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Non-random selection into entrepreneurship in the realm of government decentralization and corruption

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ABSTRACT

For the past few decades, the extant literature on corruption has primarily relied on firm-level survey measures – for example, those from the World Bank – to explore the relevant empirical determinants of this illegal practice. However, these studies have potentially overlooked an underlying econometric problem – namely, non-random selection into entrepreneurship – that may bias all the estimated determinants to date if ignored in the analysis. Here, I assess this possibility by applying the traditional Heckman (1979) correction procedure in a *novel way*: using two different samples. I use my proposed solution in the context of government decentralization and firm-level corruption as a plausible application. Specifically, I revisit the question of the causal impact of government decentralization on firm-level corruption when the underlying sample selection issue is addressed. Results are worth noting. I find reasonable evidence of selection bias. On controlling for this, fiscal decentralization substantially *decreases* firm-level bribery, in general. This finding is in *contradiction* to the results reported by naive estimation strategies where the sample selection issue is completely ignored.

1. Introduction

The United Nations (UN) has a target in its 2030 Agenda for Sustainable Development to significantly reduce all forms of corruption (Goal 16, UN General Assembly, 2015).[2] As an inter-agency task team, the World Bank recognizes corruption as the main hindrance to ending extreme poverty by the target year of 2030 and enhancing the overall well-being of the poorest 40% of the population in developing countries (World Bank Brief, 2021).[3] Corruption is defined as “the abuse of entrusted power for private gain” by Transparency International. It encompasses not only business and government bodies but also civil society, education, health, sports, and many other sectors. Corrupt practices institute a considerable amount of cost to society as a whole and come in various forms: social, economic, political, and environmental.[4]

Given that corruption is such a worrisome phenomenon globally, an extensive literature has long been concerned with finding the empirical determinants of this illicit practice. However, to do so requires an empirical measure. And this is not straightforward,

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[2] See https://www.un.org/en/development/desa/population/migration/generalassembly/docs/globalcompact/A_RES_70_1_E.pdf.

[3] See https://www.worldbank.org/en/topic/governance/brief/anti-corruption.

[4] See https://www.transparency.org/en/what-is-corruption.

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given the very nature of the activity. It is not only challenging to get unbiased data but having its validity unquestioned is equally difficult (Lambsdorff, 2006).

Early studies have heavily relied on *aggregate* measures of corruption either at the country-level (for example, the perception-based index from Transparency International) or the state-level within a country (for example, convictions of public officials in the US from the Public Integrity Section).[5] The country-level perception-based indices provided a means to enhance our understanding of the levels of corruption from one country to another (Lambsdorff, 2006). Simultaneously, within-country measures like convictions of public officials in the US helped capture the specific corruption environment observed in a country that may be considerably different from others. Pointing to this, Lambsdorff (2006, p. 82) states: "[T]he precise legal definition of bribery and corruption can be different in each national context, the differences drawn between bribery, embezzlement and fraud may be troublesome and the statistical methodology of counting and aggregating used in each national agency can differ considerably from that used elsewhere".

These measures were, therefore, extremely valuable in developing the pertinent literature and contributing to our understanding of the crime to a significant extent. Nevertheless, such measures are not ideal and have their own limitations. They contain substantial measurement errors and do not vary much over time (Treisman, 2007; Choudhury, 2021). Moreover, the number of countries included, the total number of survey sources used, and the methodology applied to construct these measures generally change over the years. For example, the corruption perception index (CPI) used seven survey sources in 1995, with 41 countries in the sample. By 2018, CPI surveyed 180 countries with 13 different sources to construct the index.[6] Additionally, because of a methodology change in 2012, CPI scores before this year are not comparable to that of later years.[7] Besides these, controlling for unobserved heterogeneity in cross-country or cross-state analysis while using such aggregate measures becomes difficult for empirical researchers.

Following this, various units within the World Bank started conducting firm-level surveys in the 1990s to assess the business environment across the globe. These surveys provided us with experience-based corruption measures for the first time. From 2005–06, a more consolidated approach towards this effort marked the beginning of Enterprise Surveys (ES) to report corruption measures at the *firm level*.[8] With this newly available measure, a large number of studies started exploring the empirical determinants of corruption afresh. This new disaggregated measure led to a deeper understanding of the relationships between corruption at the firm level and various other economic variables, thereby enabling researchers to study the phenomenon in detail. To be more specific, these data sets have been used in more than 564 journal articles, reports, and books between 1999 to 2021 – including corruption-focused studies – according to the official records of the World Bank.[9]

While this new firm-level measure offers distinct advantages over the previous ones, it comes with its unique problem: *selection into entrepreneurship*. To the best of my knowledge, this issue has not been acknowledged so far, let alone addressed. If this issue indeed exists, as I comprehend, then ignoring it in the analysis may lead to an incorrect estimation of the parameters, thereby producing wrong inferences about the significance of the independent variables (Tonoyan et al., 2010). Under such a circumstance, all the empirical studies using the firm-level corruption measure from ES up to now as the outcome variable would bear the possibility of being erroneous in their implications. But before moving further with the details of this potential concern, it is helpful first to briefly establish the connection between firm-level corruption and entrepreneurial effort.

The most widespread opinion in this context is that corruption facilitates the entry of nascent entrepreneurs into a market, particularly when the quality of institutions is substandard. For example, when the entry regulations are intensive, offering bribes to government officials may help get away with some regulatory requirements. Supporting this, Damania et al. (2004) argue that weak institutions can actually create an extremely lucrative environment for bureaucratic corruption at a lower level. In this respect, Dreher and Gassebner (2013, p. 416) also state: "Despite the overall impact of corruption on growth being negative, it may still promote entrepreneurial activity which has been suppressed by rigid regulations". In contrast to this popular belief, extant theories put forth an alternate viewpoint that is totally conflicting in nature (Myrdal, 1968; Shleifer and Vishny, 1993). In this school of thought, corruption is almost not ever helpful for entrepreneurship, even in the presence of an adverse business climate (Dutta and Sobel, 2016). Against this backdrop, empirical studies have not been able to provide any conclusive evidence either; rather, they reported contradictory results. Several studies find evidence favoring each of these two arguments; some even find mixed results (for example, Hanousek and Kochanova, 2016; Zakharov, 2019). I discuss these arguments and evidence in detail in Section 2.

For now, suppose one believes that corruption facilitates the entry process for nascent entrepreneurs in a market. In that case, a successful entry is contingent on how corrupt these entrepreneurs are and their willingness to pay bribes to initiate a start-up process. So, one may presume this willingness to be a crucial determinant of participation in entrepreneurship. Alluding to this, Dreher and Gassebner (2013, p. 427) state: "While it seems reasonable to assume that corruption...are...exogenous to the entrepreneur's decision to enter the market in the short run, this might not be true in the longer term". This possibility then makes the selection into entrepreneurship *non-random*. Reinforcing this reasoning, Boudreaux et al. (2018) explain that entrepreneurs' corruptibility may alter the very structure of an economy when they find it more beneficial to reallocate resources to those areas that yield higher profits through corrupt activities. Moreover, entrepreneurs are expected to participate in corrupt activities readily when their prevailing culture, norm, and surrounding social structure justify their actions as a matter of course (Tonoyan et al., 2010).

[5] See https://www.transparency.org/en/cpi/2020 and https://www.justice.gov/criminal-pin/annual-reports. See, for example, Goel and Nelson (2011), Choudhury (2021) for applications of US conviction data.

[6] See https://www.transparency.org/files/content/pages/2018_CPI_FAQs_EN.pdf.

[7] See http://transparency.org.my/filemanager/files/shares/CPI-Frequenty-Asked-Questions.pdf.

[8] See https://www.enterprisesurveys.org/content/dam/enterprisesurveys/documents/methodology/Enterprise-Surveys-Manual-and-Guide.pdf.

[9] See "ES-research-used-in-different studies" on the website: https://www.enterprisesurveys.org/en/enterprise-research.

Table 1
Comparison of data structures.

Ideal data structure		Less ideal data structure		My case			
Without sample selection issue		Traditional Heckman (1979) selection model		'Two-sample Heckman selection' approach			
(One sample)		*(One sample)*		*(First sample: individual level)*		*(Second sample: firm level)*	
E	C	E	C	E	C	E	C
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	√	Y or N	.	Y	√
Y or N	√	Y or N	.	Y or N	.		
Y or N	√	Y or N	.	Y or N	.		
Y or N	√	Y or N	.	Y or N	.		
Y or N	√	Y or N	.	Y or N	.		
Y or N	√	Y or N	.	Y or N	.		

Note: E stands for entrepreneurial effort taking the observations of Yes or No, as represented by Y or N, respectively. C stands for corruption experience where we observe data for √ and . represents missing observation.

Based on all this, it is fair to presume that potential entrepreneurs *self-select* themselves into entrepreneurial activity contingent on their preference, perspective, and environment to pay bribes to public officials. This, therefore, builds a strong case for the underlying sample selection issue here. It is essential to bear in mind that the firms we observe in various corruption-related surveys to collect firm-level data basically represent the sub-population of entrepreneurs: the successful entrepreneurs, to be precise. So, if successful entrepreneurship is genuinely an outcome of bribery, these firms are likely to suffer from selection bias. The unobservables that affect successful selection into entrepreneurship are likely to be correlated with the unobservables that affect an entrepreneur's decision to engage in bribery. This is mainly because the decision to pay bribes is expected to impact the chances of being a successful firm in the end. Thus, ignoring the sample selection issue in any such empirical analysis is expected to bias all estimated determinants of firm-level corruption.

To summarize, if the concern about sample selection is real and matters at all, then all the extant studies using the ES data to find empirical determinants of firm-level corruption are potentially flawed. The objective of this study is to assess this possibility. In this course, I reanalyze the impact of an economic variable – namely, government decentralization, that I discuss shortly – on firm-level corruption once the bias is controlled for. At the risk of sounding repetitive, it is worth highlighting that the concern raised here is relevant to *all* empirical investigations that involve bribery of firms observed in various corruption-related surveys as the dependent variable. In this regard, my current study makes a contribution to the literature on corruption in general and firm-level corruption in particular. If the posed concern is ultimately found to carry some truth, the prior estimates of various economic variables on firm-level bribery would not only be biased, but previous claims of causality would also be questionable. Thus, evaluating the potential problem is genuinely warranted.

While the concern is reasonable and remains unaddressed in the extant literature, conducting an analysis to control for this sample selection issue is not straightforward. The traditional solution for controlling such selection bias is the Heckman (1979) correction procedure. This solution conventionally requires information on the outcome of interest (corruption in the current context) *and* the selection equation's outcome (entrepreneurial efforts in the present context) in the *same* data set. However, there are no firm-level corruption data that contains information on all entrepreneurial efforts (nascent- and non-entrepreneurs). Instead, ES data have only successful entrepreneurs, as explained above. Hence, implementing Heckman (1979) in its *usual* way is not a viable option in the current context.

