). Results for other years fail to reject the null hypothesis that the treatment effect is equal to the treatment effect in the prior year.

[INCLUDE TABLE 9, FIGURE 4, AND FIGURE 5 ABOUT HERE]

The remainder of this subsection discusses potential reasons for the strengthened estimated treatment effect observed in years 2013, 2015, and 2017 (relative to the respective preceding year). This discussion is merely suggestive; one should exercise caution in assigning any particular ra-

tionale to year-over-year variation, as several factors could influence these changes. For instance, firms may adjust their behavior in response to investigation announcements, which do not always culminate in enforcement actions and typically occur well before enforcement actions are issued. In addition, it may take time for firms to implement new procedures and adjust operations in response to signals of heightened enforcement. A one-year lag is assumed in the following suggestions because, ex ante, one year appears to be a reasonable amount of time for firms to respond to enforcement signals. This process, however, is ultimately unobservable.

The strengthened estimated treatment effect in 2013 can potentially be attributed to the appointment of a new SFO director in April 2012, who issued a series of statements emphasizing the SFO's tough stance on bribery (e.g., see Binham 2012, Pinsent Masons 2012). The strengthened estimated treatment effect in 2015 could have resulted from 2014 UK legislation introducing deferred prosecution agreements (DPAs). DPAs are agreements, made at the prosecutor's discretion, which suspend prosecution of a firm in exchange for the fulfillment of a number of conditions, such as paying financial penalties, making improvements to compliance programs, and compensating victims. DPAs, used in the FCPA context since 2004 (Christensen et al. 2020), are powerful tools in anti-corruption enforcement, allowing regulators to overcome hurdles that hinder prosecution (e.g., difficulty obtaining evidence). According to the OECD Working Group on Bribery, the introduction of DPAs, in combination with adoption of the UK Bribery Act, "have given the SFO greater legal powers than ever before to deal with corporate offending" (OECD 2017, p. 50). Finally, the strengthened estimated treatment effect in 2017 may be related to the uptick in UKBA enforcement actions in 2016 (see Figure 1).

7. US Firms' Business in the United Kingdom

7.1. Impact of the UKBA on US Firms' Business in the United Kingdom

The results thus far suggest that, on average, US firms curb their mean exposure to corrupt countries following adoption of the UKBA. Certain firms, however, might be unable to justify the

costs associated with carrying on substantial business in the UK in the post-adoption period. To explore whether UKBA adoption led some US firms to curb their business in the UK, I first provide descriptive evidence of pre-adoption characteristics conditional on whether treatment firms continue to disclose a UK segment in the post-adoption period (recall that all treatment firms disclose a UK segment in the pre-adoption period). In Panel A of Table 10, I tabulate mean pre-adoption variables after splitting treatment firms into two groups: (1) 40 treatment firms that no longer disclose a UK segment in any post-adoption year; and (2) 211 treatment firms that continue to disclose a UK segment in at least one post-adoption year.

Treatment firms that discontinue UK segment disclosure have greater pre-adoption corruption exposure compared to other treatment firms; the difference of 0.25 is significant at the 1% level. Treatment firms that discontinue UK segment disclosure also disclose significantly fewer pre-adoption geographic segments, suggesting lower reporting transparency. The magnitude of firms' UK revenues is represented by $logUKRev_{it}$, i.e., the natural logarithm of 1 plus revenues from a UK segment. Consistent with lower costs impacting firms' decision to curb business in the UK, pre-adoption UK revenues are significantly smaller for treatment firms that discontinue UK segment disclosure relative to other treatment firms. Further, treatment firms that discontinue UK segment disclosure are less profitable and smaller than other treatment firms in the pre-adoption period; such firms may face greater pressure with respect to obtaining or retaining positive net-present-value projects.

[INCLUDE TABLE 10 ABOUT HERE]

Next, I examine the likelihood that treatment firms discontinue UK segment disclosure. An uncharacteristically large number of treatment firms discontinuing UK segment disclosure suggests that adoption of the UKBA leads some firms to curb their business in the UK. In order to credibly evaluate treatment firms' propensity to discontinue UK segment disclosure, it is first necessary to establish a benchmark control group. In keeping with the logic of the falsification test described in

Section 6.4, I construct a control group of US firms that disclose a pre-adoption German segment. I base my analysis upon the rationale that, other than the additional costs of foreign bribery brought on by the UKBA, there is no obvious reason for US firms with material UK (German) business to be more (less) likely to discontinue material UK (German) business following adoption. I therefore investigate the likelihood that US firms with a pre-adoption UK segment discontinue UK segment disclosure *relative to* the likelihood that US firms with a pre-adoption German segment discontinue German segment disclosure. I utilize the following cross-sectional model:

$$DropSeg_i = \alpha_0 + \alpha_1 UKTreat_i + \epsilon_i \tag{3}$$

where $UKTreat_i$ is equal to one if firm i discloses a segment for the UK, but not for Germany, in at least one sample year prior to 2010, and zero if firm i discloses a segment for Germany, but not for the UK, in at least one sample year prior to 2010. When $UKTreat_i$ is equal to one (zero), $DropSeg_i$ is equal to one if firm i does not disclose a segment for the UK (Germany) in any post-adoption year, and zero otherwise.

Columns (1), (2), and (3) of Table 10, Panel B present results of estimating Equation 3 using a linear probability model (LPM), a probit model, and a logistic model, respectively. Coefficient estimates on $UKTreat_i$ are positive and statistically significant. The (untabulated) odds ratio from the logistic regression indicates that the odds that treatment firms discontinue UK segment disclosure are 1.68 times the odds that control firms discontinue German segment disclosure. The probability that firms with a pre-adoption UK segment discontinue UK segment disclosure is therefore $\approx$ 63% higher [1.68/(1 + 1.68)]. These results suggest that some treatment firms curb post-adoption business in the UK.

[INCLUDE TABLE 10 ABOUT HERE]

To gain further insight as to whether adoption leads some treatment firms to curb business in the UK, I estimate the cross-sectional multivariate model in Equation 4 to examine determinants of treatment firms' decision to discontinue UK segment disclosure. For this analysis, the sample

consists of firm-level pre-adoption observations for treatment firms in the baseline sample.[33]

$$NoUKSeg\_post_i = \mu_0 + \mu_1 CE\_pre_i + \mu_2 logUKRev\_pre_i + \mu_3 logMB\_pre_i + \mu_4 ROA\_pre_i + \mu_5 Size\_pre_i + \mu_6 logSegCount\_pre_i + \epsilon_i \quad (4)$$

$NoUKSeg\_post_i$ is equal to one if treatment firm i does not disclose a post-adoption UK segment, and zero otherwise. CE_pre_i, $logUKRev\_pre_i$, $logMB\_pre_i$, ROA_pre_i, $Size\_pre_i$, and $logSegCount\_pre_i$ are the pre-adoption firm-level means of CE_{it}, $logUKRev_{it}$, $logMB_{it}$, ROA_{it}, $Size_{it}$, and $logSegCount_{it}$.

Results of LPM, probit, and logistic regressions are presented in Columns (4), (5), and (6) of Table 10, Panel B. Consistent with the descriptive evidence in Panel A, coefficient estimates on CE_pre_i are statistically significant, suggesting that, following adoption, treatment firms with relatively high pre-adoption corruption exposure are more likely to curb their business in the UK. Treatment firms exhibiting relatively lower pre-adoption reporting transparency (by way of having fewer geographic segments) are also more likely to discontinue UK segment disclosure. Coefficient estimates on $logUKRev\_pre_i$ are negative and statistically significant; consistent with the idea that firms with relatively low pre-adoption revenues in the UK face lower costs of curbing business in the UK. On the other hand, relatively low pre-adoption UK revenues may have a greater chance of becoming immaterial for segment reporting thresholds in the post-adoption period.

The results in Table 10 collectively suggest that, for a select group of firms with relatively high pre-adoption exposure to corrupt countries, the costs of adoption exceed the benefits of con-

[33] Of the 251 treatment firms in the baseline sample, 3 firms are missing UK revenue data, resulting in 248 treatment firms.

tinuing material business in the UK.

8. Additional Regressions

8.1. US Firms' Incremental Exposure to the UKBA

This subsection examines the effect of UKBA adoption conditional on US firms' material business in countries where facilitating payments are perceived to be most common. Facilitating, or grease, payments are (typically, but not necessarily, small) payments made to secure or expedite routine governmental services, such as acquiring licenses, work permits, or visas. Unlike the FCPA, the UKBA does not provide an exception for facilitating payments. It is ex ante unclear, however, whether this statutory difference has practical significance for US firms. Despite the FCPA exception, some US firms may have adopted a conservative approach prior to the UKBA by implementing policies that prohibit facilitating payments (Warin et al. 2010).

The World Bank conducts Corruption Enterprise Surveys of firms in various countries (World Bank Group 2019). For each surveyed country that overlaps with the country-level segments in my sample, I rank the average response for six data items related to facilitating payments (see Appendix A.4 for additional details). I then construct an indicator variable, $HighFPRisk_i$, which is equal to one for firms disclosing pre-adoption segment revenues from at least one country for which the average response to these data items is ranked in the top tercile (hereafter, "High FP Countries"), and zero otherwise. $HighFPRisk_i$ is equal to one for 66 distinct firms in the baseline sample.

Results of estimating Equation 2 after including $Treat \times Post \times HighFPRisk$ for the baseline and extended samples are presented in Column (1) of Tables 11 and 12, respectively.[34] The estimated coefficient of interest is negative for both samples, but is only statistically significant in the extended sample regression (at the 5% level). The $Treat \times Post \times HighFPRisk$ coefficient estimate for the extended sample (-0.1836) is substantially larger in magnitude than for the baseline sample (-0.0190), suggesting that, in the early years of the UKBA, US firms perceived lower

[34] Other main and interaction variables are collinear with firm fixed effects. Details are provided in the notes to Tables 11 and 12.

incremental risk related to facilitating payments relative to the later years.

[INCLUDE TABLES 11 AND 12 ABOUT HERE]

Statements by the SFO in 2010 indicate that small and one-off facilitating payments are unlikely to result in prosecution (Warin et al. 2010). I therefore examine heterogeneity in the treatment effect based upon the proportion of pre-adoption revenues derived from High FP Countries. I create another indicator variable, $HighFPRiskRev_i$, which is equal to one if, for firm i, $HighFPRisk_i$ is equal to one, and, among firms for which $HighFPRisk_i$ is equal to one, firm i has an above-median pre-adoption proportion of revenue from High FP Countries (i.e., segment revenue from High FP Countries scaled by total revenue), and zero otherwise. Results of estimating Equation 2 for the baseline and extended samples after including $Treat \times Post \times HighFPRiskRev$ are reported in Column (2) of Tables 11 and 12, respectively. For the baseline (extended) sample, the estimated coefficient on $Treat \times Post \times HighFPRiskRev$ is -0.4345 (-0.3217) and is statistically significant at the 1% (5%) level. For the baseline (extended) sample, the estimated treatment effect for firms that derive a high proportion of revenues from High FP Countries is approximately 8.94 (4.32) times stronger than for other firms.[35] These results collectively provide supporting evidence that adoption of the UKBA drives the observed treatment effect.

8.2. The Effect of the UKBA Conditional on Bribery Risk

Next, I perform regressions conditional on firms' risk of bribery. Specifically, I examine whether the treatment effect differs for firms that conduct business in countries associated with a relatively high risk of bribery. I construct an indicator variable, $HighBribeRisk_i$, which is equal to one if, in the pre-adoption period, a firm discloses a segment for at least one country ranked in the top five countries perceived to be sources of foreign bribery according to Transparency International's

[35] For the baseline sample, [(-0.4345 + -0.0547)/-0.0547] = 8.94. For the extended sample, [(-0.3217 + -0.0968)/-0.0968] = 4.32.

2008 Bribe Payers Index (Transparency International 2008), and zero otherwise.[36] I then estimate Equation 2 after including $Treat \times Post \times HighBribeRisk$ and $Post \times HighBribeRisk$.[37]

[INCLUDE TABLES 13 AND 14 ABOUT HERE]

Regression results for the baseline and extended samples are presented in Column (1) of Tables 13 and 14, respectively. For the baseline (extended) sample, the estimated coefficient of -0.1018 (-0.1170) on $Treat \times Post \times HighBribeRisk$ is statistically significant at the 10% level, suggesting a more pronounced treatment effect for firms carrying on pre-adoption business in high-risk countries. These findings demonstrate that the treatment effect varies predictably with firms' pre-adoption bribery risk, lending further support to the hypothesis that adoption of the UKBA leads treatment firms to curb their exposure to corrupt countries (relative to control firms).