Table 1 provides a visual representation of the problem as a reference point for my readers. The three columns represent the three possible data structures, respectively, that a researcher may encounter while carrying an analysis. The first column, "Ideal Data Structure" captures the situation of an absence of a sample selection issue. Hence, all the observations have information on both entrepreneurial effort (E) and corruption experience (C). Entrepreneurial effort (E) can either be a "yes" (represented by Y) or a "no" (represented by N) for an observation. The second column, "Less Idea Data Structure" is the case where there is a sample selection concern, but the traditional Heckman (1979) correction procedure can be implemented readily. Here, a researcher has information on the entrepreneurial effort (E) for all observations. However, only a sub-sample, i.e., the first few observations in the table, contains information on corruption experience (C), while missing for the remaining. Thus, a single data set containing information on both E and C is available to apply the correction technique conventionally.

In contrast to these, the third column, "My Case" represents the unique situation relevant to my context. Here, there is a sample selection issue, but the traditional Heckman (1979) correction procedure is not a viable option. This is because there is no single data set containing information on both the entrepreneurial effort (E) and corruption experience (C) for all observations. Instead, only entrepreneurial effort (E) is observed in one sample of adult individuals with no information on corruption experience (C). And, corruption experience (C) is observed only for firms in a separate sample where these firms are already successful entrepreneurs, as depicted by Y for entrepreneurial effort (E).

Given this impediment, I propose to solve the problem at hand by applying the standard Heckman Selection (1979) model in a *novel way*. As Klevmarken (1982, p. 12-13) states: "Future research based on microdata might have to rely more and more on the kind of incomplete data discussed in this paper. To be able to do this, we will need some vehicle, a model, which links the variables of the different data sets". To this end, I use *two separate samples* — experience-based corruption measures from ES and nascent entrepreneurship measures from Global Entrepreneurship Monitor (GEM) — to obtain a new 'two-sample Heckman selection estimator.'

As a cursory reference, my implementation strategy is analogous to two-sample-two-stage least-squares estimation (TS2SLS). The main idea was actually introduced by Angrist and Krueger (1992) as a two-sample instrumental variable (TSIV) estimation. The essence of this methodology is that under certain conditions, the traditional instrumental variable approach can address the potential endogeneity of a regressor in a structural equation even when the outcome variable and the regressor belong to two different samples. However, because of computational feasibility, TS2SLS gained popularity over TSIV among empirical researchers (see Inoue and Solon, 2010; Pacini and Windmeijer, 2016). I discuss these and my implementation strategy in detail in Section 3. But at this point, it is worth noting that my proposed solution here is applicable to all those empirical studies that may lack one comprehensive data set to implement the Heckman (1979) correction strategy in a conventional way. Specifically, any researcher running into the situation represented by the third column, "My Case" of Table 1, has the possibility of availing of this suggested route. This is where I believe the second contribution of my study comes in: an extension of Heckman (1979) correction technique to handle a unique data situation.

Now returning to the point of reanalyzing an economic variable in the process, I use government decentralization as a plausible area of application. Government decentralization refers to the distribution of power among the various levels of a country's government: central, state, and local. This is known to impact the rent extraction behavior of public officials in several conflicting ways. On the one hand, greater decentralization of power may bring public officials and the people close to each other and make the former more accountable (Fan et al., 2009). On the other hand, it may disrupt coordination among officials themselves and eventually increase the incentive and opportunities to extract bribes (see, for example, Monte and Papagni, 2007).

I choose government decentralization as the main explanatory variable of interest here for two reasons. First, this is an extensively discussed topic with substantial evidence of government decentralization affecting corruption. Readers are referred to Fan et al. (2009) for a good review on this wide-ranging subject. Second, prior empirical studies report government decentralization to 'causally' impact firm-level corruption experience (Choudhury, 2015). However, as illustrated earlier, these studies have failed to consider the potential sample selection problem. So, revisiting the question of the 'causal impact' of government decentralization on firm-level corruption while accounting for the underlying selection issue strikes as a credible point of investigation.

The results are noteworthy. I find reasonable evidence of selection bias, thereby confirming the decision to participate in entrepreneurship to be non-random. Once this bias is controlled for, government decentralization of a country *decreases* firm-level corruption in general by a significant amount. To be specific, greater fiscal decentralization, that is, a greater share of revenue to the sub-national government, decreases informal gifts by firms to officials while securing a government contract. This decrease is statistically significant and quite substantial in terms of economic significance. In contrast, naive estimation strategies report conflicting signs, magnitude, and statistical significance when the underlying sample selection problem is completely ignored. My main finding reinforces the need to consider sample selection issues while conducting a causal empirical analysis with firm-level corruption as the outcome of interest. Failing to do so is necessarily going to yield incorrect inference. I discuss the implications of my results in detail in Section 5.

To help comprehend my analysis better, I organize the paper in the following way. Section 2 briefly reviews the related literature. Sections 3 and 4 discuss the empirical methodology and data used, respectively. Section 5 presents the results. Finally, Section 6 concludes.

2. Related literature

Decades back, Leff (1964, p. 10) stated: "[G]raft can provide the direct incentive necessary to mobilize the bureaucracy for more energetic action on behalf of the entrepreneurs". Concurring with the author, Nye (1967, p. 420) said: "Corruption may provide the means of overcoming discrimination against members of a minority group, and allow the entrepreneur...to provide his skills. In East Africa...corruption may be prolonging the effective life of...the Asian minority entrepreneur...beyond what political conditions would otherwise allow". Thereafter, extensive research has been done to understand the relationship between corruption and entrepreneurship. So, I first briefly discuss the theoretical arguments prevalent regarding this relationship. Then I present empirical evidence available in the literature. Finally, I review the linkage between corruption and government decentralization as this is the area of application for addressing the underlying concern of sample selection.

2.1. Corruption and entrepreneurship

The extant literature describes the relationship between corruption and entrepreneurship as "complex" and based on "context" (Mohamadi et al., 2017, p. 50). A considerable number of studies find evidence of corruption being beneficial for nascent entrepreneurs, especially in the presence of stringent entry regulations in a market (Dreher and Gassebner, 2013; Bologna and Ross, 2015). However, others support corruption as detrimental to entrepreneurship (Méon and Sekkat, 2005; Freund et al., 2016).

To elaborate, the favorable impact of corruption on start-up efforts is embedded within the well-known "grease the wheels" hypothesis. The argument was proposed by Leff (1964), Huntington (1968), and Leys (1965). Incompetent bureaucracy and poor institutional quality cause hindrance to investments. In such an instance, bribes provide the necessary "grease" to overcome the inefficiency and improve investment and overall growth of a country. In this case, bribes work as a "trouble-saving device" that overcomes the difficulties associated with starting a business (Méon and Sekkat, 2005, p. 70). Levin and Satarov (2000, p. 115) report that small entrepreneurs in the Russian Federation during the post transition years paid millions of dollars as bribes to public officials on a monthly basis: approximately "10% of total revenues in small- and middle-size businesses". It is not entirely the corruptibility of officials who are the bribe-seekers that is responsible for such transactions. But, that of entrepreneurs as bribe-payers also play a key role. In this respect, Tonoyan et al. (2010, p. 824) argue that an entrepreneur is likely to engage in corrupt behavior in transition countries because of the deeply rooted culture and norms prevalent in the society that "justify that 'if others behave illegally, so can I' Lefebvre (2001)".

The contrasting theory – known as the "sand the wheels" hypothesis – was put forward by several researchers like Myrdal (1968), Shleifer and Vishny (1993), Bardhan (1997), and Jain (2001), to name a few. This suggests that corruption, in effect, is harmful to entrepreneurship. Corrupt public officials deliberately delay the whole process to extract rents. Moreover, if several agents are involved, the entire procedure becomes expensive. Subsequently, "one distortion adds up to the others instead of compensating them" (Méon and Sekkat, 2005, p. 73). As a consequence, corruption discourages entrepreneurial activity overall. An interesting point to note here is that the crux of both the arguments – which are conflicting in nature – is the very presence of low-quality governance in a country. Now whether corruption in combination "grease the wheels" or "sand" them is a debatable question.[10]

Empirical analyses, as mentioned earlier, present an ambiguous picture as well. Dreher and Gassebner (2013, p. 425) find corruption to "function as efficient grease" to entrepreneurial efforts in the presence of strict regulations. Bologna and Ross (2015, p. 61), although they find corruption to "generally discourage business activity", present evidence "consistent with the 'grease the wheels' hypothesis" when the institutional quality is low. In contrast, Freund et al. (2016) report a positive association between requests for bribes and delays in the process. Méon and Sekkat (2005) also find evidence favoring the "sand the wheels" hypothesis. Using LinkedIn data on entrepreneurial activity for 176 countries, Avnimelech et al. (2014) also find that countries with high levels of corruption experience low levels of productive entrepreneurship, and this negative effect is more pronounced in developed countries than developing nations. Dutta and Sobel (2016, p. 179) make an interesting statement in this context: "[C]orruption never improves entrepreneurship; it simply hurts less when business climates are not conducive to growth in the first place".

In addition to these, some studies find mixed evidence supporting both hypotheses. Chowdhury et al. (2015) find corruption to both "grease" and "sand" international entrepreneurial activity, depending on the types of tax and export costs prevalent in a country. Likewise, Berdiev and Saunoris (2018) report that corruption is detrimental to entrepreneurship in the formal sector, but it promotes entrepreneurial activity in the informal sector. Regardless of the precise role of corruption in this context, its very presence is likely to influence the decision of (not) entering the market. Endorsing such a possibility, Banerjee et al. (2015, p. 43) demonstrate in a lab experiment that corruption is an element of "occupational sorting". Corrupt people self-select in bureaucracy due to higher opportunities to extract rents than those available in private jobs. Along similar lines, Monte and Papagni (2001, p. 2) state: "Corruption can increase the ability of agents to get resources from central and local governments. Therefore, public resources reward the more "able" people, not the best entrepreneurs". Thus, the prevalence of corruption may screen out honest entrepreneurs and attract those who have a greater propensity to engage in bribery.

Recently, Anguera-Torrell (2020, p. 3) demonstrates with a theoretical model that "the entrance of undesirable producers" in the market "crowds out" the good producers. Empirically, Choudhury (2019) provides some support for this argument. The author finds evidence of a compositional change in the types of entrepreneurs entering the market after a country becomes a formal member of the World Trade Organization (WTO). Firms entering the market after a country becomes a formal member of the WTO are likely to be more bribe-prone than those existing prior to the formal membership status. Similarly, Boudreaux et al. (2018, p. 181) find that resources are re-allocated within the U.S. economy to those industries that are "better situated to profit from corruption". For example, in favor of the infrastructure sector and away from education and non-profit organizations.

Given all of the above, the concern about the selection into entrepreneurship being non-random in nature strikes reasonable. And if this is true, then not controlling for it in the empirical analysis is likely to bias the estimates.

2.2. Corruption and government decentralization

A large literature focuses on unraveling the multi-faceted relationship between corruption and government decentralization. As Monte and Papagni (2007, p. 381) point out, the "relationship is not clear". Referring to this, Fan et al. (2009, p. 32) also state: "Given the complicated, interacting effects that theorists have posited, it seems quite unlikely *a priori* that there exists a simple, general relationship between decentralization and corruption that holds in different contexts and geographical settings".

As such, researchers find each form of government decentralization to impact the corruption level of a country differently. Political decentralization generally captures power distribution within a government in terms of the number of administrative tiers, the average size of the bottommost tier, or simply the federal structure (for example, Treisman, 2000). Fiscal decentralization, on the other hand, represents the share of government revenue or government expenditure at the subnational level (for example, Fisman and Gatti, 2002). As mentioned earlier in Section 1, with greater power in the hands, accountability of public officials at the subnational level may go in either direction, depending on the environment and circumstances. For example, if the competition is

[10] Readers are referred to Méon and Sekkat (2005) and Chowdhury et al. (2015) for good reviews on the "grease the wheels" versus "sand the wheels" hypotheses and the corresponding empirical evidence.

intense among governments to attract resources and local businesses, it may make the officials more disciplined and honest in the interest of jurisdictional growth and development. Alternately, it may induce greater incentives and opportunities to extract rents. Fan et al. (2009) provide a good review of all the associated theoretical arguments.

Empirically, political decentralization tends to increase bribery, overall, in cross-country analysis. On the contrary, fiscal decentralization has a curbing impact (Fan et al., 2009; Choudhury, 2015). In the case of a specific country analysis, the results are mixed. Goel and Nelson (2011, p. 471), for example, find government decentralization per se to not "necessarily reduce corruption" in the U.S., but it is "the type of decentralization" that actually matters. For Italy, Monte and Papagni (2007) find corruption to increase with more power transfers from central to local governments.

In short, despite the contradictory findings, prior studies provide substantial evidence of government decentralization impacting corruption significantly. One can, therefore, ascertain the valuable contributions of these existing studies in bringing to the fore the connection between these two very complicated phenomena. However, as one shortcoming, those studies that use firm-level corruption measures fail to take into account the potential problem of sample selection. Ignoring this in the analysis may bias all the reported impacts. Considering this, revisiting the question of how decentralization causally affects firm-level corruption seems tenable.

3. Empirical strategy

I first explain the sample selection issue in the current context, followed by the traditional solution to control for such bias. I then discuss the difficulty of implementing the solution here in a conventional way. Finally, I present my preferred strategy to overcome the problem.

3.1. Sample selection issue

The 'true' model to estimate the causal impact of government decentralization on firm-level corruption is given by Eq. (1).

$$C^*_{ij} = \alpha + \beta D_j + X_j\theta_1 + X_{ij}\theta_2 + \epsilon_{ij}, \tag{1}$$

where C^*_{ij} is a latent measure of corruption. The subscripts, i and j, indicate firm and country, respectively. D_j is the decentralization measure. X_j is a vector of country-level observable attributes, and X_{ij} is a vector of firm-level observable attributes that impact C^*_{ij}. θ_1 and θ_2 are conformable vectors of parameters. My parameter of interest is β. Recall, I have government decentralization (D_j) as the explanatory variable of interest merely as an application. This choice can fundamentally vary from researcher to researcher based on one's question of interest. But regardless of what the primary explanatory variable is, the necessity to control for potential non-random selection into entrepreneurship is relevant for all empirical studies concerning firm-level corruption C^*_{ij} as the outcome.

So moving on to this plausible concern, selection bias arises in this model because we do not observe C^*_{ij}. Instead, we observe C_{ij} because a firm's corruption is only reported if it becomes a *successful* entrepreneur. For that, it must select to be an entrepreneur in the first place. Both these aspects potentially depend on the willingness to pay a bribe (explained before). The following equations formally express this whole idea.

$$\begin{aligned} C_{ij} &= C^*_{ij} && if\ E_{ij} = 1 \\ &= . && if\ E_{ij} = 0 \quad \text{and,} \\ E_{ij} &= 1 && if\ E^*_{ij} > 0 \\ &= 0 && if\ E^*_{ij} \leq 0, \end{aligned} \tag{2}$$

where

$$E^*_{ij} = \pi_0 + \pi_1 D_j + X_j\pi_2 + X_{ij}\pi_3 + ER_j\pi_4 + \mu_{ij}. \tag{3}$$

E_{ij} and E^*_{ij} are observed and latent measures of entrepreneurial effort, respectively. E^*_{ij} is determined by the same set of observable country-level (D_j and X_j) and firm-level (X_{ij}) attributes as that of the outcome of interest, C^*_{ij}. However, it is also determined by a specific country-level attribute – represented by ER_j – that otherwise does not have any direct effect on C^*_{ij}. ER_j plays the key role in identifying the model that I discuss in the next section. Traditionally, (1) is referred as the outcome equation and (3) as the selection equation which can now be re-written as the following:

$$C_{ij} = \alpha + \beta D_j + X_j\theta_1 + X_{ij}\theta_2 + \epsilon_{ij}, \tag{4}$$

$$E_{ij} = \pi_0 + \pi_1 D_j + X_j\pi_2 + X_{ij}\pi_3 + ER_j\pi_4 + \mu_{ij}, \tag{5}$$

where ϵ and μ are mean zero, *correlated* error terms with covariance $\sigma_{\epsilon\mu}$. Hence, naive estimation of (4) will yield a biased and inconsistent estimate of β.[11]

[11] Although a panel data structure is generally more appealing for an empirical investigation, there is a considerable lack of data on firm-level corruption for the same countries over time with matching entrepreneurial and decentralization measures. Thus, a cross-sectional analysis is the best option at present to address the question of interest. A future study maybe in line as and when a panel data setting is feasible.

3.2. Traditional solution

The above problem of bias is traditionally solved by implementing the Heckman (1979) procedure. In the first stage, the selection Eq. (5) is estimated by probit using the entire set of observations – that is, the full sample – to obtain the linear prediction, denoted by $\widehat{Z}_{ij}$ (see Heckman, 1979, p. 156-157).[12] Then the standard Inverse Mill's ratio (IMR) is constructed, defined by (6).[13]

$$\widehat{IMR}_{ij} = \frac{\phi(\widehat{Z}_{ij})}{1 - \Phi(\widehat{Z}_{ij})} \tag{6}$$

where $\phi(.)$ and $\Phi(.)$ represent the corresponding standard normal probability density function (PDF) and cumulative distribution function (CDF), respectively. Following this, the outcome Eq. (4) is augmented with $\widehat{IMR}_{ij}$. This is given as follows:

$$C_{ij} = \alpha + \beta D_j + X_j\theta_1 + X_{ij}\theta_2 + \delta\widehat{IMR}_{ij} + \varepsilon_{ij} \tag{7}$$

where $\delta = \frac{\sigma_{\epsilon\mu}}{\sqrt{\sigma_\mu}}$ and σ_μ is the variance of the first stage error term.[14] This Eq. (7) is then estimated in the second stage by standard Ordinary Least Square (OLS) using the specific sub-sample where observations for the outcome – C_{ij} in this case – are available. The estimate of β is, therefore, unbiased and consistent in Eq. (7).

Though ε_{ij} is now a well-behaved error term in (7), it is heteroskedastic in nature due to the presence of the regressors in $\widehat{IMR}_{ij}$. Computing usual heteroskedasticity-robust standard errors also do not yield the correct standard errors. This is because the value of IMR is actually unknown, and instead, an estimated value is used (see Heckman, 1979). Given this, correct standard errors are obtained via bootstrap, clustered by country to allow errors to be correlated among firms within a country but not across countries.

Before I discuss the difficulty in replicating the above steps *in the exact manner* in my study, it is important to revisit the intuition behind this strategy. The sample selection issue is essentially an "omitted variable" problem (Heckman, 1979, p. 153, 155). The above correction is a "two-stage" method that solves the problem by adding $\widehat{IMR}_{ij}$ in the outcome equation (Heckman, 1979, p. 153). $\widehat{IMR}_{ij}$ serves as the additional regressor in (7) that controls for the bias.

To fix ideas, this IMR is a potential determinant of the outcome: firm-level corruption in the current context. However, it gets omitted from the "selected sample" where the outcome is observed. To make things worse, this IMR is unknown (Woolridge, 2002, p. 563). Hence, it is estimated and then added to the outcome equation. So, how is it estimated? The "full sample" that contain observations on the selection equation's outcome – entrepreneurial effort (E_{ij}) in this case – is used to estimate the IMR for the "selected subsample": the sub-sample containing observations for firm-level corruption (C_{ij}) (Heckman, 1979, p. 157). Putting forth the argumentation, Woolridge (2002, p. 564) states: "Obtain the probit estimate...using all N observations. Then, obtain the estimated inverse Mills ratio...(at least for $i = 1, \ldots, N_1$)" where N_1 represents observations on the outcome.

In short, using all the information available for the selection rule — given by (5) — we make predictions, that is otherwise not observed for the selected sample – given by (4). This is then added to the outcome equation to mend for the omitted variable. This is formally represented by Eq. (7).[15] Further, the selection equation must have exclusion restrictions to implement this correction procedure successfully. This is given by ER_j in (5). Intuitively, ER_j is an exogenous characteristic that has no direct impact on the outcome C_{ij} but is likely to influence it through E_{ij} only, thereby identifying the model.

3.3. Implementation difficulty

Now, the "convention...adopted" in the Heckman (1979, p. 154) procedure is to use *one common data set*, where the selection rule is observed for all subjects, but the outcome of interest is reported for the first few.[16] Hence, both the selection and outcome equations are estimated using one single sample. However, given the specific question at hand, it is practically impossible to observe entrepreneurial effort (E_{ij}) and firm-level corruption (C_{ij}) in the same data set. Actually, no such data set exists currently, to the best of my knowledge. While entrepreneurial effort is questioned and reported in individual-level surveys like GEM, corruption experience is covered only in firm-level surveys where these firms already represent the group of successful entrepreneurs, like ES. Thus, the standard solution to the sample selection problem is not *directly* applicable in the current analysis.

Having said that, the intuition for Heckman (1979) correction remains valid even in the current context. A sample that has information on entrepreneurial effort for all potential subjects represents the "full sample", as per the basic concept in Heckman (1979, p. 157). Likewise, the reported corruption experience of firms reflects the "selected sample" where the outcome of interest is observed. Thus, I propose my preferred strategy in the next section.

[12] Here $Z_{ij} \equiv Z_i$ of Heckman (1979, p. 156).
[13] Here $IMR_{ij} \equiv \lambda_i$ of Heckman (1979, p. 156).
[14] Here $\delta \equiv C$ of Heckman (1979, p. 157).
[15] See Heckman (1979, p. 156) for details on Z_{ij}.
[16] This is referred to as "$I_1 < I$" in Heckman (1979, p. 154). Also, see, for example, Vella (1998), Mulligan and Rubinstein (2008).