The $Post \times HighBribeRisk$ coefficient estimate is 0.1327 for the baseline sample and 0.1432 for the extended sample. The $Post \times HighBribeRisk$ coefficient estimate captures the mean change in corruption exposure from before to after adoption for control firms exposed to countries with a high risk of bribery relative to other control firms. For both samples, the positive and statistically significant $Post \times HighBribeRisk$ coefficient estimate suggests a substitution effect: by inducing treatment firms to curb corruption exposure, adoption of the UKBA may introduce new investment opportunities for control firms operating in countries with a high risk of bribery, allowing these firms to increase their corruption exposure.

8.3. The Effect of the UKBA Conditional on Enforcement Risk

A greater likelihood of enforcement by UK regulators encourages greater compliance by increasing the potential costs of UKBA violations. US firms with substantial business in the UK

[36] The top five countries perceived to be sources of foreign bribery are, in order, Russia, China, Mexico, India, and Italy. The 2008 Bribe Payers Index "ranks 22 of the most economically influential countries according to the likelihood of their firms to bribe abroad." (Transparency International 2008, p. 2). Cases involving firms with material business in a country not included in the Bribe Payers Index, where said country has a greater risk of foreign bribery than the top five Bribe Payers Index countries, biases against finding a significant treatment effect.

[37] Other main and interaction variables are collinear with other variables. Details are provided in the notes to Tables 13 and 14.

may expect a higher chance of enforcement; I therefore condition analyses on the magnitude of firms' UK revenue. Specifically, I estimate Equation 2 after including $Treat \times Post \times logUKRev$ and $Treat \times logUKRev$.

Results for the baseline and extended samples are presented in Column (2) of Tables 13 and 14, respectively. The $Treat \times Post \times logUKRev$ coefficient estimate for the baseline (extended) sample is -0.0111 (-0.0156) and is statistically significant at the 10% (5%) level, suggesting that firms expecting a greater likelihood of enforcement curb post-adoption mean corruption exposure to a greater extent. Column (3) of Tables 13 and 14 presents Equation 2 results after including variables from the Columns (1) and (2) specifications. The incremental treatment effect remains statistically significant for both high risk proxies, indicating that these risks have distinct effects.

9. Robustness Tests

I perform a variety of robustness regressions for both the baseline and extended samples. First, I estimate the main specifications after holding the CPI score fixed as of 2008 and 2007 rather than 2009 (see Supplementary Appendix Tables S.2.1 through S.2.4). Second, to ensure results are not driven solely by firms in the Business Industry, which is the most (second-most) represented industry in the treatment (control) group, I estimate the main specifications after excluding firms in this industry. Results, presented in Tables S.3.1 and S.3.2, are robust to this modification.

Finally, firms that do not disclose a pre-adoption UK segment but that disclose a post-adoption UK segment are not included in my baseline nor extended samples because I require that control firms do not disclose a UK segment in any sample year. To ensure results are not driven by this ex post classification, I estimate the main regressions for both samples after defining the control group as US firms that disclose at least one non-US country-level segment in the pre-adoption period and do not disclose a UK segment in the *pre-adoption period* (rather than in the full sample

period). This classification does not affect inferences (see Tables S.4.1 and S.4.2).

10. Conclusion

To the best of my knowledge, this study is the first to provide empirical evidence on extraterritorial implications of anti-corruption laws on US firms. I provide plausibly causal evidence suggesting that adoption of the UKBA in 2010 curbs the mean corruption exposure of US firms with exposure to its jurisdiction, relative to similar US firms with little to no exposure to its jurisdiction. Further, this study's setting and design mitigate empirical identification issues inherent in cross-country and single-country domestic studies examining the effect of anti-corruption laws generally. This approach thus demonstrates how extraterritorial legal provisions, which are becoming increasingly popular in addressing cross-border issues, can potentially be exploited in future studies examining the causal impact of regulations.

Careful design, attention to institutional details, additional analyses, and robustness tests help mitigate the possibility that unobserved firm heterogeneity or confounding shocks contaminate my findings. Despite these efforts, the observational nature of this study does not permit me to completely rule out the potential for confounding variables. I use segment disclosure data because it provides information on business in various countries at the firm level, and because opportunities for treatment firms to manipulate segment disclosures may be limited given that segment disclosures are audited. Nevertheless, a limitation of this study stems from the possibility that treatment firms systematically adjust their segment disclosure in the post-adoption period in order to conceal material revenues in corrupt countries, or in the UK.

Multinational firms based in developed countries play an important economic role in foreign, developing countries (The Brookings Institution 2015). As corruption causes inefficient contracting and has a variety of other negative economic and social implications (e.g., Rose-Ackerman 1996), the findings of this study suggest one potential benefit of extraterritoriality in anti-corruption laws: the curbing of corruption in foreign multinational firms subject to the jurisdiction of such laws. The

participation of countries other than the US in strict, extraterritorial anti-corruption enforcement may help alleviate the collective action problem, thereby reducing corruption overall (e.g., Schuman 2011, Brewster and Buell 2017). This study, however, does not speak to the overall net benefit of extraterritoriality in anti-corruption laws.

It should be kept in mind that bribing behavior is influenced by the institutional environment of the country where a multinational firm is based (D'Souza 2012, D'Souza and Kaufmann 2013). If US firms subject to the FCPA engage in low corruption relative to other countries, the observed treatment effect may represent the lower bound of the effect of adopting extraterritorial anti-corruption laws on foreign multinational firms. On the other hand, the treatment effect may be weaker outside of the US if, for example, firms in countries with weak institutions rely more heavily on bribe payments to compete or are unaccustomed to anti-corruption compliance systems. Future research exploring the interplay between the country of adoption and the home country of affected firms may yield further insight into the role of institutions and country-specific factors in determining the effect of foreign anti-corruption laws on foreign bribery. In addition, the treatment and control firms in my sample have relatively 'clean' corruption exposure scores. To the extent firms less exposed to corrupt countries have less opportunity to curb bribe payments, these estimates may understate the effect of foreign anti-corruption laws with extraterritorial jurisdiction. Alternatively, such firms may be most willing to make changes to international operations; for example, if they do not depend on bribe payments to compete.

Appendices

A.1. Variable Definitions and Data Sources

Variable	Description	Source
CE_{it}	For firm i in year t with segment revenue disclosed for J individual countries, the revenue-weighted corruption exposure score, CE, which is measured at the firm-year level, is calculated as: $$CE_{it} = \sum_{j=1}^{J} \left((10 - CPI_j) \times \frac{Revenue_{ijt}}{\sum_{j=1}^{J} Revenue_{ijt}} \right)$$ where $Revenue_{ijt}$ is the Compustat segment revenue from country j disclosed by firm i in year t and CPI_j is Transparency International's CPI score for country j in 2009.	Transparency International's Corruption Perceptions Index (CPI); Compustat Segment Data
$Treat_i$	An indicator variable equal to one if firm i discloses a country-level geographic segment for the UK in at least one sample year prior to 2010, and zero otherwise.	Compustat Segment Data
$Post_t$	An indicator variable equal to one if year t is 2010 or later, and zero otherwise.	Compustat Segment Data
$Size_{it}$	The natural logarithm of the total assets (AT) for firm i in year t.	Compustat North America
$logMB_{it}$	The natural logarithm of the market-to-book ratio for firm i in year t. The market-to-book ratio is calculated as total assets (AT) + market value of equity (PRCC_F $\times$CSHO) - common equity (CEQ) - deferred taxes (TXDB), scaled by total assets (AT).	Compustat North America
ROA_{it}	The net income (NI) divided by total assets (AT) for firm i in year t.	Compustat North America
$logSegCount_{it}$	The natural logarithm of the total geographic segments disclosed by firm i in year t.	Compustat Segment Data

Variable	Description	Source
$logForeignRev_{it}$	The natural logarithm of one plus the total foreign geographic segment revenues disclosed by firm i in year t.	Compustat Segment Data
$logUKRev_{it}$	The natural logarithm of one plus the UK geographic segment revenues disclosed by firm i in year t.	Compustat Segment Data
$UKTreat_i$	An indicator variable equal to one if firm i separately discloses a segment for the UK, but not for Germany, in at least one sample year prior to 2010, and zero if firm i separately discloses a segment for Germany, but not the UK, in at least one sample year prior to 2010.	Compustat Segment Data
$DropSeg_i$	When $UKTreat_i$ is equal to one, $DropSeg_i$ is equal to one if firm i does not disclose a UK segment in any post-adoption year, and zero otherwise. When $UKTreat_i$ is equal to zero, $DropSeg_i$ is equal to one if firm i does not disclose a segment for Germany in any post-adoption year, and zero otherwise.	Compustat Segment Data
$HighBribeRisk_i$	An indicator variable equal to one if, in the pre-adoption period, firm i discloses a segment for at least one country ranked in the top five countries perceived to be sources of foreign bribery according to Transparency International's 2008 Bribe Payers Index, and zero otherwise. Based on a survey of senior business executives, the Bribe Payers Index captures the likelihood of foreign firms from countries in which respondents do business to engage in bribery when doing business in the respondents' country. The top five countries perceived to be sources of foreign bribery are, in order, Russia, China, Mexico, India, and Italy.	Transparency International's 2008 Bribe Payers Index
$HighFPRisk_i$	An indicator variable equal to one if firm i discloses pre-adoption segment revenue from at least one country for which facilitating payments are most common, i.e., at least one country for which the average response to six World Bank Enterprise Corruption Survey data items relating to facilitating payments is ranked in the top tercile among countries represented in the sample, and zero otherwise (see Appendix A.4 for additional details). World Bank data is retrieved from https://www.enterprisesurveys.org/data/exploretopics/corruption#--7.	World Bank Corruption Enterprise Survey; Compustat Segment Data

Variable	Description	Source
$HighFPRiskRev_i$	An indicator variable equal to one if $HighFPRisk_i = 1$ for firm i *and,* among firms for which $HighFPRisk_i = 1$, firm i has an above-median pre-adoption proportion of revenue from countries for which facilitating payments are most common, i.e., segment revenue from countries where facilitating payments are most common divided by total revenue, and zero otherwise.	World Bank Corruption Enterprise Survey; Compustat Segment Data

A.2. Examples of US Firm 10-K Disclosure Involving the UKBA

Equifax Inc. Form 10-K Item 1A: Risk Factors (fiscal year-end 12/31/2010)

Economic, political and other risks associated with international sales and operations could adversely affect our results of operations.

[...] In many foreign countries, particularly those with developing economies, it is common to engage in business practices that are prohibited by laws and regulations applicable to us, such as the Foreign Corrupt Practices Act and the newly passed but not yet implemented U.K. Bribery Act. Although we implement policies and procedures designed to facilitate compliance with these laws, our employees, contractors and agents, as well as those companies to which we outsource certain of our business operations, may take actions in violation of our policies. Any such violation, even if prohibited by our policies, could have a material adverse effect on our business and reputation.

CDI Corp. Form 10-K Item 1A: Risk Factors (fiscal year-end 12/31/2010)

CDI operates in many different jurisdictions and could be materially and adversely affected by violations of the US Foreign Corrupt Practices Act, the UK Bribery Act and similar worldwide anti-corruption laws.

The US Foreign Corrupt Practices Act ("FCPA") and similar worldwide anti-corruption laws, including the UK Bribery Act of 2010, generally prohibit companies and their intermediaries from making improper payments to government officials and others for the purpose of obtaining or retaining business. CDI's internal policies mandate compliance with these anti-corruption laws. We operate in parts of the world that have experienced governmental corruption to some degree, and in certain circumstances, strict compliance with anti-corruption laws may conflict with local customs and practices. Despite our training and compliance programs, there can be no assurance that our internal control policies and procedures will protect us from reckless or criminal acts committed by those of our employees or agents who violate our policies. Our continued expansion outside the US, including in developing countries, could increase the risk of such violations in the future. Violations of these laws, or allegations of such violations, could disrupt our business and result in a material adverse effect on our results of operations or financial condition.

Vonage Holdings Corp. Form 10-K Item 1A: Risk Factors (fiscal year-end 12/31/2011)

We may be exposed to liabilities under the Foreign Corrupt Practices Act, the UK Bribery Act, and similar laws, and any determination that we violated any of these laws could have a material adverse effect on our business.

We are subject to the Foreign Corrupt Practice Act ("FCPA"), the UK Bribery Act and other laws that prohibit improper payments or offers of payments to foreign governments and their officials and political parties by persons and entities for the purpose of obtaining or retaining business. We have operations, agreements with third parties, and make sales internationally. Our international activities create the risk

of unauthorized payments or offers of payments by one of our employees, consultants, sales agents or distributors, even though these parties are not always subject to our control. It is our policy to implement safeguards to discourage these practices by our employees, consultants, sales agents or distributors However, our existing safeguards and any future improvements may prove to be less than effective, and our employees, consultants, sales agents or distributors may engage in conduct for which we might be held responsible. Violations of the FCPA, the UK Bribery Act or other laws may result in severe criminal or civil sanctions, and we may be subject to other liabilities, which could negatively affect our business, operating results, and financial condition.

Marsh & McLennan Companies, Inc. Form 10-K Item 1A: Risk Factors (fiscal year-end 12/31/2011)

Our compliance systems and controls cannot guarantee that we are in compliance with all potentially applicable U.S. federal and state or foreign laws and regulations, and actions by regulatory authorities or changes in legislation and regulation in the jurisdictions in which we operate may have an adverse effect on our business.

[...] Compliance with foreign and U.S. laws and regulations that are applicable to our operations is complex and may increase our cost of doing business in international jurisdictions. These laws and regulations include import and export requirements, anti-corruption laws such as the U.S. Foreign Corrupt Practices Act and the UK Bribery Act 2010, local laws prohibiting corrupt payments to governmental officials, as well as various trade sanctions laws such as the various international legislative and regulatory requirements relating to trade with Iran.

Jones Lang Lasalle Inc. Form 10-K Item 1A: Risk Factors (fiscal year-end 12/31/2011)

Burden of complying with multiple and potentially conflicting laws and regulation and dealing with changes in legal and regulatory requirements

[...] Additionally, changes in legal and regulatory requirements can impact our ability to engage in business in certain jurisdictions or increase the cost of doing so. The legal requirements of U.S. statutes may also conflict with local legal requirements in a particular country, as, for example, when anonymous hotlines required under U.S. law were construed to conflict in part with French privacy laws. The jurisdictional reach of laws may be unclear as well, as when laws in one country purport to regulate the behavior of affiliated corporations within our group that are operating in other countries. There is some uncertainty, for example, in the jurisdictional reach of the new U.K. Bribery Act, and the standards for illegal activity in that Act are in some ways higher than those established under the US Foreign Corrupt Practices Act.

PMFG, Inc. Form 10-K Item 1A: Risk Factors (fiscal year-end 6/30/2012)

We are subject to United States and foreign laws and regulations including export control and economic sanctions laws and regulations. These regulations are complex, change frequently and have tended to become more stringent over time. Implementing compliance with the requirements of any new or amended U.S. or foreign laws and regulations as well as failure to comply with any laws and regulations could adversely affect our results of operations, financial condition and our strategic objectives.

As a result of our global operations, we face a variety of special United States and international legal and compliance risks, in addition to the risks of our domestic business. These federal, state and local laws, regulations and policies are complex, change frequently, have tended to become more stringent over time and increase our cost of doing business. These laws and regulations include [...] anti-corruption and bribery laws such as the United States Foreign Corrupt Practices Act and U.K. Bribery Act, and trade sanctions laws and regulations. In the event new laws and regulations are enacted or existing laws are amended, implementing compliance with such new or amended laws may result in a loss of revenue, increased costs of doing business and a change to our strategic objectives, all of which could adversely affect our results of operations. In addition, we are subject to the risk that we, our affiliated entities or their respective officers, directors, employees and agents may take action determined to be in violation of any of these laws. An actual or alleged violation could result in substantial fines, sanctions, civil or criminal penalties, debarment from government contracts, curtailment of operations in certain jurisdictions, competitive or reputational harm, litigation or regulatory action and other consequences that might adversely affect our results of operations, financial condition or strategic objectives.

Cubic Corporation Form 10-K Item 1A: Risk Factors (fiscal year-end 8/31/2012)

We may be liable for civil or criminal penalties under a variety of complex laws and regulations, and changes in governmental regulations could adversely affect our business and financial condition.

Our businesses must comply with and are affected by various government regulations that impact our operating costs, profit margins and our internal organization and operation of our businesses. These regulations affect how we do business and, in some instances, impose added costs. Any changes in applicable laws could adversely affect our business and financial performance. Any material failure to comply with applicable laws could result in contract termination, price or fee reductions or suspension or debarment from contracting. The more significant regulations include: [...] the Foreign Corrupt Practices Act and the U.K. Bribery Act [...]

Apollo Group, Inc. Form 10-K Item 1A: Risk Factors (fiscal year-end 9/30/2012)

Our non-U.S. operations are subject to risks not inherent in our U.S. operations, which could adversely affect our business.

Through Apollo Global, we operate physical and online educational institutions in the United Kingdom, Europe, Chile, Mexico and elsewhere, and we are actively seeking further expansion in other countries, including India, where we have entered into a start-up joint venture. [...] In addition, our non-U.S. operations are subject to the U.S. Foreign Corrupt Practices Act and the U.K. Bribery Act which require extensive compliance vigilance on our part and, in some cases, puts our foreign operations at a competitive disadvantage with local companies. If one or more of our foreign operations ceases to be economically practical, we may be forced to discontinue such operations or seek a buyer, either of which might result in a substantial loss of value to Apollo Global and, therefore, Apollo Group.

A.3. Statements Regarding Section 7 Extraterritorial Jurisdiction

This appendix provides further insight into the criteria that subjects non-UK firms to the UKBA's extraterritorial jurisdiction under section 7, and thus the study's rationale for identifying US treatment firms as those disclosing a UK segment in the pre-adoption period. The first statement is an excerpt from official UKBA guidance published by the UK Ministry of Justice.

> *As regards bodies incorporated, or partnerships formed, outside the United Kingdom, whether such bodies can properly be regarded as carrying on a business or part of a business 'in any part of the United Kingdom' will [...] be answered by applying a common sense approach. Where there is a particular dispute as to whether a business presence in the United Kingdom satisfies the test in the Act, the final arbiter [...] will be the courts as set out above. However, the Government anticipates that applying a common sense approach would mean that organisations that do not have a demonstrable business presence in the United Kingdom would not be caught. The Government would not expect, for example, the mere fact that a company's securities have been admitted to the UK Listing Authority's Official List and therefore admitted to trading on the London Stock Exchange, in itself, to qualify that company as carrying on a business or part of a business in the UK and therefore falling within the definition of a 'relevant commercial organisation' for the purposes of section 7. Likewise, having a UK subsidiary will not, in itself, mean that a parent company is carrying on a business in the UK, since a subsidiary may act independently of its parent or other group companies* (Ministry of Justice 2012a, p. 15–16).

The second statement is an excerpt from the OECD Working Group on Bribery's Phase 3 report.

> *On his part, the SFO Director has stated repeatedly that he would not apply an overly "technical" interpretation to the term "carrying on business." Jurisdiction based solely on a stock listing is "unlikely." However, jurisdiction could be applied to companies that have "economic engagement" with the UK (i.e., trading, raising finance, carrying out corporate functions, or dealing with numerous stakeholders in the UK). At the on-site visit, the SFO explained that "carrying on a business" should be understood to be "buying and selling" in the UK* (OECD 2012, p. 16).

A.4. World Bank Data Items For Facilitating Payment Analysis

The World Bank Enterprise Survey on Corruption (World Bank Group 2019) surveys business owners and top managers in various countries.* In constructing the $HighFPRisk_i$ variable, I first determine the overlap between the country-level segments in my sample and the countries surveyed in the World Bank Enterprise Survey on Corruption. For each country in this list of countries, I calculate the average firm response to the six World Bank survey questions below; these questions involve activities conducted by firms that make facilitating payments. Next, I identify countries for which the average firm response is ranked in the top tercile. Finally, $HighFPRisk_i$ is equal to one if a firm discloses, as a country-level geographic segment, any pre-adoption revenue from one or more of the countries ranked in the top tercile, and zero otherwise.

1. It is said that establishments are sometimes required to make gifts or informal payments to public officials to "get things done" with regard to customs, taxes, licenses, regulations, services etc. On average, what percentage of total annual sales, or estimated total annual value, do establishments like this one pay in informal payments or gifts to public officials for this purpose?
2. In reference to [your establishment's] application for a construction-related permit, was an informal gift or payment expected or requested?
3. In reference to [your establishment's] application for an operating license, was an informal gift or payment expected or requested?
4. In reference to [your establishment's] application for an import license, was an informal gift or payment expected or requested?
5. In reference to [your establishment's] application for an electrical connection, was an informal gift or payment expected or requested?
6. In reference to [your establishment's] application for a water connection, was an informal gift or payment expected or requested?

*Further details on survey methodology may be found at https://www.enterprisesurveys.org/methodology (accessed April 2019). World Bank data is retrieved from https://www.enterprisesurveys.org/data/exploretopics/corruption#–7 (accessed April 2019).

Figure 1: Annual Number of Concluded Cases Brought Under the UKBA

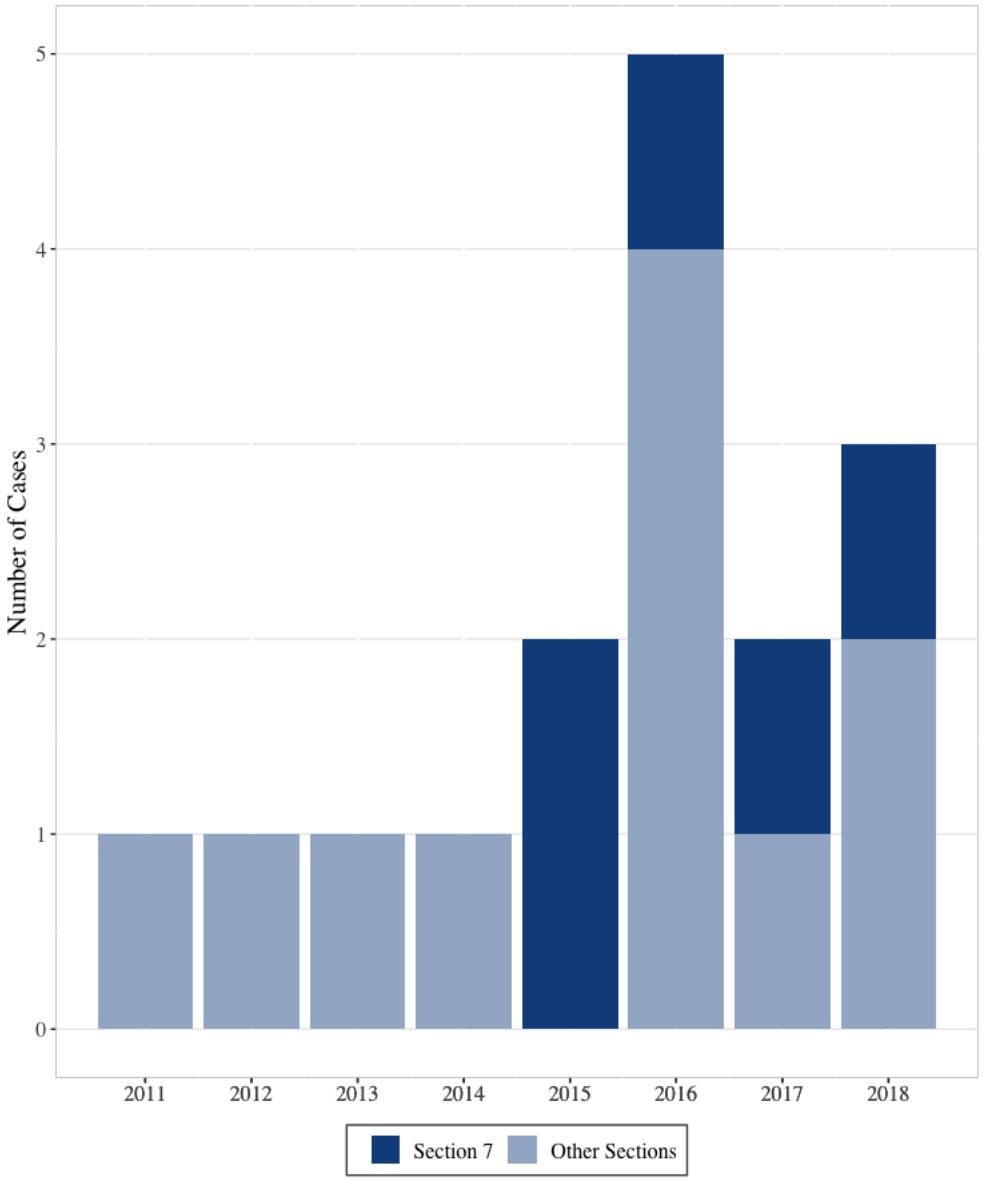

Figure 1 plots the number of settled cases brought under the UKBA by settlement year. Cases involving section 7 of the UKBA are represented in dark shading. Case information is retrieved from EY (2019).