3.4. Preferred strategy

Because of the unavailability of one common data set on entrepreneurial effort and corruption, I use two different samples instead to conduct my analysis. Data on entrepreneurial effort of individuals is the first sample that essentially represents the "full sample" containing observations for all potential subjects (nascent entrepreneurs and non-entrepreneurs). Data on corruption experience of firms represents the second sample that represents the "selected subsample" containing observations on firm-level corruption. In order to be precise, I re-write the selection and outcome equations as follows, respectively:

$$E_{1sj} = \pi_0 + \pi_1 D_j + X_j \pi_2 + X_{1sj}\pi_3 + ER_j \pi_4 + \mu_{1sj}, \tag{8}$$

$$C_{2ij} = \alpha + \beta D_j + X_j \theta_1 + X_{2ij}\theta_2 + \delta \widehat{IMR}_{2ij} + \varepsilon_{2ij}, \tag{9}$$

where $1s$ indicate individual s from the first sample and $2i$ refers to firm i from the second sample, and j continues to indicate the country. Following the main argumentation of the Heckman (1979) procedure, I estimate the selection equation – given by (8) – using the first sample. From the information available in the first sample, I get the linear prediction $\hat{Z}_{2ij}$ for the second sample and construct the IMR. Thus, $\widehat{IMR}_{2ij}$ has $2i$ as the subscript, even though it is a function of characteristics with $1s$ as the subscript.[17] It plays the role of the (omitted) determinant for the successful firms observed in the second sample. It is predicted based on the observable determinants of entrepreneurial effort in the first sample (D_j, X_j, X_{1sj} and ER_j). Recall, both D_j and X_j are observable country-specific characteristics. Hence, they are common to both samples. However, X_{1sj} is a vector of individual-specific characteristics observed in the first sample. So, I find comparable firm-specific attributes X_{2ij} that are observed in the second sample, like ownership status, sector, and size in terms of employees. Thus, $X_{1sj} \equiv X_{2ij}$.[18] Eq. (9) is then estimated via OLS to get the unbiased and consistent estimate of β.[19]

Principally, my empirical strategy follows the Heckman (1979) correction method, but the implementation is influenced by the TS2SLS estimation. The TS2SLS method is the traditional two-stage least-squares procedure to address endogeneity in the structural equation due to the *presence* of an endogenous regressor in the model. However, the endogenous regressor and the outcome of interest belong to two different samples. Hence, two different data sets are used in the correction process (Inoue and Solon, 2010).

As opposed to this, endogeneity in the sample selection case is because of an omitted variable in the structural equation representing the "selected sample". As explained earlier, this omitted variable is predicted using all possible information on all potential subjects, and this is usually available in the same sample. However, this is available in a different sample for my case, given the unique data structure. So, I take a cue from the above methodology and implement the Heckman (1979) correction to address the sample selection concern in the current context. I, therefore, term my unconventional approach as the 'two-sample Heckman selection method.' While this methodology may not be ideal, the current analysis cannot be performed any better, given the specific question of interest.

4. Data

As explained, I pool data from multiple sources. Definition of the variables and their sources are reported in Table A.1 of the Appendix. Summary statistics are presented in Table 2. I briefly mention the variables here for an overview. Corruption variable C_{2ij} is a firm-level experience-based measure from ES: my second sample. I use two alternate measures: informal payments and contract payments. While the former refers to the percent of total annual sales paid as informal payments, the latter is the percent of contract value paid as informal gifts to the government to secure a contract. To shed some light on the main characteristics of this sample, almost 56% of the sample considers an "inadequately educated workforce" an obstacle to some degree to their business. After dropping 4.6% of the sample with missing observations, around 36% of the remaining firms are found located in a city with a population of over one million, and only 17% are in an area with less than 50,000 people. Only 48% of the total sample report their ownership category. Out of that, approximately 64% are "all-men-owned", and only 10% are "all-women-owned", whereas only 6% have "equally divided" ownership. Again, only 40% of all firms provide information about the education status of their female employees. Of that, approximately 37% have female employees with more than 12 years of education.

Now, one may question the choice of data here and argue in favor of perception indices like that from Transparency International. But, recent studies uphold that perception-based measures are likely to have more measurement errors, and experience-based measure is necessarily an advancement in the literature (Treisman, 2007; Razafindrakoto and Roubaud, 2010). Having said that, experience-based measures are not flawless either. In this context, Fan et al. (2009, p. 15) state: "Of course, no approach is completely

[17] For clarification: Eq. (8) is estimated using the first sample to get the predicted IMR *for the firms* observed in the second sample. So, although IMR is a function of characteristics with subscript $1s$, it is still a prediction for firms in the second sample. It is then added to Eq. (9), and then this equation is estimated using the second sample. Thus, it is reasonable to have the subscript $2i$ for the predicted IMR in Eq. (9) to denote that this IMR is that additional regressor for the firms observed in the second sample.

[18] To compare some summary statistics between the two samples, I discuss the difference in mean and the corresponding standardized mean difference of all the control variables in the next section.

[19] Note, the basic model of Heckman (1979), presented before by Eqs. (5) and (7), remain the same in my preferred strategy, given by (8) and (9). The only difference is in the notation that reflects the two different *sources* from where I get my outcome of interest versus where I observe my selection equation's outcome. Pooling these two different sources does not alter the fundamental setup and underlying assumptions. Given this, the properties of the Heckman (1979) estimator, unbiasedness and consistency, are likely to carry over to my current setting.

Table 2
Summary statistics.

Variables	N	Mean	SD	Min	Max
Dependent Variable: Outcome Equation					
Corruption: Informal Payments	15533	0.74	5.37	0	100
Corruption: Contract Payments	3097	1.81	6.89	0	100
Dependent Variable: Selection Equation					
Nascent Entrepreneurship (dummy)	126119	0.22	0.41	0	1
Main Explanatory Variable					
Fiscal Decentralization: Tax	126119	0.13	0.13	0.00	0.49
Fiscal Decentralization: Revenue	116670	0.14	0.14	0.01	0.61
Country Specific Characteristics					
GDP per Capita (log)	126119	9.78	0.54	7.60	10.76
Government Revenue	126119	21.44	4.54	11.14	28.80
Imports	126119	36.60	18.42	20.19	88.01
Voice and Accountability	126119	0.54	0.18	0.18	0.92
Fuel Exports	126119	14.80	21.34	0.56	70.29
Population (log)	126119	17.41	1.55	14.09	21.02
Firm Specific Characteristics					
Ownership Status (dummy)	33708	0.36	0.48	0	1
Sector (dummy)	33068	0.40	0.49	0	1
Size (dummy)	28973	1.62	0.75	1	3
Exclusion Restriction					
Firm Entry Regulation: Procedures (log)	126119	2.13	0.36	0.69	2.64
Firm Entry Regulation: Time (log)	126119	2.95	0.65	1.79	4.41
Firm Entry Regulation: Cost (log)	123847	2.17	1.08	−0.55	4.34
Firm Entry Regulation: Capital (log)	94474	1.89	2.18	−5.62	4.61

Notes: Variable definitions and their sources are presented in Table A.1. Countries forming the sample and their corresponding year of data used in the analysis are reported in Table A.2. Firm specific characteristics refer to the common characteristics found among the nascent entrepreneurs in the Global Entrepreneurship Monitor (GEM) dataset and firms observed in the Enterprise Surveys (ES) for collecting corruption related data.

without problems; it is possible that questions that focus more closely on managers' direct experience with corruption might not be answered with complete frankness for fear of some kind of self-incrimination. However, we believe this danger is less serious than the danger that bias will creep into the assessments of "experts" and foreign businessmen because of inconsistencies in media coverage".

Given this, firms' direct experience still stands up as a better choice to explore the relevant topic of corruption and government decentralization in a cross-national setting. Moreover, the specific concern of non-random selection into entrepreneurship is relevant to *firm-level* corruption only. Perception-based measures indicate an overall level of corruption from an expert's perspective, who may or may not be a resident of the country in question. These measures may suffer from other biases that extant studies have pointed out, but not this specific selection bias. Thus, the very objective of my current research is taken away if the perception indices are used as an alternate measure. Besides, as Fan et al. (2009, p. 1) appropriately point out: "A number of scholars have sought to answer this question empirically by looking for relationships between measures of political or fiscal decentralization and cross-national indexes of perceived corruption derived from surveys of risk analysts, businessmen and citizens. In particular, scholars have examined perceived corruption ratings produced by Transparency International (TI), the World Bank (WB), and the business consultancy Political Risk Services, which publishes the International Country Risk Guide (ICRG). The findings of these studies have been mixed and sometimes mutually contradictory".

Next, the entrepreneurial effort, E_{1sj} is a binary indicator of nascent entrepreneurship. This refers to the active involvement of an individual in a start-up effort. The measure is taken from GEM Adult Population Survey: my first sample. Here, more than 45% of adults are reported to be female. After dropping some missing observations, almost 37% of the sample are aged below 35 years, and nearly 17% are 55 years and above. 73.69% of the remaining sample report having 0–4 members in their permanent household; 13% of adults have no education, and only 35.45% have attained post-secondary education or higher. While 90% of the sample report their income status, of that, 34.56% belong to the upper 33 percentile and 31.57% to the lowest.

For the main explanatory variable D_j, I use fiscal decentralization from the International Monetary Fund (IMF) Fiscal Decentralization dataset. I use two alternate proxies: tax revenue and revenue, both for the sub-national government. It is useful to reiterate here that government decentralization is a multi-dimensional phenomenon. It has various forms – like political decentralization and fiscal decentralization – that arguably impact corruption in different ways. I use fiscal decentralization specifically for two reasons. First, data availability: matching countries observed in both the GEM and ES data sets for the same year is challenging in itself. The problem exacerbates when decentralization data must also be observed for those countries in their respective years. The best data set to circumvent this issue is the IMF fiscal decentralization set. Nevertheless, my sample has only 24 countries, ranging between 2010

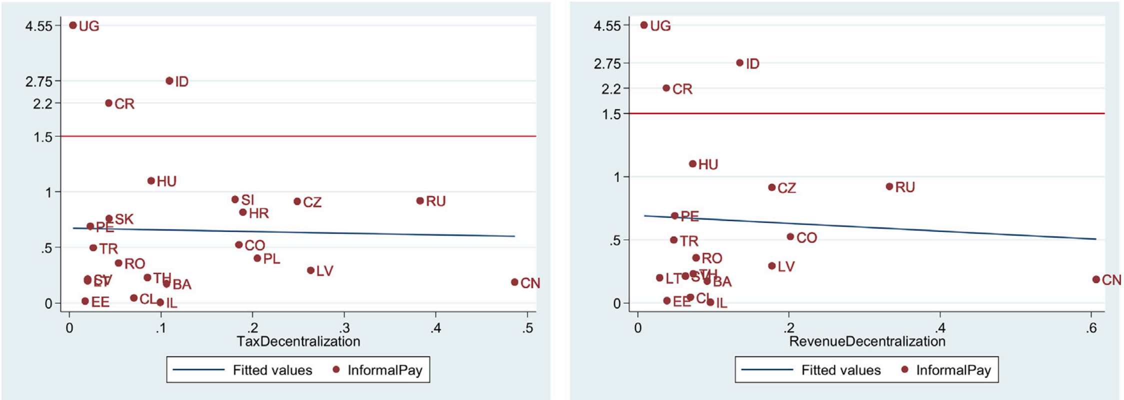

Fig. 1. Scatterplots of government decentralization vs. firm-level corruption (Informal Payments).
Note: Left panel corresponds to Tax Decentralization as the proxy for fiscal decentralization; right panel corresponds to Revenue Decentralization as the proxy. Both panels use Informal Payments as the corruption measure. The red line in each panel shows the break in the y-axis for the legibility of all data points. All countries are reported according to their respective ISO codes described in the ISO 3166 international standard. Refer to Table A.2 in the Appendix for details. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

and 2016, because of the above-explained limitation (refer to Table A.2 in the Appendix for details).[20] This leads to an additional challenge in my current analysis: a few clusters problem. I will return to this point shortly.