Figure 2: Relative Changes in Mean CE (Unweighted)

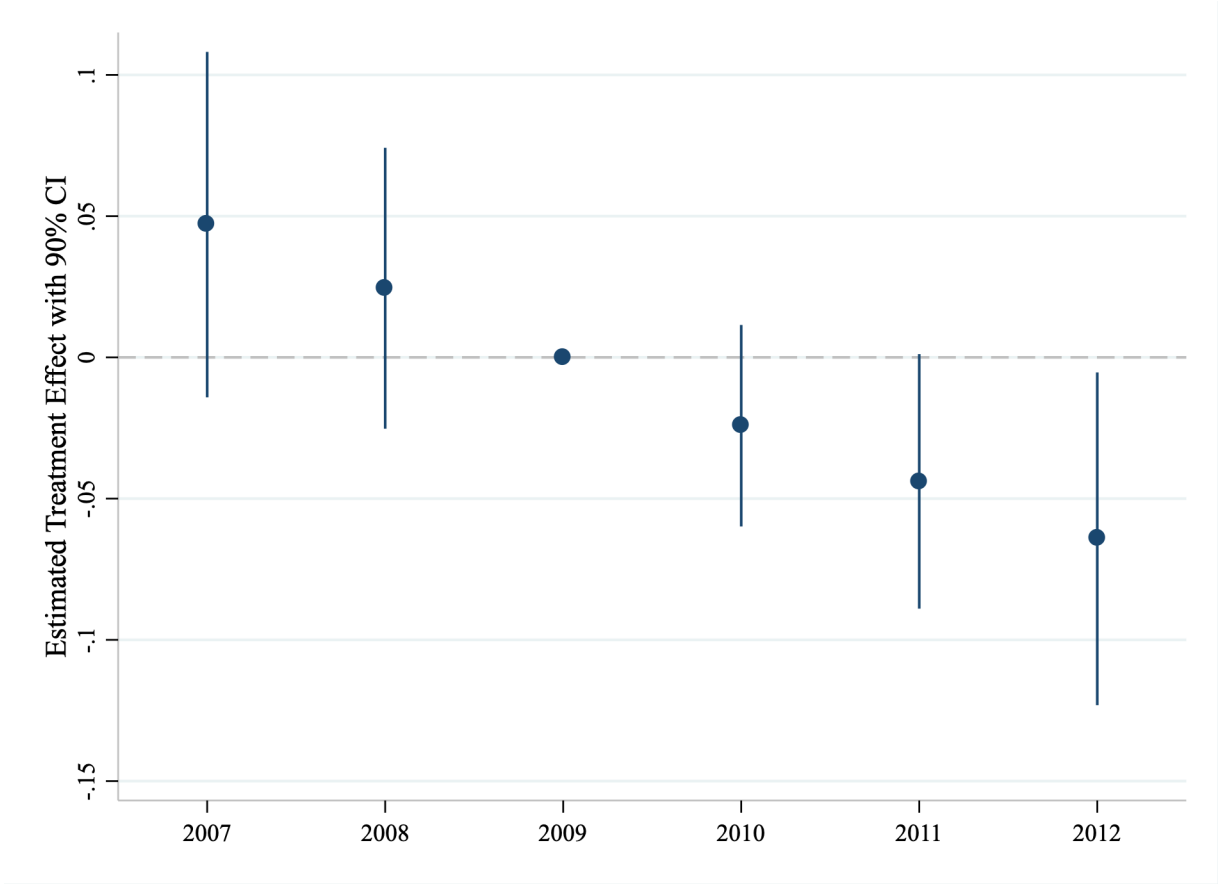

Figure 2 plots coefficient estimates and 90% confidence intervals from the unweighted regression presented in Column (1) of Table 6, which is estimated by replacing $Post_t$ in the Table 5 Column (3) specification with separate indicators for each sample year (other than 2009, which serves as the benchmark).

Figure 3: Relative Changes in Mean CE (Entropy-Balanced)

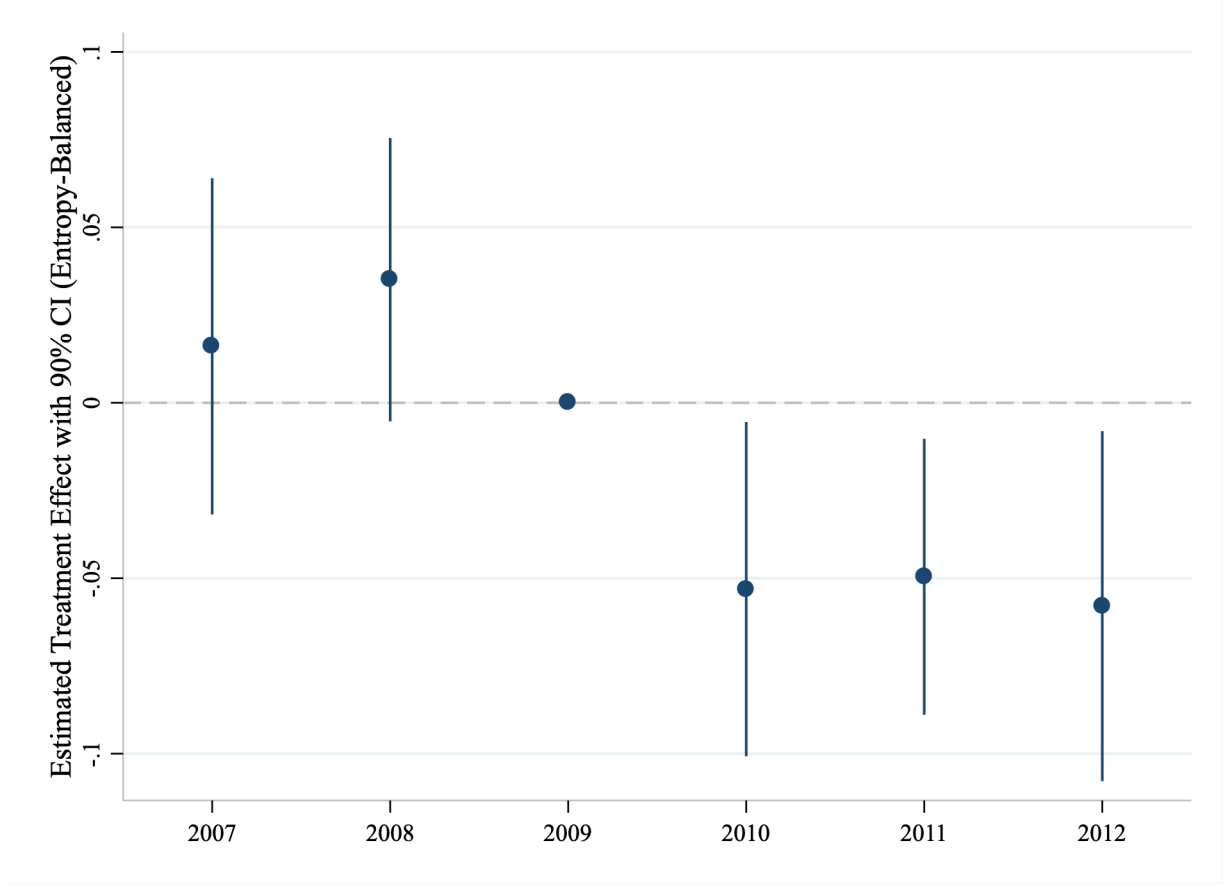

Figure 3 plots coefficient estimates and 90% confidence intervals from the entropy-balanced regression presented in Column (2) of Table 6, which is estimated by replacing $Post_t$ in the Table 5 Column (4) specification with separate indicators for each sample year (other than 2009, which serves as the benchmark).

Figure 4: Relative Changes in Mean CE (Unweighted – Extended Sample Period)

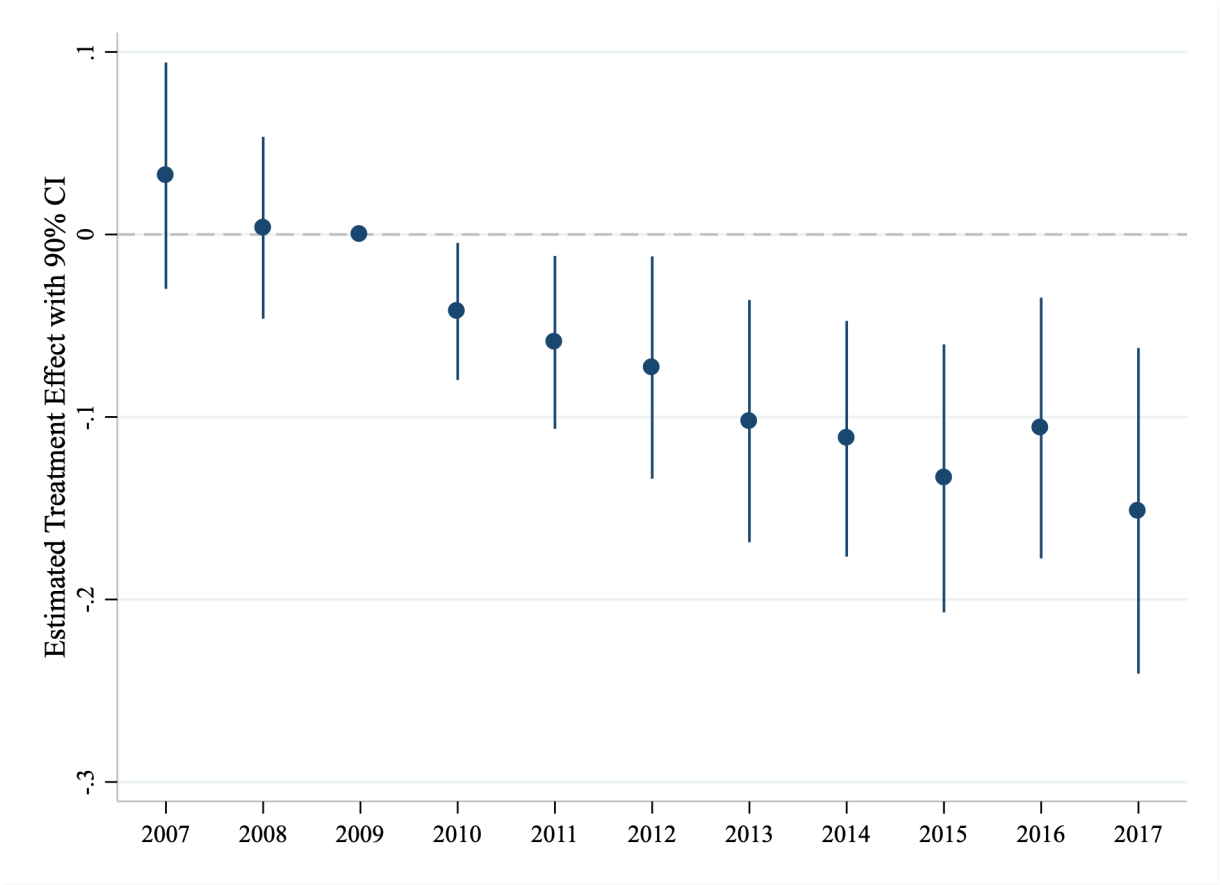

Figure 4 plots coefficient estimates and 90% confidence intervals from the unweighted regression presented in Column (1) of Table 9, which is estimated by replacing $Post_t$ in the Table 8 Column (3) specification with separate indicators for each sample year (other than 2009, which serves as the benchmark).

Figure 5: Relative Changes in Mean CE (Entropy-Balanced – Extended Sample Period)

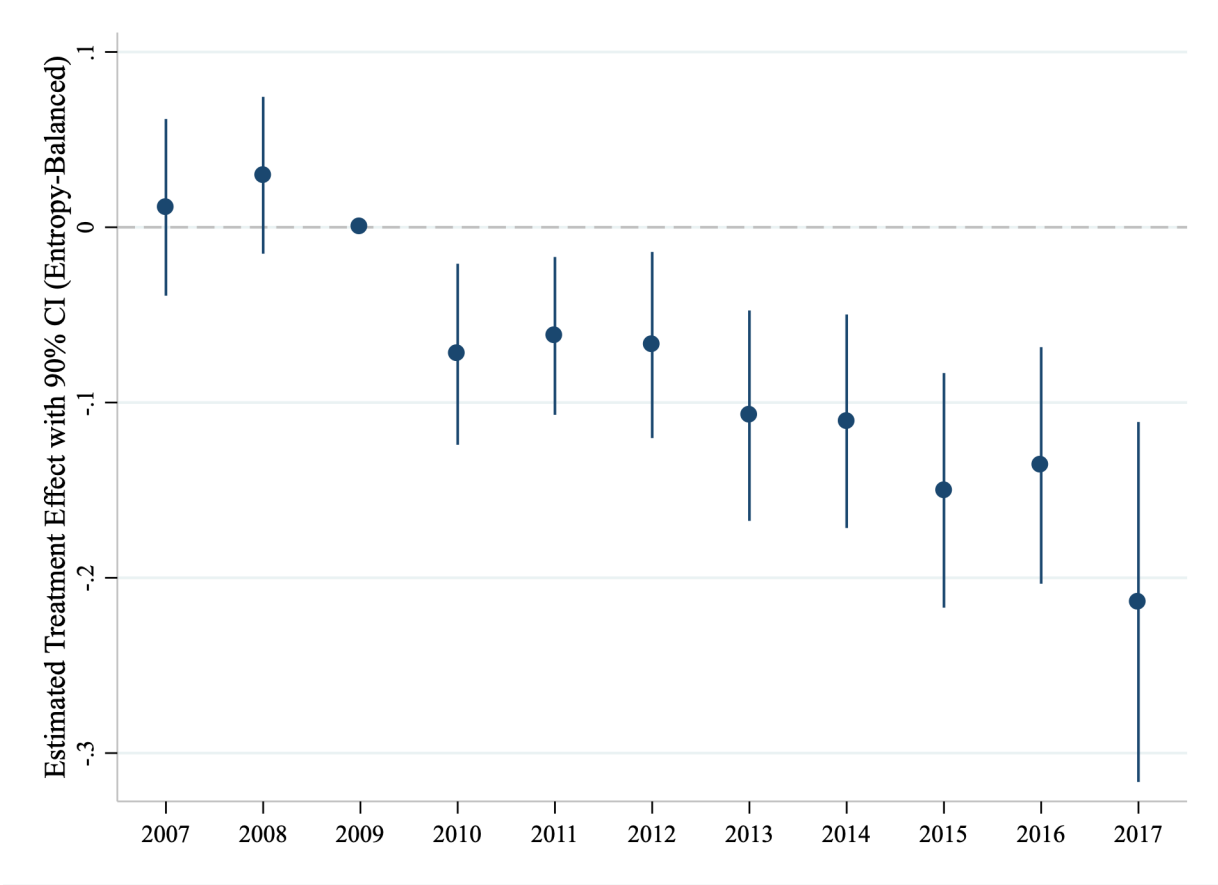

Figure 5 plots coefficient estimates and 90% confidence intervals from the entropy-balanced regression presented in Column (2) of Table 9, which is estimated by replacing $Post_t$ in the Table 8 Column (4) specification with separate indicators for each sample year (other than 2009, which serves as the benchmark).

Table 1: Industry Composition

	Treatment Firms	Percentage	Control Firms	Percentage
Agriculture	1	0.40	6	0.86
Food Products	1	0.40	16	2.31
Candy & Soda	0	0.00	3	0.43
Beer & Liquor	4	1.59	0	0.00
Recreation	3	1.20	6	0.86
Entertainment	2	0.80	5	0.72
Printing and Publishing	3	1.20	2	0.29
Consumer Goods	3	1.20	9	1.30
Apparel	2	0.80	14	2.02
Healthcare	3	1.20	4	0.58
Medical Equipment	10	3.98	32	4.61
Pharmaceutical Products	12	4.78	50	7.20
Chemicals	8	3.19	21	3.03
Rubber and Plastic Products	2	0.80	7	1.01
Textiles	1	0.40	2	0.29
Construction Materials	7	2.79	19	2.74
Construction	1	0.40	8	1.15
Steel Works, etc.	5	1.99	16	2.31
Fabricated Products	0	0.00	6	0.86
Machinery	17	6.77	32	4.61
Electrical Equipment	5	1.99	21	3.03
Automobiles and Trucks	10	3.98	18	2.59
Aircraft	6	2.39	4	0.58
Shipbuilding, Railroad Equipment	1	0.40	2	0.29
Precious Metals	1	0.40	3	0.43
Non-Metallic & Industrial Metal Mining	2	0.80	5	0.72
Coal	0	0.00	3	0.43
Petroleum and Natural Gas	9	3.59	30	4.32
Utilities	1	0.40	8	1.15
Communication	9	3.59	12	1.73
Personal Services	6	2.39	8	1.15
Business Services	53	21.12	55	7.93
Computers	9	3.59	19	2.74
Electronic Equipment	18	7.17	87	12.54
Measuring and Control Equipment	8	3.19	19	2.74
Business Supplies	3	1.20	14	2.02

	Treatment Firms	Percentage	Control Firms	Percentage
Shipping Containers	1	0.40	4	0.58
Transportation	2	0.80	15	2.16
Wholesale	4	1.59	31	4.47
Retail	1	0.40	23	3.31
Restaurants, Hotels, Motels	0	0.00	3	0.43
Banking	1	0.40	0	0.00
Insurance	1	0.40	6	0.86
Real Estate	3	1.20	6	0.86
Trading	8	3.19	19	2.74
Other	4	1.59	21	3.03
Total	251	100.00	694	100.00

Table 1 presents the industry composition of treatment and control firms before performing the entropy balancing procedure for the baseline sample over the period 2007–2012. Industry composition is based upon the Fama and French 48-industry classification.

Table 2: Mean Exposure to Corrupt Countries by Year

	Unweighted		Entropy-Balanced	
Year	Treatment	Control	Treatment	Control
2007	2.63	3.10	2.63	2.61
2008	2.64	3.29	2.64	2.64
2009	2.68	3.43	2.68	2.68
2010	2.70	3.46	2.70	2.75
2011	2.72	3.34	2.72	2.74
2012	2.74	3.30	2.74	2.78

Table 2 presents mean CE_{it} of treatment and control firms by year before and after performing the entropy balancing procedure for the baseline sample over the period 2007–2012. A full list of variable definitions and data sources is provided in Appendix A.1.

Table 3: Descriptive Statistics

Panel A: Summary Statistics Before Entropy Balancing

	N	Mean	Std.Dev.	P25	P50	P75
CE	5,077	3.15	1.37	2.41	2.50	3.30
$Assets$ (in millions)	5,077	4,667.03	19,071.98	104.28	575.65	2,625.99
ROA	5,077	-0.07	1.82	-0.03	0.04	0.09
MB	5,077	2.27	10.75	1.04	1.38	2.01
$SegCount$	5,077	4.71	3.08	3.00	4.00	6.00

Panel B: Summary Statistics After Entropy Balancing

	N	Mean	Std.Dev.	P25	P50	P75
CE	5,077	2.69	0.70	2.39	2.49	2.76
$Assets$ (in millions)	5,077	6,701.43	22,445.68	176.47	970.73	4,009.30
ROA	5,077	-0.01	0.34	-0.01	0.04	0.08
MB	5,077	1.75	1.71	1.08	1.41	1.99
$SegCount$	5,077	6.17	3.82	3.00	5.00	8.00

Panel C: Correlations Before Entropy Balancing

	CE	$Size$	ROA	$logMB$
$Size$	-0.174***	1		
ROA	0.0185	0.130***	1	
$logMB$	-0.125***	-0.170***	-0.169***	1
$SegCount$	-0.125***	0.263***	0.0332*	0.0218

Panel D: Correlations After Entropy Balancing

	CE	$Size$	ROA	$logMB$
$Size$	0.0982***	1		
ROA	0.0722***	0.260***	1	
$logMB$	0.0692***	-0.0644***	-0.0326*	1
$SegCount$	0.314***	0.233***	0.0958***	0.0128

Panel E: Frequency of Country-Level Segments

Country	Firm-Year-Segments	CPI Score
United States of America	4,816	7.5
Canada	1,827	8.7
China	1,339	3.6
United Kingdom	1,195	7.7
Japan	856	7.7

Panel E: Frequency of Country-Level Segments		
Country	Firm-Year-Segments	CPI Score
Germany	816	8.0
Mexico	605	3.3
France	409	6.9
Australia	406	8.7
South Korea	358	5.5
Taiwan	330	5.6
Italy	265	4.3
Brazil	263	3.7
Singapore	251	9.2
The Netherlands	224	8.9
India	211	3.4
Spain	166	6.1
Malaysia	150	4.5
Switzerland	130	9.0
Hong Kong	130	8.2
Israel	121	6.1
Philippines	114	2.4
Belgium	113	7.1
Russia	96	2.2
Argentina	91	2.9
Sweden	88	9.2
Norway	78	8.6
Ireland	77	8.0
South Africa	66	4.7
Thailand	65	3.4
Hungary	58	5.1
Poland	57	5.0
Chile	47	6.7
Denmark	44	9.3
Czech Republic	42	4.9
Colombia	41	3.7
Finland	35	8.9
New Zealand	34	9.4
United Arab Emirates	33	6.5
Venezuela	32	1.9
Vietnam	30	2.7
Costa Rica	28	5.3
Nigeria	27	2.5
Indonesia	26	2.8

Panel E: Frequency of Country-Level Segments		
Country	Firm-Year-Segments	CPI Score
Turkey	26	4.4
...	...	...
Total	16,800	

Table 3 presents descriptive statistics. Panels A and B (C and D) report summary statistics (correlations) before and after performing the entropy balancing procedure for the baseline sample over the period 2007–2012. Panel E reports the count of firm-year-segments and the 2009 CPI scores by country. For brevity, the list of countries in Panel E is limited to the 45 most frequently disclosed countries. In Panels C and D, statistical significance at the 0.1%, 1%, and 5% levels is denoted by ***, **, and *, respectively. A full list of variable definitions and data sources is provided in Appendix A.1.

Table 4: Pre-Adoption Distributional Properties Before and After Entropy Balancing

Panel A: Covariate Balance Before Entropy Balancing							
	Mean		Mean	Normalized	Variance		Variance
	Treatment	Control	Difference	Difference	Treatment	Control	Ratio
CE	2.65	3.28	-0.63***	-0.54	0.45	2.28	0.20
$Size$	6.66	5.98	0.68***	0.31	4.63	4.89	0.95
ROA	-0.02	-0.07	0.05**	0.10	0.18	0.42	0.43
$logMB$	0.40	0.43	-0.03	-0.05	0.25	0.46	0.54
$logSegCount$	1.62	1.26	0.36***	0.65	0.35	0.27	1.30
$logForeignRev$	5.44	4.62	0.82***	0.35	5.61	5.51	1.02

Panel B: Covariate Balance After Entropy Balancing							
	Mean		Mean	Normalized	Variance		Variance
	Treatment	Control	Difference	Difference	Treatment	Control	Ratio
CE	2.65	2.64	0.01	0.01	0.45	0.44	1.02
$Size$	6.66	6.69	-0.02	-0.01	4.63	4.60	1.01
ROA	-0.02	-0.01	-0.01	-0.02	0.18	0.06	3.00
$logMB$	0.40	0.41	-0.00	-0.01	0.25	0.25	1.00
$logSegCount$	1.62	1.62	-0.00	-0.01	0.35	0.34	1.03
$logForeignRev$	5.44	5.46	-0.02	-0.01	5.61	5.44	1.03

Table 4 presents the pre-adoption (i.e., 2007–2009) mean and variance of treatment and control firm variables before and after performing the entropy balancing procedure for the baseline sample. The mean difference is the simple difference in means. Statistical significance of univariate t-tests of the mean difference at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively. The normalized difference is equal to $(\bar{x}_{tr} - \bar{x}_c) \div \left(\frac{s^2_{tr}+s^2_c}{2}\right)^{\frac{1}{2}}$ where $\bar{x}_{tr}$ (s^2_{tr}) and $\bar{x}_c$ (s^2_c) denote the pre-adoption sample mean (variance) of treatment and control firms, respectively. The variance ratio is equal to $s^2_{tr} \div s^2_c$. A full list of variable definitions and data sources is provided in Appendix A.1.

Table 5: Effect of the UKBA on US Firms' Exposure to Corrupt Countries

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0628***	-0.0649**	-0.0633***	-0.0694***
	(-2.8800)	(-2.5617)	(-2.8685)	(-3.0176)
$Size$	0.0089	0.0451	0.0195	0.0457*
	(0.3880)	(1.5012)	(0.8757)	(1.9548)
ROA	0.0317***	-0.0156	0.0291***	-0.0242
	(7.2550)	(-0.7052)	(6.2880)	(-1.1053)
$logMB$	0.0211	0.0161	0.0034	0.0055
	(0.8633)	(0.4260)	(0.1341)	(0.1767)
$logSegCount$	0.1496**	0.0005	0.1541**	0.0021
	(1.9635)	(0.0094)	(2.0457)	(0.0416)
Observations	5,077	5,077	5,070	5,070
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.957	0.890	0.957	0.903

Table 5 reports regressions estimating the treatment effect of UKBA adoption on CE_{it} for the baseline sample over the period 2007–2012. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure, which reweights control firm observations to obtain covariate balance with treatment firm observations in the pre-adoption period. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 6: Test of Pre-Adoption Common Trends

	(1) Unweighted Sample	(2) Entropy-Balanced
$Treat \times 2007$	0.0470	0.0161
	(1.2657)	(0.5527)
$Treat \times 2008$	0.0245	0.0351
	(0.8102)	(1.4303)
$Treat \times 2010$	-0.0242	-0.0531*
	(-1.1168)	(-1.8359)
$Treat \times 2011$	-0.0439	-0.0496**
	(-1.6041)	(-2.0767)
$Treat \times 2012$	-0.0642*	-0.0579*
	(-1.7956)	(-1.9131)
Observations	5,070	5,070
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Country-Year Fixed Effects	Yes	Yes
Industry×Year Fixed Effects	Yes	Yes
Standard Error Clusters	Firm	Firm
Adjusted R^2	0.957	0.903
Pre-Adoption Joint F Statistic	0.874	1.655
Pre-Adoption Joint F P-value	0.418	0.192