Second, prior studies find no evidence of fiscal decentralization being endogenous in nature, as opposed to political decentralization (Choudhury, 2015). This potentially reduces one complication, at least, in the current research. Even so, one may propose to investigate this possibility eventually. At present, it is beyond the scope of the paper to explore this channel due to considerable data constraints.[21] For an overview, Figs. 1–4 present the scatterplots of these variables. Fig. 1 shows a negative association between government decentralization and firm-level corruption when the latter is measured by informal payments. However, the association turns positive in Fig. 2 when contract payments measure firm-level corruption. Both figures capture tax revenue and revenue as the proxies for fiscal decentralization on the left and right panels, respectively.

Fig. 3 reveals government decentralization to be negatively associated with entrepreneurial effort, where tax revenue proxy is plotted in the left panel and revenue in the right. On the other hand, the entrepreneurial effort is positively related to firm-level corruption, as depicted in Fig. 4, with informal payments captured in the left panel and contract payments in the right. In other words, with more entrepreneurial efforts taken, a country is likely to experience more firm-level bribery. This is fairly consistent with my argument for potential non-random selection in entrepreneurship and the prior empirical evidence of a compositional change in the type of entrepreneurs (discussed earlier in Section 2.1).

Looking at Figs. 3 and 4 together, one may naively interpret them to imply that with greater decentralization, there is lesser entrepreneurial effort and lesser firm-level corruption. That is, decentralization is negatively associated with the corrupt practices of firms. However, correlation is not transitive. As confirmed in Fig. 2, fiscal decentralization and corrupt payments relate positively instead. Nevertheless, in the next section, I show that once the potential sample selection issue is accounted for in the analysis, this relationship between fiscal decentralization and firms' corrupt payments turns negative.

As for other covariates, I follow extant literature. I use the gross domestic product (GDP) per capita, government revenue, imports, voice and accountability, fuel exports, and population to control for country-level characteristics given by X_j.[22] For firm-level characteristics X_{2ij}, I use dummy variables to control for the ownership status, the sector, and the size of a firm. The choice for these firm-specific attributes depends on both the extant literature (see Fan et al., 2009) and the feasibility of observing comparable features for nascent entrepreneurs in the GEM data set $\left(X_{1sj}\right)$. Strictly speaking, the sample designs of ES and GEM are

[20] ES had started in 2006. GEM data are available only till 2016. Nevertheless, the final sample period is restricted from 2010 to 2016. This is only because of the matching difficulty of countries in respective years in both these data sets and the IMF set as well.

[21] Current data lack enough variation to control for the potential endogeneity of fiscal decentralization through instrumental variables. This can only be addressed in future research when more data are available. Like any other empirical investigation, I concede my study is not perfect. Given this, one may have some reservations in interpreting the estimates of my analysis as "causal". But it is important to realize that previous studies have already explored the question of the "causal impact" of fiscal decentralization on firm-level corruption and found no evidence of endogeneity. However, these studies failed to take into consideration the potential sample selection issue, thereby mistakenly interpreting the impact as "causal". The objective of my study is to deal with this particular shortcoming. The focus is to address the *overlooked* sample selection issue *alone* while taking the other aspects as is, based on prior evidence. In that sense, I reckon my analysis to be "causal."

[22] Government revenue captures the size of a government, and voice and accountability reflects democracy status or institutional quality of a country. Several proxies from various sources – e.g., World Governance Indicators and Fraser Institute – have been used in the extant literature to capture institutional quality. To name a few, voice and accountability, political stability, government effectiveness, regulatory quality, the rule of law, and control of corruption (Kandil, 2009; Wagner et al., 2009; Kuncic, 2014). See La Porta et al. (1999), Goel and Nelson (2010), Fan et al. (2009), Di Matteo (2013), and Amin and Soh (2019) for country-level controls concerning corruption.

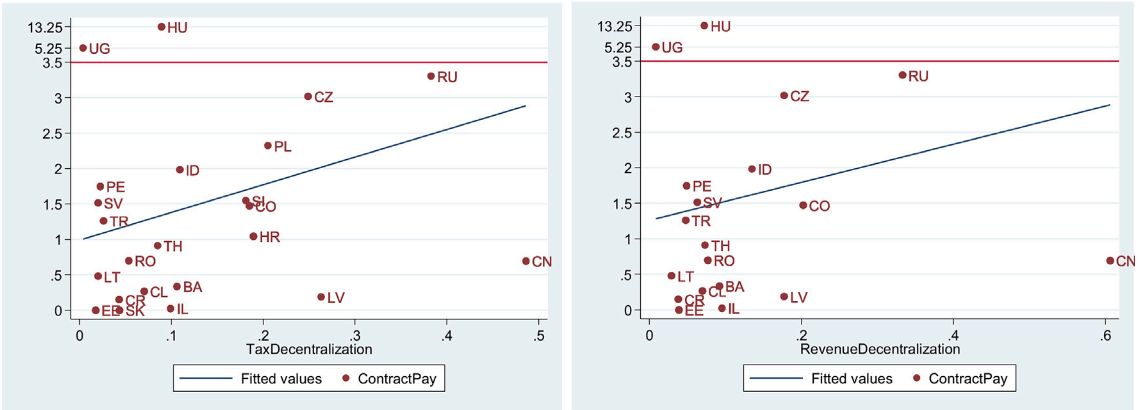

Fig. 2. Scatterplots of government decentralization vs. firm-level corruption (Contract Payments).
Note: Left panel corresponds to Tax Decentralization as the proxy for fiscal decentralization; right panel corresponds to Revenue Decentralization as the proxy. Both panels use Contract Payments as the corruption measure. The red line in each panel shows the break in the y-axis for the legibility of all data points. All countries are reported according to their respective ISO codes described in the ISO 3166 international standard. Refer to Table A.2 in the Appendix for details. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

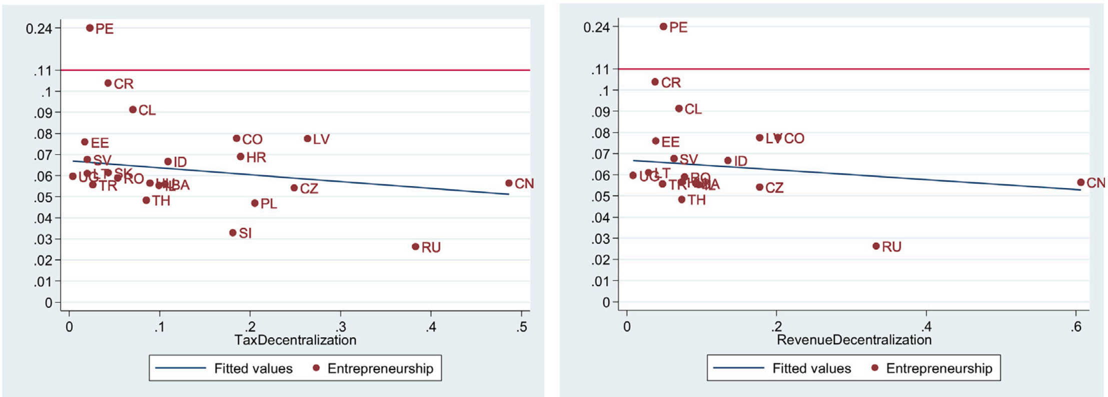

Fig. 3. Scatterplots of government decentralization vs. entrepreneurial effort.
Note: Left panel corresponds to Tax Decentralization as the proxy for fiscal decentralization; right panel corresponds to Revenue Decentralization as the proxy. The red line in each panel shows the break in the y-axis for the legibility of all data points. All countries are reported according to their respective ISO codes described in the ISO 3166 international standard. Refer to Table A.2 in the Appendix for details. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

not *precisely* the same; that is practically implausible. GEM surveys the adult population, which includes both entrepreneurs and non-entrepreneurs, whereas ES interviews only firms operating in an economy. The target group is fundamentally different for the two surveys.

Despite this difference, if one reflects deeply on the issue, the decision of some of the adult population of a country to take up entrepreneurial activities eventuates into a thriving business environment in an economy. Thus, intuitively, the “full sample” having information on entrepreneurial effort for *all* observations can only be represented by the adult population of a nation. Meanwhile, the “selected sub-sample” containing information on bribery payments to public officials while running a business can exclusively be reported by the firms functioning in the country. Note, I observe the same country in a certain year in both the samples — for example, Peru in 2010 in both GEM and ES. Given this, it is unlikely that the determinants of entrepreneurship within a particular economy in the first sample will not be applicable to that same economy in the second sample. I acknowledge that this approach is not conventional but as stated earlier, using these two different data sources is the best possible option available at present to answer the specific question at hand.

Nonetheless, I present the difference in mean (MD) of all the control variables between the two samples of *entrepreneurs* alone and the corresponding standardized mean difference (SMD) in Table A.3 in the Appendix to compare the baseline covariates. MD is calculated via a two-sample t-test to check the equality of means of a variable between two unpaired samples; SMD is the

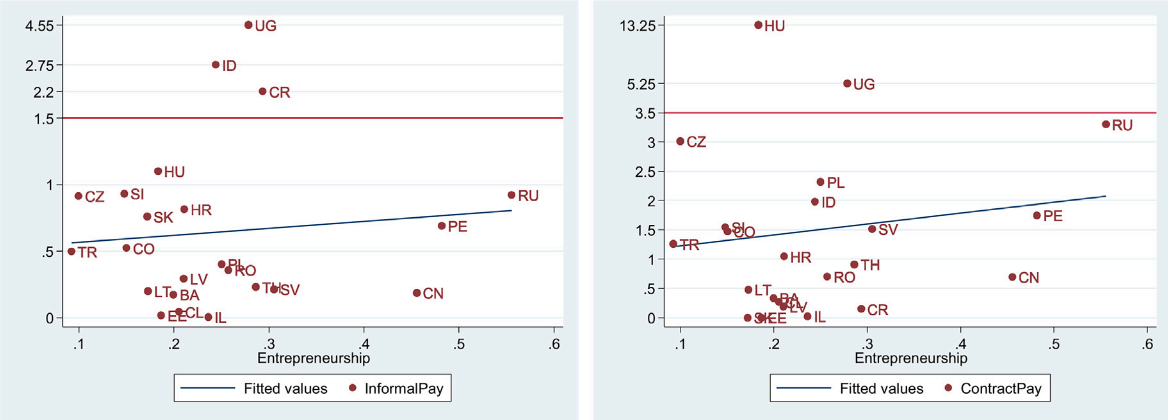

Fig. 4. Scatterplots of entrepreneurial effort vs. firm-level corruption.
Note: Left panel corresponds to Informal Payments as the corruption measure; right panel corresponds to Contract Payments as the corruption measure. The red line in each panel shows the break in the y-axis for the legibility of all data points. All countries are reported according to their respective ISO codes described in the ISO 3166 international standard. Refer to Table A.2 in the Appendix for details. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

difference in means divided by their respective standard deviations (Andrade, 2020).[23] While both the statistics can be used when the outcomes are measured in the same unit, SMD is the only choice when measured in different units (Takeshima et al., 2014). However, in recent times, the use of SMD is preferred over the use of statistical significance testing, especially in the setting of a binary exposure (Austin, 2019).