Table 6 reports, for the baseline sample over the period 2007–2012, results of estimating Equation 2 after replacing $Post_t$ with separate indicators for each sample year (other than 2009, which serves as the benchmark). Columns (1) and (2) report results before and after performing the entropy balancing procedure, respectively. The pre-adoption joint F-statistic and p-value result from a test of the null hypothesis that the $Treat_i \times 2007$ and $Treat_i \times 2008$ estimated coefficients jointly equal zero. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry×year fixed effects, respectively. Regressions are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the controls and intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 7: Placebo Regressions ("Treatment" Assigned to Germany-Exposed Firms)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0065	-0.0282	-0.0226	-0.0251
	(-0.2073)	(-0.5349)	(-0.7260)	(-0.5296)
$Size$	0.0104	0.0393	0.0171	0.0411
	(0.4273)	(0.9513)	(0.7540)	(1.0810)
ROA	0.0320***	-0.0025	0.0298***	-0.0022
	(7.7047)	(-0.2417)	(6.9745)	(-0.2212)
$logMB$	0.0207	0.0598	0.0033	0.0535
	(0.8170)	(0.9932)	(0.1258)	(0.8102)
$logSegCount$	0.1599**	-0.0339	0.1652**	-0.0486
	(2.0833)	(-0.4024)	(2.1909)	(-0.5868)
Observations	5,077	5,077	5,070	5,070
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.957	0.874	0.958	0.886

Table 7 reports placebo regressions estimating the treatment effect of UKBA adoption on CE_{it} over the period 2007–2012. $Treat_i$ equals one if firm i discloses a geographic segment for Germany (as opposed to the UK) in at least one sample year prior to 2010, and zero otherwise. The control group comprises US firms that disclose at least one non-US country-level segment in at least one sample year prior to 2010, but that do not disclose a geographic segment for Germany in any sample year. The treatment (control) group comprises 170 (773) distinct firms and 938 (4,139) firm-year observations. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercepts are untabulated. Robust standard errors clustered at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 8: Effect of the UKBA on US Firms' Exposure to Corrupt Countries
(Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0887***	-0.1033***	-0.0942***	-0.1116***
	(-3.4356)	(-3.4825)	(-3.5647)	(-4.1621)
$Size$	0.0209	0.0343	0.0216	0.0234
	(1.0033)	(1.4606)	(0.9909)	(0.9825)
ROA	0.0200*	-0.0268	0.0188*	-0.0447
	(1.8111)	(-0.7300)	(1.7711)	(-1.2582)
$logMB$	0.0387*	0.0458*	0.0179	0.0280
	(1.8531)	(1.9509)	(0.8155)	(1.2106)
$logSegCount$	0.1100**	0.0686	0.1218**	0.0720*
	(2.0194)	(1.5515)	(2.2029)	(1.9502)
Observations	7,905	7,905	7,887	7,887
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.946	0.874	0.947	0.885

Table 8 reports regressions estimating the treatment effect of UKBA adoption on CE_{it} for the extended sample over the period 2007–2017. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 9: Test of Pre-Adoption Common Trends (Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced
$Treat \times 2007$	0.0322	0.0114
	(0.8553)	(0.3713)
$Treat \times 2008$	0.0037	0.0297
	(0.1222)	(1.0913)
$Treat \times 2010$	-0.0422*	-0.0725**
	(-1.8479)	(-2.3101)
$Treat \times 2011$	-0.0591**	-0.0620**
	(-2.0543)	(-2.2665)
$Treat \times 2012$	-0.0729**	-0.0672**
	(-1.9711)	(-2.0830)
$Treat \times 2013$	-0.1023**	-0.1075***
	(-2.5356)	(-2.9487)
$Treat \times 2014$	-0.1119***	-0.1107***
	(-2.8546)	(-2.9916)
$Treat \times 2015$	-0.1336***	-0.1501***
	(-2.9969)	(-3.6933)
$Treat \times 2016$	-0.1061**	-0.1359***
	(-2.4451)	(-3.3155)
$Treat \times 2017$	-0.1514***	-0.2138***
	(-2.7932)	(-3.4279)
Observations	7,887	7,887
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Country-Year Fixed Effects	Yes	Yes
Industry$\times$Year Fixed Effects	Yes	Yes
Standard Error Clusters	Firm	Firm
Adjusted R^2	0.947	0.886
Pre-Adoption Joint F Statistic	0.794	0.920
Pre-Adoption Joint F P-value	0.452	0.399

Table 9 reports, for the extended sample over the period 2007–2017, results of estimating Equation 2 after replacing $Post_t$ with separate indicators for each sample year (other than 2009, which serves as the benchmark). Columns (1) and (2) report results before and after performing the entropy balancing procedure, respectively. The pre-adoption joint F-statistic and p-value result from a test of the null hypothesis that the $Treat_i \times 2007$ and $Treat_i \times 2008$ estimated coefficients jointly equal zero. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for

each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry$\times$year fixed effects, respectively. Regressions are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the controls and intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 10: Effect of the UKBA on US Firms' Business in the United Kingdom

Panel A: Treatment Firm Pre-Adoption Means by Post-Adoption UK Segment Disclosure Status

Group	N	# of Firms	CE	$logUKRev$	$logMB$	ROA	$Size$	$logSegCount$
(1) No Post-Adoption UK Segment	116	40	2.86	2.68	0.44	-0.08	5.98	1.51
(2) Post-Adoption UK Segment	593	211	2.61	4.09	0.40	-0.00	6.80	1.64
Difference			0.25***	-1.41***	0.05	-0.08*	-0.81***	-0.13**

Panel B: Likelihood and Determinants of Discontinuing Post-Adoption UK Segment Disclosure

	Dependent Variable = $DropSeg_i$			Dependent Variable = $NoUKSeg\_post_i$		
	(1) LPM	(2) Probit	(3) Logistic	(4) LPM	(5) Probit	(6) Logistic
$UKTreat$	0.1105**	0.3151*	0.5206*			
	(2.0252)	(1.9503)	(1.9280)			
CE_pre				0.1006*	0.3289**	0.5640**
				(1.9455)	(2.1854)	(2.2157)
$logUKRev\_pre$				-0.0473**	-0.2139***	-0.3518***
				(-2.5706)	(-3.2125)	(-3.1328)
$logMB\_pre$				0.0193	0.0238	0.0251
				(0.3545)	(0.1116)	(0.0684)
ROA_pre				-0.2100	-0.6309	-0.9887
				(-1.2116)	(-1.2166)	(-1.0553)
$Size\_pre$				0.0244	0.0974	0.1410
				(1.2741)	(1.3300)	(1.0980)
$logSegCount\_pre$				-0.0907**	-0.3729*	-0.6547*
				(-1.9935)	(-1.9037)	(-1.7922)
Observations	325	325	325	248	248	248
Standard Error Clusters	Firm	Firm	Firm	Firm	Firm	Firm
Adjusted/Pseudo R^2	0.009	0.009	0.009	0.077	0.106	0.102

Panel A of Table 10 reports mean CE_{it}, $logUKRev_{it}$, $logMB_{it}$, ROA_{it}, $Size_{it}$, and $logSegCount_{it}$ over the pre-adoption period conditional on the whether the treatment firm continues to disclose a UK segment in the post-adoption period. Treatment firms in group (1) do not disclose a post-adoption UK segment. Treatment firms in group (2) continue to disclose a post-adoption UK segment. Univariate mean differences between the two groups are reported in the last

row of Panel A. Panel B reports regressions estimating: (A) the likelihood that firms with a pre-adoption UK segment (i.e., treatment firms) do not disclose a post-adoption UK segment, relative to the likelihood that a control group of firms with a pre-adoption German segment do not disclose a post-adoption German segment [Columns (1) – (3)]; and (B) potential determinants of treatment firms' decision to not disclose a post-adoption UK segment [Columns (4) – (6)]. Specifically, Columns (1) – (3) report cross-sectional regressions estimating Equation 3. $UKTreat_i$ is equal to one if firm i discloses a segment for the UK, but not for Germany, in at least one sample year prior to 2010, and zero if firm i discloses a segment for Germany, but not for the UK, in at least one sample year prior to 2010. In estimating Equation 3, the sample consists only of firms for which $UKTreat_i$ is equal to either one (227 firms) or zero (98 firms). When $UKTreat_i$ is equal to one (zero), $DropSeg_i$ is equal to one if firm i does not disclose a UK (German) segment in any post-adoption year, and zero otherwise. Column (1), (2), and (3) reports regressions estimating Equation 3 using a linear probability model (LPM), a probit model, and a logistic model, respectively. Columns (4) – (6) report cross-sectional regressions estimating Equation 4 for the pre-adoption sample of treatment firm-level observations. $NoUKSeg\_post_i$ is equal to one if treatment firm i does not disclose a post-adoption UK segment, and zero otherwise. CE_pre_i, $logUKRev\_pre_i$, $logMB\_pre_i$, ROA_pre_i, $Size\_pre_i$, and $logSegCount\_pre_i$ are the firm-level means of CE_{it}, $logUKRev_{it}$, $logMB_{it}$, ROA_{it}, $Size_{it}$, and $logSegCount_{it}$ over the pre-adoption period. The sample comprises 248 treatment firms (of the 251 treatment firms that disclose a pre-adoption UK segment, 3 are missing UK revenue data). Coefficient estimates for the intercepts are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 11: Conditional Regressions – Incremental Exposure to the UKBA over the FCPA

	(1)	(2)
$Treat \times Post \times HighFPRisk$	-0.0190	
	(-0.1877)	
$Treat \times Post \times HighFPRiskRev$		-0.4345***
		(-2.9630)
$Post \times HighFPRisk$	0.0518	
	(0.7162)	
$Post \times HighFPRiskRev$		0.1705
		(1.4226)
$Treat \times Post$	-0.0675***	-0.0547**
	(-2.8357)	(-2.3203)
Observations	5,070	5,070
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Country-Year Fixed Effects	Yes	Yes
Industry×Year Fixed Effects	Yes	Yes
Standard Error Clusters	Firm	Firm
Adjusted R^2	0.903	0.904

Table 11 reports regressions conditional on pre-adoption exposure to countries where facilitating payments are common for the baseline sample over the period 2007–2012. Column (1) presents results from Ordinary Least Squares (OLS) estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighFPRisk_i$ and $Post_t \times HighFPRisk_i$. Column (2) presents results from OLS estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighFPRiskRev_i$ and $Post_t \times HighFPRiskRev_i$. $HighFPRisk_i$ is an indicator variable equal to one if firm i discloses pre-adoption segment revenue from at least one country for which facilitating payments are most common, i.e., at least one country for which the average response to six World Bank Enterprise Corruption Survey data items relating to facilitating payments is ranked in the top tercile among countries represented in the sample, and zero otherwise (see Appendix A.4 for additional details). $HighFPRiskRev_i$ is an indicator variable equal to one if $HighFPRisk_i = 1$ for firm i *and,* among firms for which $HighFPRisk_i = 1$, firm i has an above-median pre-adoption proportion of revenue from countries for which facilitating payments are most common, i.e., segment revenue from countries where facilitating payments are most common divided by total revenue. Regressions are estimated after performing the entropy balancing procedure. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry×year fixed effects, respectively. $HighFPRisk_i$, $HighFPRiskRev_i$, and $Treat_i \times HighFPRisk_i$, and $Treat_i \times HighFPRiskRev_i$ are subsumed by firm fixed effects. Regressions are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 12: Conditional Regressions – Incremental Exposure to the UKBA over the FCPA (Extended Sample Period)

	(1)	(2)
$Treat \times Post \times HighFPRisk$	-0.1836**	
	(-2.1496)	
$Treat \times Post \times HighFPRiskRev$		-0.3217**
		(-2.0424)
$Post \times HighFPRisk$	0.1309*	
	(1.8640)	
$Post \times HighFPRiskRev$		0.1304*
		(1.9190)
$Treat \times Post$	-0.0902***	-0.0968***
	(-3.3458)	(-3.6067)
Observations	7,887	7,887
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Country-Year Fixed Effects	Yes	Yes
Industry×Year Fixed Effects	Yes	Yes
Standard Error Clusters	Firm	Firm
Adjusted R^2	0.886	0.886