Further, Cohen (1988) proposed an effect size index – known as Cohen's d – to compare the sample means. This index is "interpreted as a sample-based estimate of the strength of the relationship between two variables in a statistical population" (Yang and Dalton, 2012, p. 1). Andrade (2020, p. 3) states: "This guidance is now almost set in stone. SMD values of 0.2–0.5 are considered small, values of 0.5–0.8 are considered medium, and values > 0.8 are considered large". Table A.3 shows the two-sample t-test is statistically insignificant for GDP per capita, implying no difference in the means of this variable across the two samples. Although all other variables report the difference as statistically significant, none of the differences are economically large, with the exception of fuel exports. Moreover, all these variables report small or medium size effects, as reported by Cohen's d statistic. Together, I interpret these as evidence in favor of common characteristics of entrepreneurs between the two samples.

Lastly, for exclusion restriction ER_j in the selection equation, I use entry regulations for starting a business from World Bank's Doing Business data. I use four alternate proxies for this: procedures, time, cost, and capital. Note that the exclusion restriction in my study must be a country-level measure. This is mainly because I use two different data sets for my investigation. Any individual-specific characteristic observed in GEM data affecting entrepreneurship decisions will not be observed in the firm-level ES data. Given this, only a country-level characteristic – that is exogenous to the outcome equation – is feasible.

Entry regulation is reasonable because these regulations may affect corrupt practices only when there is an attempt to start up a business. Djankov et al. (2002, p. 4) find "stricter regulation of entry is associated with sharply higher levels of corruption". However, firms can only experience such a high level of corruption when (potentially corrupt) individuals intend to take on some entrepreneurial effort. Alternately, if intensive entry regulations discourage entrepreneurial activities, as Klapper et al. (2006) find with European firms, overall bribe payments are likely to reduce. In short, entry regulations as exclusion restrictions seems appropriate in the current context.

Regarding the few clusters problem mentioned above, I have only 24 clusters in total in my analysis due to the limitation of data. This number reduces further as alternate measures are used to estimate the impact. Hence, standard bootstrapping clustered by country is not a viable option in this case to report the correct standard errors, unlike that explained earlier in section 3.2. Instead, score wild bootstrapping clustered at the country-level with 500 repetitions is applied to get the correct p-values for each estimate (see Cameron and Miller, 2015).[24]

In addition to all of the above, I extend my analysis by including industry-level fixed effects in Eqs. (4) and (5) once the primary set of investigations is conducted. This modification enables controlling for the specific industry types of firms or entrepreneurs at a more *disaggregated* level and improves the study to a reasonable degree.[25] I will discuss this aspect in detail in the next section.

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[23] Two-sample t-test is executed on Stata using the -ttest- command, whereas SMD is calculated using -esize- command.

[24] "[S]core wild bootstrap was proposed by Kline and Santos (2012) for nonlinear models, including maximum likelihood and GMM models" (Cameron and Miller, 2015, p. 356). I use STATA's *-boottest-* command with the option of *-cluster-* by country.

[25] I am grateful to an anonymous referee for pointing this out.

Table 3
Impact of fiscal decentralization on corruption.

Variables	Informal payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−1.621	1.743	−1.973	−1.474	−1.773	−0.553	−0.974	−1.291	−1.469	−13.470 ‡	−0.638
p-value	(0.390)	(0.312)	(0.346)	(0.444)	(0.188)	(0.430)	(0.784)	(0.534)	(0.462)	(0.078)	(0.578)
IMR							0.271	−0.850	−0.725	31.162	0.246
p-value							(0.942)	(0.802)	(0.566)	(0.106)	(0.832)
N	15527	11798	15527	15527	15290	12035	11798	15527	15527	15290	12035
Clusters	23	16	23	23	22	17	16	23	23	22	17
Revenue Decentralization	−3.155	−1.558	−3.303	−3.080	−3.672	−0.836 ‡	0.332	−3.317	−2.989	−4.080	0.047
p-value	(0.186)	(0.218)	(0.214)	(0.164)	(0.230)	(0.086)	(0.342)	(0.138)	(0.182)	(0.196)	(0.972)
IMR							−1.605 ‡	0.312	−0.558	1.975	−1.307
p-value							(0.092)	(0.934)	(0.658)	(0.708)	(0.494)
N	14281	10789	14281	14281	14281	10789	10789	14281	14281	14281	10789
Clusters	19	13	19	19	19	13	13	19	19	19	13

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Model with “No ER-No IMR” excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model “With ER” includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model “With IMR” controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with “All” include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. See text for further details.

5. Results

The results using informal payments as the corruption measure are presented in Table 3. Those using contract payments are reported in Table 4. While the top panel in each table presents the results from estimation using tax revenue as the fiscal decentralization proxy, the bottom panel reports those using revenue as the proxy.

To better understand the performance of my preferred model – given by Eq. (9) – I use two alternate baseline specifications for comparative analysis. The first specification completely ignores entry regulation in the current context *and* the sample selection issue.[26] The results of this are presented under the column labeled “No ER-No IMR”. The second specification assumes entry regulation to be a potential determinant of corrupt practices by firms and, therefore, is controlled for in the main model. However, it ignores the sample selection problem.[27] The results of this are under the column labeled “With ER”. Finally, the results of my preferred model are given under the column “With IMR” that controls for the selection issue and in doing so, I use entry regulation as the exclusion restriction, as explained earlier.

When including entry regulation in the second baseline specification and my preferred model, I use five alternative ways to estimate the impact: all the four proxies together and then each of the four proxies at a time. These are presented under columns “All,” “Procedures,” “Time”, “Cost”, and “Capital”, respectively.

The results are interesting. Table 3 bottom panel reveals that in the “No ER-No IMR” specification, the estimated effect of revenue decentralization is −44.17% but is not statistically different from zero. However, one also cannot rule out effects as large as 30.66% (based on the upper limit of 95% confidence interval) or as small as −120.82% (given the lower limit of the 95% confidence interval) due to one standard deviation increase in revenue decentralization.[28] The estimated effect of revenue decentralization changes to −21.8% when considering the other specification: “With ER” and with “All” the proxies for entry regulation together, but this is also not statistically different from zero. Nevertheless, effects as large as 252.28% and as small as −241.64% again cannot be ruled out given the 95% confidence interval (CI) due to one standard deviation change in revenue decentralization.[29] Though the point estimate is smaller in this instance, the CI is wider here implying one cannot rule out the possibility of large effects.

In contrast to these, my preferred model, “With IMR”, finds evidence in favor of sample selection bias when considering “All” the four proxies for entry regulation together. This is reflected by the statistical significance of the coefficient of $\widehat{IMR}_{2ij}$ at the $p < 0.10$ confidence level. The point estimate suggests a decrease in informal payments by 32% due to an increase in the bias by

[26] Model “No ER-No IMR” is formally given as:

$$C_{2ij} = \alpha + \beta D_j + X_j\theta_1 + X_{2ij}\theta_2 + \epsilon_{2ij}..$$

[27] Model “With ER” is formally given as:

$$C_{2ij} = \alpha + \beta D_j + X_j\theta_1 + X_{2ij}\theta_2 + ER_j\theta_3 + \epsilon_{2ij}..$$

[28] Although not presented here in the interest of brevity, the 95% confidence interval is given as [−8.63, 2.19].

[29] In this case, the 95% CI is given as [−17.26, 18.02].

Table 4
Impact of fiscal decentralization on corruption.

Variables	Contract payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−0.923	9.228 †	−1.998	−0.682	0.926	−0.431	−1.933	−0.295	−0.778	−20.377 ‡	−1.578
p-value	(0.666)	(0.042)	(0.426)	(0.752)	(0.580)	(0.772)	(0.654)	(0.872)	(0.718)	(0.056)	(0.716)
IMR							2.616	−2.070	−0.520	51.979 ‡	2.178
p-value							(0.700)	(0.490)	(0.846)	(0.078)	(0.718)
N	3095	2076	3095	3095	3025	2146	2076	3095	3095	3025	2146
Clusters	24	17	24	24	23	18	17	24	24	23	18
Revenue Decentralization	−5.178	29.063	−6.300	−5.102	−5.234	−2.493	−11.647 ‡	−3.829	−5.699	−8.612 †	−3.536
p-value	(0.186)	(0.246)	(0.210)	(0.176)	(0.344)	(0.468)	(0.094)	(0.196)	(0.138)	(0.030)	(0.604)
IMR							17.786 *	−2.804	1.105	6.724	1.941
p-value							(0.004)	(0.706)	(0.758)	(0.324)	(0.860)
N	2755	1806	2755	2755	2755	1806	1806	2755	2755	2755	1806
Clusters	19	13	19	19	19	13	13	19	19	19	13

Notes: ‡ $p<0.10$, † $p<0.05$, * $p<0.01$. Model with “No ER-No IMR” excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model “With ER” includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model “With IMR” controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with “All” include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. See text for further details.

one standard deviation.[30] This revelation is striking. Once the bias is controlled for, the estimated effect of revenue decentralization is 4.65% but is not statistically different from zero. However, effects as large as 19.25% and as small as −16% cannot be ruled out, given the 95% CI, due to an increase in revenue decentralization by one standard deviation.[31] These findings are in contradiction to those reported by the baseline specifications to some reasonable degree. First, the point estimate is larger in this instance.[32] Second, the tighter width of the 95% CI rules out the possibility of *huge* effects – as reported by the naive estimation strategies – once the underlying sample selection issue is addressed.

To put things into perspective: the estimated impact of fiscal decentralization on corrupt practices is not statistically different from zero when the sample selection issue is totally ignored. Nevertheless, the point estimates indicate that one cannot entirely dismiss a curbing impact on firm-level corrupt practices in such an instance. This suggestion is qualitatively consistent with prior findings (Fan et al., 2009; Choudhury, 2015). The usefulness of decentralizing government revenue to a greater extent may, therefore, come across as a plausible recommendation to control firm-level bribery if one relies on these point estimates alone. This is in accordance with one argument existing in the literature: giving sub-national governments a greater share of revenue may disincentivize local officers from asking for bribes from firms (Fan et al., 2009; Choudhury, 2015). However, such an understanding turns out to be moderately fallacious when considering the underlying selection issue.

First, the finding here corroborates my argument that bribe-prone nascent entrepreneurs are likely to self-select into entrepreneurial activity when the business environment is not conducive. Second, controlling for the selection bias yields an estimated impact that is not statistically different from zero. This is consistent with the general results of the naive estimation strategies. But apart from this, the point estimate per se and the 95% CI hint at a relatively different story. To begin with: it is necessary to bear in mind that the point estimate, in this case, represents the best estimate available to understand the association between the two phenomena, given the evidence of selection bias. In other words, though it is hard to infer a *definitive* answer about the ‘true value’ of the parameter of interest – unlike what one would prefer ideally – the coefficient here is still the best point estimate available at this stage.[33]

Considering this, it unfolds that giving a greater revenue share to sub-national governments cannot oust the possibility of an increase in informal payments by firms. This implication is more supportive of the alternate argument that exists in the literature: “local officials may derive greater utility from bribe revenue, which they can spend at will, than from increased revenue officially received by the local budget, which may be costly to embezzle” (Fan et al., 2009, p. 19). Basically, if the associated risk and cost of embezzling from official funds are high, then giving sub-national governments a greater share of revenue will not deter local officials

[30] The standard deviation of IMR when the impact is estimated with informal payments as the corruption measure and revenue decentralization as the proxy is given as 0.20.

[31] The CI is given as [−1.143, 1.375].

[32] Discussing both the economic and statistical significance of a coefficient estimate is crucial to understanding the “importance” of a variable’s impact (Miller and Rodgers, 2008, p. 2). Reinforcing this argument, Hoover and Siegler (2008, p. 2) state: “a coefficient that is estimated to have economically large size, *even if it is statistically insignificant*, cannot properly be neglected.”

[33] Note that failing to reject the null does not mean that the truth is zero. It simply means that available data are insufficient to rule out this possibility with any confidence. However, the data also cannot rule out the possibility of the estimated effect of 5% approximately. Nor can it rule out effects as large as 19.25% and as small as −16%, given the 95% CI. One can only claim that the available data are just sufficient to rule out values *outside* this confidence interval.

from demanding bribes to increase their personal income. Next, one cannot fail to notice that the tighter CI here, when the bias is taken care of, eliminates the possibility of *very large* effects, as opposed to the implications of the baseline specifications. Thus, it is fair to interpret that the impression the point estimates give about the probable impact of fiscal decentralization on firm-level corruption *alters reasonably* when the sample selection problem is taken into consideration.

The significance of this problem gets reinforced when I move to the results presented in Table 4. The top panel shows that the estimated effect of tax decentralization on contract payments is −12% for the "No ER-No IMR" specification but is not statistically different from zero. However, effects as large as 75.83% and as small as −72.85% cannot be ruled out either, given the 95% CI.[34] As opposed to this, the second specification – "With ER" and with "Cost" as the only entry regulation proxy – indicates the estimated effect to be 12% but not statistically different from zero. Yet, effects as large as 54.55% and as small as −50.86% cannot be ruled out.[35] Interestingly, when I consider my preferred model "With IMR" and with "Cost" as the only entry regulation proxy, there is reasonable evidence of selection bias. The coefficient of IMR is statistically significant at the $p < 0.10$ confidence level. The point estimate reveals an increase in contract payments by 780% due to an increase in the bias by one standard deviation.[36] After controlling for this bias, contract payments by firms are likely to *decrease* by almost 265% due to an increase in tax decentralization by one standard deviation. This impact is statistically significant at the $p < 0.10$ confidence level.

I find similar results when using revenue decentralization as the proxy, as reflected in the bottom panel of Table 4. The first baseline specification of "No ER-No IMR" shows that the estimated impact of revenue decentralization on contract payments is approximately −72.5% due to one standard deviation increase in revenue decentralization but is not statistically different from zero. Even so, effects as large as 54% and as small as −204.96% cannot be ruled out given the 95% CI.[37] In contrast, the estimated effect is almost 407% but is not statistically different from zero when I consider the alternate specification "With ER" and "All" the proxies for entry regulation together. However, given the exceptionally wide 95% CI in this case, effects as large as 8375% and as small as −4688% cannot be ruled out.[38] Note, these findings of the naive estimation strategies are somewhat qualitatively consistent with that discussed for the top panel of this table with tax decentralization as the proxy.

Now when I consider my preferred model "With IMR" and "All" the proxies for entry regulation together, there is strong evidence of sample selection bias. The IMR coefficient is statistically significant at the $p < 0.01$ confidence level. The point estimate indicates an increase in contract payments by 302% almost due to an increase in bias by one standard deviation.[39] Once this bias is controlled for in the outcome equation, revenue decentralization is likely to *decrease* contract payments of firms by almost 163% due to an increase by one standard deviation. This impact is statistically significant at the $p < 0.10$ confidence level. Findings of this preferred model are also consistent with that reported in the top panel of Table 4 when tax decentralization is used as the alternate measure for government decentralization.[40]

In short, my preferred estimates in Table 4 indicate fiscal decentralization to *reduce* informal gifts by firms to government officials when trying to secure a contract, and that too by a considerable degree. This finding is in line with the former argument – discussed earlier – about greater revenue share to sub-national governments providing less incentive to local government officials to extract rents from firms. But that may simply imply an increased possibility of embezzlement of official funds instead. Hence, the final effect of greater decentralization on the *overall* corruption level of a country is ambiguous. To elaborate, corruption in a country may exist in various forms like bribery, graft, embezzlement, fraud, extortion, favoritism, nepotism, and others (Morris, 2011). My finding here indicates a decrease in *firm-level bribery* due to increased fiscal decentralization. But, suppose one believes that this may lead to increased embezzlement of official funds by public officials. In that case, we cannot conclude anything with certainty regarding the impact on a country's *overall* corruption level, taking all the forms together due to increased fiscal decentralization. For that, a specific measure to capture corruption in the form of embezzlement is necessary. That calls for a separate analysis in the future as it is currently not viable in this investigation.

Next, in trying to interpret the contrasting nature of the estimated impacts of fiscal decentralization on informal payments (Table 3) and contract payments (Table 4), one may try to conjecture the intentions of local officials in the first place. If local officials are selflessly driven to support economic activities in their immediate area, then that gives them enough impetus not to ask for bribes from firms. Alternatively, if they intend to embezzle from official funds, as explained above, then also there is no reason to ask for bribes from firms. In both cases, greater fiscal decentralization hints at reduced firm-level corruption. So, hypothetically,

[34] The CI is given as [−5.604, 5.833].

[35] The 95% CI is given as [−3.912, 4.196].

[36] The standard deviation of IMR is 0.15 when the impact is estimated using contract payments as the corruption measure and tax decentralization as the proxy.

[37] The 95% CI is given as [−14.64, 3.871].

[38] The 95% CI is given as [−334.9, 598.5].

[39] The standard deviation of IMR when using contract payments as corruption measure and revenue decentralization as the proxy for estimating the impact is 0.17.

[40] Note, when the coefficient of IMR is statistically insignificant, there is a lack of evidence of the sample selection issue (like that in the top panels of Tables 3 and 4). In such an instance, we revert to the initial "naive" estimation strategies to understand the impact. The coefficients reported by these strategies are likely to be the correct estimates in that case. In this respect, Heckman (1979, p. 158) states: "The usual formulas for standard errors for least squares coefficients are not appropriate except in the important case of the null hypothesis of no selection bias…In that case, the usual regression standard errors are appropriate..". Given this, the top panel of Table 3 implies an *absence* of a causal link between tax decentralization and informal payments by firms because the estimates under "No ER-No IMR" and "With IMR" specifications are all statistically insignificant. In contrast, the top panel of Table 4 reveals tax decentralization to *increase* contract payments by firms when all the entry regulation proxies are included in the model (column "All" "With ER" specification). The impact is statistically significant at the $p < 0.05$ confidence level.

both informal payments and contract payments should decrease with greater decentralization. However, statistically, this turns out to be valid only for the latter.

One plausible explanation is if local officials are relatively altruistic towards regional development and the well-being of its people. Thus, when the power is relegated to the local level due to greater decentralization, they find less incentive to demand bribes from firms when the latter approaches them to secure contracts. This, therefore, explains the decrease in contract payments compared to what the firms may experience when dealing with a more centralized government. However, local officials may not be entirely benevolent either. In the case of informal payments as a percentage of annual sales, greater power with local officials does not necessarily change the situation for firms. This is possible if merely a *reallocation* of bribe payments takes place with greater decentralization: instead of paying the central government, the rent is steered towards the local officials.

Having said that, one can put forth an alternate hypothesis as well. If local officials are more concerned about their long-term benefits, they may help a business grow in their jurisdiction in the initial stage with the objective of extracting rents once these businesses flourish. In other words, it may be profitable for them to support a firm initially to secure contracts and grow overall so that they can eventually get a share of the annual sales from these businesses. This then encompasses the possibility of increasing informal payments with greater decentralization, as indicated by the point estimate, while reducing contract payments considerably.

To sum up, I find reasonable evidence for non-random selection into entrepreneurship in the current context.[41] For instances where there is such evidence, fiscal decentralization has an estimated effect of approximately 5% but is not statistically different from zero as far as informal payments are concerned. But, greater fiscal decentralization is likely to *decrease* contract payments by substantial amounts, which are statistically significant, once the sample selection problem is addressed. One may raise concerns about the actual magnitude of these impacts, given their massiveness. Nevertheless, the point to consider is the *contradictory* nature of these estimated impacts compared to those derived from naive estimation strategies: in terms of the sign, size, and statistical significance. The naive estimates suggest that failure to account for the sample selection issue is likely to have a downward (upward) bias in the point estimates of fiscal decentralization's impact on informal (contract) payments in comparison to my preferred estimates.

Given these results, it seems crucial at this stage to reinvestigate the question while controlling for the *specific type* of industry that a firm or an entrepreneur belongs to in this scenario. Recall that the current analysis already controls for the sector type by broad categories following prior studies (e.g., Fan et al., 2009; Choudhury, 2015). But here, as such, I extend the preferred model by including industry-level fixed effects in both the outcome (4) and selection (5) equations and repeat the entire exercise with this modification. Due to the nature of the question raised in this study, this strikes me as a reasonable improvement over the basic model utilized in the extant literature so far. But having said that, it is not straightforward to incorporate industry dummies at a more disaggregated level in the current analysis.

Note that the GEM data report its industry codes as per the ISIC Rev 4, while the ES has according to the ISIC Rev 3.1.[42] Since some ISIC Rev 3.1 four-digit sector codes split into multiple categories of ISIC Rev 4 sectors and vice versa, it is not possible to *perfectly* match the industries across the two samples. Thus, with the help of the concordance table, a corresponding probability that a particular ISIC Rev 3.1 industry in the ES data set belongs to an ISIC Rev 4 industry is generated. This, thereby, gives me the "possibility" that a sector is associated with a specific ISIC Rev 4 industry code in both samples and facilitates the inclusion of industry dummies disaggregated at the two-digit level code in all the specifications. While generating these probabilities, the underlying premise entails each ISIC Rev 3.1 sector splitting into multiple ISIC Rev 4 codes with equal weights. Although a strong assumption, this is the only feasible way to include industry-level fixed effects in the current analysis, mainly because of the complexity associated with using two separate samples.[43]

The results of this extended model using the two alternate corruption measures are presented in Tables 5 and 6, respectively. The results are qualitatively similar for informal payments (Table 5) compared with those reported initially by the preferred model in Table 3, with just one exception. The statistical significance of the coefficient of IMR is lost in the bottom panel, where revenue decentralization is the proxy and "All" entry regulation proxies are included. Thus, there is a lack of evidence for selection bias here. When considering the results for contract payments as the corruption measure (Table 6), evidence for the sample selection issue is robust to the inclusion of industry-level fixed effects: top panel under "With IMR" and "Cost" column and bottom panel "With IMR" and "All" column.