Table 12 reports regressions conditional on pre-adoption exposure to countries where facilitating payments are common for the extended sample over the period 2007–2017. Column (1) presents results from Ordinary Least Squares (OLS) estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighFPRisk_i$ and $Post_t \times HighFPRisk_i$. Column (2) presents results from OLS estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighFPRiskRev_i$ and $Post_t \times HighFPRiskRev_i$. $HighFPRisk_i$ is an indicator variable equal to one if firm i discloses pre-adoption segment revenue from at least one country for which facilitating payments are most common, i.e., at least one country for which the average response to six World Bank Enterprise Corruption Survey data items relating to facilitating payments is ranked in the top tercile among countries represented in the sample, and zero otherwise (see Appendix A.4 for additional details). $HighFPRiskRev_i$ is an indicator variable equal to one if $HighFPRisk_i = 1$ for firm i *and,* among firms for which $HighFPRisk_i = 1$, firm i has an above-median pre-adoption proportion of revenue from countries for which facilitating payments are most common, i.e., segment revenue from countries where facilitating payments are most common divided by total revenue. Regressions are estimated after performing the entropy balancing procedure. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry×year fixed effects, respectively. $HighFPRisk_i$, $HighFPRiskRev_i$, and $Treat_i \times HighFPRisk_i$, and $Treat_i \times HighFPRiskRev_i$ are subsumed by firm fixed effects. Regressions are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 13: Conditional Regressions – Bribery and Enforcement Risk

	(1)	(2)	(3)
$Treat \times Post \times HighBribeRisk$	-0.1018*		-0.1040**
	(-1.9521)		(-2.0078)
$Treat \times Post \times logUKRev$		-0.0111*	-0.0105*
		(-1.9427)	(-1.8922)
$Post \times HighBribeRisk$	0.1327***		0.1326***
	(2.9030)		(2.8942)
$Treat \times logUKRev$		-0.0086	-0.0099
		(-0.7757)	(-0.9042)
$Treat \times Post$	-0.0323	-0.0316	0.0041
	(-1.6293)	(-0.9588)	(0.1423)
Observations	5,070	5,070	5,070
Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes
Industry×Year Fixed Effects	Yes	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm
Adjusted R^2	0.904	0.903	0.904

Table 13 reports regressions conditional on proxies for enforcement risk and pre-adoption bribery risk for the baseline sample over the period 2007–2012. Column (1) presents results from Ordinary Least Squares (OLS) estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighBribeRisk_i$ and $Post_t \times HighBribeRisk_i$. $HighBribeRisk_i$ is an indicator variable equal to one if, in the pre-adoption period, firm i discloses a segment for at least one country ranked in the top five countries perceived to be sources of foreign bribery according to Transparency International's 2008 Bribe Payers Index. Column (2) presents results from OLS estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times logUKRev_{it}$ and $Treat_i \times logUKRev_{it}$. $logUKRev_{it}$ is equal to the natural logarithm of one plus revenues from a UK geographic segment. Column (3) presents results from OLS estimations of Equation 2 after including additional variables in the previous two columns. Regressions are estimated after performing the entropy balancing procedure. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry×year fixed effects, respectively. $HighBribeRisk_i$ and $Treat_i \times HighBribeRisk_i$ are subsumed by firm fixed effects. $Post_t \times logUKRev_{it}$ and $logUKRev_{it}$ are subsumed by $Treat_i \times Post_t \times logUKRev_{it}$ because $logUKRev_{it}$ is always equal to zero when $Treat_i$ is equal to zero. Regressions are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table 14: Conditional Regressions – Bribery and Enforcement Risk (Extended Sample Period)

	(1)	(2)	(3)
$Treat \times Post \times HighBribeRisk$	-0.1170*		-0.1205*
	(-1.8558)		(-1.9473)
$Treat \times Post \times logUKRev$		-0.0156**	-0.0151**
		(-2.0432)	(-2.0869)
$Post \times HighBribeRisk$	0.1432***		0.1443***
	(2.6885)		(2.7030)
$Treat \times logUKRev$		-0.0089	-0.0103
		(-0.7318)	(-0.8707)
$Treat \times Post$	-0.0672***	-0.0582	-0.0142
	(-3.1138)	(-1.3697)	(-0.4021)
Observations	7,887	7,887	7,887
Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes
Industry×Year Fixed Effects	Yes	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm
Adjusted R^2	0.886	0.886	0.887

Table 14 reports regressions conditional on proxies for enforcement risk and pre-adoption bribery risk for the extended sample over the period 2007–2017. Column (1) presents results from Ordinary Least Squares (OLS) estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times HighBribeRisk_i$ and $Post_t \times HighBribeRisk_i$. $HighBribeRisk_i$ is an indicator variable equal to one if, in the pre-adoption period, firm i discloses a segment for at least one country ranked in the top five countries perceived to be sources of foreign bribery according to Transparency International's 2008 Bribe Payers Index. Column (2) presents results from OLS estimations of Equation 2 after including additional variables $Treat_i \times Post_t \times logUKRev_{it}$ and $Treat_i \times logUKRev_{it}$. $logUKRev_{it}$ is equal to the natural logarithm of one plus revenues from a UK geographic segment. Column (3) presents results from OLS estimations of Equation 2 after including additional variables in the previous two columns. Regressions are estimated after performing the entropy balancing procedure. Controls include $Size_{it}$, ROA_{it}, $logMB_{it}$, and $logSegCount_{it}$. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and industry×year fixed effects, respectively. $HighBribeRisk_i$ and $Treat_i \times HighBribeRisk_i$ are subsumed by firm fixed effects. $Post_t \times logUKRev_{it}$ and $logUKRev_{it}$ are subsumed by $Treat_i \times Post_t \times logUKRev_{it}$ because $logUKRev_{it}$ is always equal to zero when $Treat_i$ is equal to zero. Regressions are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Supplementary Appendices

S.1. Descriptive Statistics for the Extended Sample

Table S.1.1: Industry Composition (Extended Sample Period)

	Treatment Firms	Percentage	Control Firms	Percentage
Agriculture	1	0.39	6	0.87
Food Products	1	0.39	16	2.31
Candy & Soda	0	0.00	3	0.43
Beer & Liquor	4	1.54	0	0.00
Recreation	3	1.16	6	0.87
Entertainment	2	0.77	5	0.72
Printing and Publishing	3	1.16	2	0.29
Consumer Goods	3	1.16	9	1.30
Apparel	3	1.16	16	2.31
Healthcare	3	1.16	4	0.58
Medical Equipment	10	3.86	31	4.48
Pharmaceutical Products	12	4.63	48	6.94
Chemicals	8	3.09	21	3.03
Rubber and Plastic Products	2	0.77	7	1.01
Textiles	1	0.39	2	0.29
Construction Materials	7	2.70	18	2.60
Construction	1	0.39	8	1.16
Steel Works, etc.	5	1.93	16	2.31
Fabricated Products	0	0.00	6	0.87
Machinery	17	6.56	30	4.34
Electrical Equipment	5	1.93	21	3.03
Automobiles and Trucks	11	4.25	16	2.31
Aircraft	6	2.32	4	0.58
Shipbuilding, Railroad Equipment	1	0.39	2	0.29
Precious Metals	1	0.39	3	0.43
Non-Metallic & Industrial Metal Mining	2	0.77	5	0.72
Coal	0	0.00	3	0.43
Petroleum and Natural Gas	9	3.47	30	4.34
Utilities	1	0.39	8	1.16
Communication	9	3.47	12	1.73
Personal Services	6	2.32	8	1.16
Business Services	55	21.24	59	8.53
Computers	9	3.47	19	2.75

	Treatment Firms	Percentage	Control Firms	Percentage
Electronic Equipment	20	7.72	90	13.01
Measuring and Control Equipment	9	3.47	20	2.89
Business Supplies	3	1.16	15	2.17
Shipping Containers	1	0.39	3	0.43
Transportation	2	0.77	13	1.88
Wholesale	4	1.54	30	4.34
Retail	1	0.39	22	3.18
Restaurants, Hotels, Motels	0	0.00	3	0.43
Banking	2	0.77	0	0.00
Insurance	1	0.39	6	0.87
Real Estate	3	1.16	6	0.87
Trading	8	3.09	19	2.75
Other	4	1.54	21	3.03
Total	259	100.00	692	100.00

Table S.1.1 presents the industry composition of treatment and control firms before performing the entropy balancing procedure for the extended sample over the period 2007–2017. Industry composition is based upon the Fama and French 48-industry classification.

Table S.1.2: Mean Exposure to Corrupt Countries by Year (Extended Sample Period)

	Unweighted		Entropy-Balanced	
Year	Treatment	Control	Treatment	Control
2007	2.62	3.11	2.62	2.61
2008	2.63	3.29	2.63	2.63
2009	2.67	3.44	2.67	2.67
2010	2.70	3.48	2.70	2.76
2011	2.72	3.36	2.72	2.74
2012	2.74	3.32	2.74	2.78
2013	2.75	3.24	2.75	2.80
2014	2.77	3.25	2.77	2.85
2015	2.73	3.20	2.73	2.87
2016	2.72	3.17	2.72	2.85
2017	2.76	3.17	2.76	2.96

Table S.1.2 presents mean CE_{it} of treatment and control firms by year before and after performing the entropy balancing procedure for the extended sample over the period 2007–2017. A full list of variable definitions and data sources is provided in Appendix A.1.

Table S.1.3: Descriptive Statistics (Extended Sample Period)

Panel A: Summary Statistics Before Entropy Balancing

	N	Mean	Std.Dev.	P25	P50	P75
CE	7,905	3.12	1.31	2.43	2.50	3.28
$Assets$ (in millions)	7,905	5,578.82	20,690.03	136.73	769.13	3,140.60
ROA	7,905	2.28	9.98	1.09	1.44	2.08
MB	7,905	-0.07	1.68	-0.02	0.04	0.08
$SegCount$	7,905	4.82	3.14	3.00	4.00	6.00

Panel B: Summary Statistics After Entropy Balancing

	N	Mean	Std.Dev.	P25	P50	P75
CE	7,905	2.73	0.73	2.41	2.50	2.85
$Assets$ (in millions)	7,905	7,697.51	24,591.70	235.40	1,301.51	4,770.00
ROA	7,905	1.83	1.55	1.13	1.48	2.07
MB	7,905	-0.01	0.33	-0.01	0.04	0.08
$SegCount$	7,905	6.15	3.78	3.00	5.00	8.00

Panel C: Correlations Before Entropy Balancing

	CE	$Size$	ROA	$logMB$
$Size$	-0.152***	1		
ROA	0.0135	0.142***	1	
$logMB$	-0.159***	-0.131***	-0.182***	1
$SegCount$	-0.0695***	0.259***	0.0315**	0.0369**

Panel D: Correlations After Entropy Balancing

	CE	$Size$	ROA	$logMB$
$Size$	0.114***	1		
ROA	0.0550***	0.287***	1	
$logMB$	0.0609***	-0.0500***	-0.0776***	1
$SegCount$	0.379***	0.255***	0.131***	-0.0124

Panel E: Frequency of Country-Level Segments

Country	Firm-Year-Segments	CPI Score
United States of America	7,554	7.5
Canada	2,813	8.7
China	2,143	3.6
United Kingdom	1,802	7.7

Panel E: Frequency of Country-Level Segments		
Country	Firm-Year-Segments	CPI Score
Japan	1,314	7.7
Germany	1,212	8.0
Mexico	1,033	3.3
Australia	669	8.7
France	625	6.9
South Korea	605	5.5
Taiwan	528	5.6
Brazil	474	3.7
Singapore	437	9.2
Italy	407	4.3
India	373	3.4
Netherlands	351	8.9
Malaysia	264	4.5
Spain	240	6.1
Hong Kong	211	8.2
Switzerland	208	9.0
Philippines	195	2.4
Israel	186	6.1
Belgium	169	7.1
Russia	145	2.2
Argentina	138	2.9
Sweden	123	9.2
Thailand	123	3.4
Norway	122	8.6
Ireland	114	8.0
Hungary	103	5.1
Colombia	91	3.7
South Africa	90	4.7
Poland	89	5.0
Chile	87	6.7
Denmark	78	9.3
Czech Republic	70	4.9
New Zealand	59	9.4
Vietnam	58	2.7
United Arab Emirates	54	6.5
Finland	53	8.9
Peru	51	3.7
Indonesia	47	2.8
Nigeria	45	2.5

Panel E: Frequency of Country-Level Segments		
Country	Firm-Year-Segments	CPI Score
Saudi Arabia	45	4.3
Venezuela	45	1.9
...	...	...
Total	26,592	

Table S.1.3 presents descriptive statistics for the extended sample. Panels A and B (C and D) report summary statistics (correlations) before and after performing the entropy balancing procedure for the extended sample over the period 2007–2017. Panel E reports the count of firm-year-segments and the 2009 CPI scores by country. For brevity, the list of countries in Panel E is limited to the 45 most frequently disclosed countries. In Panels C and D, statistical significance at the 0.1%, 1%, and 5% levels is denoted by ***, **, and *, respectively. A full list of variable definitions and data sources is provided in Appendix A.1.

Table S.1.4: Pre-Adoption Distributional Properties Before and After Entropy Balancing (Extended Sample Period)

Panel A: Covariate Balance Before Entropy Balancing							
	Mean		Mean	Normalized	Variance		Variance
	Treatment	Control	Difference	Difference	Treatment	Control	Ratio
CE	2.64	3.29	-0.65***	-0.56	0.45	2.26	0.20
$Size$	6.66	5.99	0.67***	0.31	4.63	4.88	0.95
ROA	-0.02	-0.07	0.05**	0.09	0.18	0.42	0.43
$logMB$	0.40	0.43	-0.03	-0.05	0.25	0.46	0.54
$logSegCount$	1.61	1.26	0.35***	0.64	0.35	0.27	1.30
$logForeignRev$	5.45	4.62	0.83***	0.35	5.57	5.54	1.01
Panel B: Covariate Balance After Entropy Balancing							
	Mean		Mean	Normalized	Variance		Variance
	Treatment	Control	Difference	Difference	Treatment	Control	Ratio
CE	2.64	2.64	0.00	0.01	0.45	0.43	1.05
$Size$	6.66	6.68	-0.02	-0.01	4.63	4.62	1.00
ROA	-0.02	-0.02	-0.00	-0.01	0.18	0.06	3.00
$logMB$	0.40	0.41	-0.00	-0.01	0.25	0.26	0.96
$logSegCount$	1.61	1.61	-0.00	-0.00	0.35	0.34	1.03
$logForeignRev$	5.45	5.47	-0.02	-0.01	5.57	5.47	1.02

Table S.1.4 presents the pre-adoption (i.e., 2007–2009) mean and variance of treatment and control firm variables before and after performing the entropy balancing procedure for the extended sample. The mean difference is the simple difference in means. Statistical significance of univariate t-tests of the mean difference at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively. The normalized difference is equal to $(\bar{x}_{tr} - \bar{x}_c) \div \left(\frac{s_{tr}^2 + s_c^2}{2}\right)^{\frac{1}{2}}$ where $\bar{x}_{tr}$ (s_{tr}^2) and $\bar{x}_c$ (s_c^2) denote the pre-adoption sample mean (variance) of treatment and control firms, respectively. The variance ratio is equal to $s_{tr}^2 \div s_c^2$. A full list of variable definitions and data sources is provided in Appendix A.1.

S.2. Robustness Tests: Measuring CPI Score Using Alternative Pre-Adoption Years

Table S.2.1: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – CPI Score Held Fixed as of 2008

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0551***	-0.0592**	-0.0545**	-0.0631***
	(-2.6460)	(-2.4423)	(-2.5803)	(-2.9059)
$Size$	0.0112	0.0498*	0.0209	0.0493**
	(0.5051)	(1.7126)	(0.9770)	(2.1874)
ROA	0.0305***	-0.0154	0.0280***	-0.0252
	(7.2929)	(-0.7240)	(6.2917)	(-1.1961)
$logMB$	0.0202	0.0186	0.0034	0.0078
	(0.8661)	(0.5142)	(0.1386)	(0.2578)
$logSegCount$	0.1219*	-0.0090	0.1245*	-0.0109
	(1.6727)	(-0.1926)	(1.7364)	(-0.2553)
Observations	5,077	5,077	5,070	5,070
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.957	0.893	0.958	0.906

Table S.2.1 reports regression results from specifications presented in Table 5 for the baseline sample over the period 2007–2012, except the CPI score used to calculate CE is held fixed as of 2008 rather than 2009. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry ×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table S.2.2: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – CPI Score Held Fixed as of 2008 (Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0827***	-0.0971***	-0.0861***	-0.1047***
	(-3.3000)	(-3.3410)	(-3.3738)	(-4.0536)
$Size$	0.0244	0.0411*	0.0255	0.0310
	(1.2420)	(1.7963)	(1.2478)	(1.3624)
ROA	0.0191*	-0.0305	0.0179*	-0.0481
	(1.7819)	(-0.8545)	(1.7434)	(-1.3760)
$logMB$	0.0365*	0.0499**	0.0163	0.0316
	(1.8552)	(2.0740)	(0.7884)	(1.3031)
$logSegCount$	0.0846	0.0582	0.0954*	0.0595*
	(1.6343)	(1.3757)	(1.8300)	(1.6795)
Observations	7,905	7,905	7,887	7,887
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry $\times$ Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.946	0.874	0.947	0.885

Table S.2.2 reports regression results from specifications presented in Table 8 for the extended sample over the period 2007–2017, except the CPI score used to calculate CE is held fixed as of 2008 rather than 2009. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry $\times$year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry$\times$year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table S.2.3: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – CPI Score Held Fixed as of 2007

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0469**	-0.0646**	-0.0476**	-0.0681***
	(-2.1865)	(-2.4926)	(-2.1767)	(-2.9900)
$Size$	0.0116	0.0614*	0.0208	0.0573**
	(0.5126)	(1.9150)	(0.9448)	(2.2053)
ROA	0.0311***	-0.0237	0.0285***	-0.0348
	(7.1696)	(-0.9647)	(6.1169)	(-1.4212)
$logMB$	0.0223	0.0130	0.0045	-0.0018
	(0.9463)	(0.3551)	(0.1807)	(-0.0652)
$logSegCount$	0.1055	-0.0189	0.1095	-0.0225
	(1.4209)	(-0.4091)	(1.5055)	(-0.5322)
Observations	5,077	5,077	5,070	5,070
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry $\times$ Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.957	0.897	0.957	0.910

Table S.2.3 reports regression results from specifications presented in Table 5 for the baseline sample over the period 2007–2012, except the CPI score used to calculate CE is held fixed as of 2007 rather than 2009. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry $\times$year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry$\times$year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table S.2.4: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – CPI Score Held Fixed as of 2007 (Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0786***	-0.1093***	-0.0832***	-0.1157***
	(-2.9816)	(-3.3613)	(-3.0990)	(-4.0707)
$Size$	0.0270	0.0534*	0.0273	0.0411
	(1.3380)	(1.9126)	(1.3040)	(1.5222)
ROA	0.0196*	-0.0403	0.0183*	-0.0586
	(1.7692)	(-1.0112)	(1.7232)	(-1.4861)
$logMB$	0.0386*	0.0511*	0.0169	0.0303
	(1.9432)	(1.8566)	(0.8118)	(1.1403)
$logSegCount$	0.0699	0.0426	0.0829	0.0486
	(1.3060)	(0.8746)	(1.5515)	(1.1334)
Observations	7,905	7,905	7,887	7,887
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry $\times$ Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.945	0.873	0.946	0.886

Table S.2.4 reports regression results from specifications presented in Table 8 for the extended sample over the period 2007–2017, except the CPI score used to calculate CE is held fixed as of 2007 rather than 2009. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry $\times$year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry$\times$year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

S.3. Robustness Tests: Excluding Firms in the Business Services Industry

Table S.3.1: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – Excluding Firms in the Business Services Industry

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0665***	-0.0701**	-0.0692***	-0.0771***
	(-2.7353)	(-2.1647)	(-2.7628)	(-2.7558)
$Size$	0.0093	0.0294	0.0246	0.0323
	(0.3493)	(0.6870)	(0.9511)	(0.9728)
ROA	0.0316***	-0.0204	0.0289***	-0.0286
	(7.6511)	(-0.8226)	(6.7656)	(-1.1234)
$logMB$	0.0125	0.0031	-0.0062	-0.0028
	(0.4879)	(0.0544)	(-0.2302)	(-0.0582)
$logSegCount$	0.2151**	0.0220	0.2258**	0.0174
	(2.3152)	(0.2621)	(2.4577)	(0.2881)
Observations	4,499	4,499	4,492	4,492
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.956	0.878	0.956	0.896

Table S.3.1 reports regression results from specifications presented in Table 5 for the baseline sample over the period 2007–2012 after dropping firms in the Business Services industry, which is the most represented industry in the baseline treatment group (53 of 251 firms) and the second-most represented industry in the baseline control group (55 of 694 firms). Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry ×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure on the sample of firms not in the Business Services Industry. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table S.3.2: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – Excluding Firms in the Business Services Industry (Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0851***	-0.0961***	-0.0940***	-0.1115***
	(-2.9256)	(-2.7397)	(-3.1881)	(-3.5490)
$Size$	0.0251	0.0547*	0.0275	0.0401
	(1.0643)	(1.8288)	(1.1019)	(1.2436)
ROA	0.0199*	-0.0385	0.0186*	-0.0561
	(1.7648)	(-0.9196)	(1.7303)	(-1.3071)
$logMB$	0.0312	0.0419	0.0077	0.0213
	(1.3779)	(1.2992)	(0.3233)	(0.6543)
$logSegCount$	0.1214*	0.0694	0.1414**	0.0751
	(1.9446)	(1.1444)	(2.2119)	(1.5852)
Observations	6,969	6,969	6,951	6,951
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.945	0.871	0.947	0.887

Table S.3.2 reports regression results from specifications presented in Table 8 for the extended sample over the period 2007–2017 after dropping firms in the Business Services industry, which is the most represented industry in the extended treatment group (55 of 259 firms) and the second-most represented industry in the extended control group (59 of 692 firms). Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry ×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure on the sample of firms not in the Business Services Industry. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 18 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

S.4. Robustness Tests: Including Post-Adoption UK Segment Firms in the Control Group

Table S.4.1: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – Control Group Includes Firms with a Post-Adoption UK Segment

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0611***	-0.0637**	-0.0632***	-0.0675***
	(-2.8320)	(-2.4862)	(-2.9159)	(-2.9513)
$Size$	0.0122	0.0382	0.0185	0.0378
	(0.5186)	(1.2896)	(0.8363)	(1.6316)
ROA	0.0322***	-0.0144	0.0297***	-0.0224
	(7.5432)	(-0.6717)	(6.5941)	(-1.0663)
$logMB$	0.0257	0.0076	0.0063	-0.0029
	(1.0469)	(0.2070)	(0.2476)	(-0.0990)
$logSegCount$	0.1530**	-0.0043	0.1590**	-0.0006
	(2.0145)	(-0.0828)	(2.1323)	(-0.0128)
Observations	5,181	5,181	5,174	5,174
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.956	0.891	0.957	0.905

Table S.4.1 reports regression results from specifications presented in Table 5 for the baseline sample over the period 2007–2012, except control firms are US firms that disclose at least one non-US country-level segment in the pre-adoption period and do not disclose a UK segment in the pre-adoption period (as opposed to the full sample period). Other sample selection procedures are the same as in the baseline sample. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry ×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 7 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

Table S.4.2: Effect of the UKBA on US Firms' Exposure to Corrupt Countries – Control Group Includes Firms with a Post-Adoption UK Segment (Extended Sample Period)

	(1) Unweighted Sample	(2) Entropy-Balanced	(3) Unweighted Sample	(4) Entropy-Balanced
$Treat \times Post$	-0.0793***	-0.0961***	-0.0858***	-0.1026***
	(-3.1159)	(-3.3726)	(-3.2957)	(-3.9489)
$Size$	0.0203	0.0258	0.0191	0.0152
	(0.9973)	(1.1237)	(0.9035)	(0.6511)
ROA	0.0201*	-0.0182	0.0188*	-0.0358
	(1.8351)	(-0.5565)	(1.7634)	(-1.1432)
$logMB$	0.0389*	0.0412*	0.0186	0.0221
	(1.8805)	(1.9006)	(0.8667)	(1.0608)
$logSegCount$	0.1251**	0.0582	0.1315**	0.0576
	(2.2352)	(1.3328)	(2.4084)	(1.5319)
Observations	8,270	8,270	8,254	8,254
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Industry×Year Fixed Effects	No	No	Yes	Yes
Standard Error Clusters	Firm	Firm	Firm	Firm
Adjusted R^2	0.944	0.870	0.945	0.881

Table S.4.2 reports regression results from specifications presented in Table 8 for the extended sample over the period 2007–2017, except control firms are US firms that disclose at least one non-US country-level segment in the pre-adoption period and do not disclose a UK segment in the pre-adoption period (as opposed to the full sample period). Other sample selection procedures are the same as in the baseline sample. Columns (1) and (2) present results from Ordinary Least Squares (OLS) estimations of Equation 2 with year fixed effects rather than industry ×year fixed effects. Columns (3) and (4) present results from OLS estimations of the main specification (Equation 2). The regressions presented in Columns (2) and (4) are estimated after performing the entropy balancing procedure. Country-year fixed effects are a series of indicator variables equal to one for each country-level segment disclosed by firm i in year t, and zero otherwise. Main effects for $Treat_i$ and $Post_t$ are subsumed by firm and year (or industry×year) fixed effects, respectively. The regressions presented in Column (3) and (4) are estimated after dropping 16 singleton observations, as retaining singleton groups in regressions with multiple levels of fixed effects overstates the statistical significance of the coefficient estimates and is computationally inefficient (Correia 2016). Coefficient estimates for the intercept are untabulated. Robust t-statistics based on clustering standard errors at the firm level are presented in parentheses below the estimated coefficients. A full list of variable definitions and data sources is provided in Appendix A.1. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, *, respectively.

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