But interestingly, after controlling for the bias, the statistical significance of the impact of government decentralization on firm-level bribery is more prominent here. Both tax (top panel "With IMR" and "Cost" entry regulation) and revenue decentralization coefficients (bottom panel "With IMR" and "All" entry regulation proxies) are statistically significant at the $p < 0.05$ confidence level, unlike that at the $p < 0.10$ confidence level in the initial analysis without the fixed effects (Table 4). The point estimates indicate a *decrease* in firms' contract payments by 87.75% and 173.63% due to one standard deviation increase in tax and revenue decentralization, respectively. Again, these results are qualitatively consistent with those initially reported from the preferred model without the industry-level fixed effects. Thus, non-random selection into entrepreneurship persists as a serious concern despite controlling for the industry types at a disaggregated level. Ignoring this aspect in the analysis proves to be fairly consequential.

Lastly, my main finding here raises some questions about the previous conclusions of empirical investigations where this selection bias is not accounted for. The reported impacts of various economic variables on firm-level corruption may misrepresent the actual

[41] As a robustness check, I repeat my analysis keeping the total number of observations the same – 10789 for Table 3 and 1806 for Table 4 – across all columns. The results are presented in the Appendix in Tables A.4 and A.5, respectively. Overall, the evidence of selection bias is substantial here, thereby rendering support to my concern about the sample selection issue in the current context.

[42] ISIC stands for the International Standard Industrial Classification of All Economic Activities.

[43] See https://ec.europa.eu/eurostat/ramon/relations/index.cfm?TargetUrl=LST_REL_DLD&StrNomRelCode=ISIC%20REV.%203.1%20-%20ISIC%20REV.%204.

Table 5
Impact of fiscal decentralization on corruption: With industry-level fixed effects.

Variables	Informal payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−1.718	1.763	−2.097	−1.582	−1.863	−0.594	−0.670	−1.436	−1.589	−5.985 ‡	−0.489
p-value	(0.338)	(0.362)	(0.292)	(0.402)	(0.180)	(0.394)	(0.796)	(0.482)	(0.404)	(0.050)	(0.662)
IMR							−0.267	−0.709	−0.539	10.408	−0.055
p-value							(0.882)	(0.828)	(0.686)	(0.168)	(0.956)
N	15473	11746	15473	15473	15236	11983	11746	15473	15473	15236	11983
Clusters	23	16	23	23	22	17	16	23	23	22	17
Revenue Decentralization	−3.261	−1.733	−3.416	−3.199	−3.713	−0.937 ‡	0.403	−3.402	−3.175	−4.464	0.387
p-value	(0.164)	(0.214)	(0.182)	(0.130)	(0.208)	(0.082)	(0.550)	(0.120)	(0.120)	(0.110)	(0.816)
IMR							−1.705	0.278	−0.292	2.658	−1.782
p-value							(0.104)	(0.910)	(0.804)	(0.496)	(0.412)
N	14231	10741	14231	14231	14231	10741	10741	14231	14231	14231	10741
Clusters	19	13	19	19	19	13	13	19	19	19	13

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Model with "No ER-No IMR" excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model "With ER" includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model "With IMR" controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with "All" include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. The only difference between this table and Table 3 is this includes industry fixed effects in all the specifications. Altogether 51 industry dummies are included. See text for further details.

Table 6
Impact of fiscal decentralization on corruption: With industry-level fixed effects.

Variables	Contract payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−1.135	8.159	−1.974	−0.949	0.725	−1.241	−5.618	−0.656	−1.050	−6.750 †	−4.377
p-value	(0.590)	(0.104)	(0.418)	(0.652)	(0.658)	(0.428)	(0.130)	(0.744)	(0.620)	(0.022)	(0.356)
IMR							8.634	−1.515	−0.281	14.290 ‡	6.041
p-value							(0.120)	(0.664)	(0.932)	(0.062)	(0.308)
N	3087	2069	3087	3087	3017	2139	2069	3087	3087	3017	2139
Clusters	24	17	24	24	23	18	17	24	24	23	18
Revenue Decentralization	−5.378	26.940	−6.137	−5.410	−5.073	−3.898	−12.402 †	−4.822 ‡	−6.056 ‡	−8.730 *	−7.839
p-value	(0.132)	(0.262)	(0.174)	(0.124)	(0.316)	(0.312)	(0.028)	(0.090)	(0.094)	(0.006)	(0.300)
IMR							16.200 *	−1.202	1.479	6.795	7.072
p-value							(0.006)	(0.830)	(0.710)	(0.232)	(0.574)
N	2747	1799	2747	2747	2747	1799	1799	2747	2747	2747	1799
Clusters	19	13	19	19	19	13	13	19	19	19	13

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Model with "No ER-No IMR" excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model "With ER" includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model "With IMR" controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with "All" include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. The only difference between this table and Table 4 is this includes industry fixed effects in all the specifications. Altogether 44 industry dummies are included. See text for further details.

effect. Not just the statistical significance, the economic significance of these reported impacts may also need reconsideration. The claims of causality, if any, are also contentious. Given these, the varied policy recommendations and implementations that may have followed warrant some reassessments. This is definitely not to suggest that the prior studies are futile. But, it is crucial to realize that their results must be treated with caution. In a non-random sample, the estimates reported by the analysis are relevant only to that specific sub-population. The results cannot be generalized to the overall population. Thus, any *common* conclusion drawn in such an instance regarding pertinent policies to control corruption in an economy will be essentially misleading.

6. Conclusion

"[C]orruption may be a proximate determinant of entrepreneurial activity" Bologna and Ross (2015, p. 60). If offering a bribe or even the inclination to do so is a prerequisite to enter a market successfully, then participation in entrepreneurship is necessarily

Table A.1
Variable definitions and sources.

Variable	Definition	Source
Informal Payments	Percent of total annual sales paid in informal payments	World Bank's Enterprise Survey (ES), Standardized Set
Contract Payments	Percent of contract value paid in informal gifts to government to secure contract	World Bank's Enterprise Survey (ES), Standardized Set
Nascent Entrepreneurship	Actively involved in start-up effort, owner, no wages yet; 1 = Yes, 0 = No	Global Entrepreneurship Monitor (GEM), Adult Population Survey
Tax Decentralization	Tax revenue decentralization, sub-national government	The International Monetary Fund (IMF) Fiscal Decentralization Dataset
Revenue Decentralization	Revenue decentralization, sub-national government	The International Monetary Fund (IMF) Fiscal Decentralization Dataset
GDP per Capita	Per capita values for gross domestic product (GDP) expressed in current international dollars converted by purchasing power parity (PPP) conversion factor	World Development Indicators Database, World Bank
Government Revenue	Revenue excluding grants, % of GDP	World Development Indicators Database, World Bank
Imports	Imports of goods and services, % of GDP	World Development Indicators Database, World Bank
Voice and Accountability	Voice and Accountability comprises of democracy index, vested interests, accountability of public officials, human rights and freedom of association	Economist Intelligence Unit (EIU)
Fuel Exports	Fuel (mineral fuels, lubricants and related materials) exports, % of merchandise export	World Development Indicators Database, World Bank
Population	Total population based on the de facto definition of population, which counts all residents regardless of legal status or citizenship	World Development Indicators Database, World Bank
Ownership Status	1 = Individual ownership of firm/start-ups ; 0 = Everything else	World Bank's Enterprise Survey (ES), Standardized Set for firms and Global Entrepreneurship Monitor (GEM), Adult Population Survey for start-ups
Sector	1 = Manufacturing sector; 0 = Service sector	World Bank's Enterprise Survey (ES), Standardized Set for firms and Global Entrepreneurship Monitor (GEM), Adult Population Survey for start-ups
Size	Number of employees: 1 = small (less than 20); 2 = medium (20 - 99); 3 = large (100 and above)	World Bank's Enterprise Survey (ES), Standardized Set for firms and Global Entrepreneurship Monitor (GEM), Adult Population Survey for start-ups
Procedures	Number of procedures required to start a business (average of men and women)	Doing Business, World Bank
Time	Number of days required to start a business (average of men and women)	Doing Business, World Bank
Cost	Cost (% of income per capita) to start a business (average of men and women)	Doing Business, World Bank
Capital	Paid-in minimum capital (% of income per capita) to start a business	Doing Business, World Bank
Industry-Level Fixed Effects	Industry dummies representing 2-digit level industry codes according to the ISIC Rev. 4.	World Bank's Enterprise Survey (ES), Standardized Set for firms and Global Entrepreneurship Monitor (GEM), Adult Population Survey for start-ups, Correspondence Table ISIC Rev. 3.1 - ISIC Rev 4. (Eurostat)

Notes: For countries where data for country level variables (like Tax Decentralization, Revenue Decentralization, Government Revenue, Imports, Procedures, Time, Cost and Capital) are missing for the specific year, I use the nearest available year. Firm specific characteristics (Ownership Status, Sector and Size) are constructed based on common characteristics found among nascent entrepreneurs in the GEM dataset and firms observed in the corruption dataset (ES). GEM data report its industry codes as per ISIC Rev 4, while the ES has according to ISIC Rev 3.1. With the help of the correspondence table, ISIC Rev 3.1 industry codes in the ES data are converted to ISIC Rev 4 industry codes for including industry-level fixed effects in Tables 5 and 6. See text for further details.

non-random. In that case, all the firms observed in various corruption-related surveys (like those from the World Bank) are likely to suffer from the sample selection issue. Extant empirical studies on firm-level corruption have overlooked this potential problem. If this concern is valid, all empirical determinants of firm-level corruption to date are likely to be biased. I evaluate this possibility in this study.

To attain my objective, I take a *novel approach* to apply the well-known solution to the sample selection issue: the Heckman (1979) correction procedure. Specifically, I use *two separate samples* – as opposed to the convention – to implement the estimation strategy. It is mainly to circumvent the absence of one common data set containing information on entrepreneurs, non-entrepreneurs, *and* firm-level corruption experience. My results corroborate the likely presence of selection bias in the current context. Upon controlling

Table A.2
Countries and their corresponding year in the sample.

Country name	ISO Alpha-2 codes	Corresponding year
Colombia	CO	2010
Chile	CL	2010
Costa Rica	CR	2010
Peru	PE	2010
China	CN	2012
Russia	RU	2012
Bosnia and Herzegovina	BA	2013
Estonia	EE	2013
Czech Republic	CZ	2013
Croatia	HR	2013
Slovenia	SI	2013
Slovakia	SK	2013
Hungary	HU	2013
Israel	IL	2013
Romania	RO	2013
Poland	PL	2013
Lithuania	LT	2013
Latvia	LV	2013
Uganda	UG	2013
Turkey	TR	2013
Sweden	SE	2014
Indonesia	ID	2015
El Salvador	SV	2016
Thailand	TH	2016

Notes: ISO Alpha-2 Codes refer to the ISO two-letter country codes as described in the ISO 3166 international standard. Sweden has missing data on Informal Payments and therefore, gets dropped from the sample when analysis is run with Informal Payments as the corruption variable. Revenue decentralization data are missing for Croatia, Slovenia, Slovakia and Poland and therefore, get dropped when revenue decentralization is used as a proxy for fiscal decentralization. Slovenia gets dropped due to missing data on (log) Cost when used as a proxy for firm entry regulation. Likewise, Chile, Colombia, Costa Rica, Israel, Peru and Uganda get dropped from the sample when log (Capital) is used as a proxy for firm entry regulation. Refer to Tables 2 and 3 for the total number of clusters in each sub-anlaysis.

for the bias, my preferred 'two-sample Heckman selection estimator' indicates fiscal decentralization to reduce firm-level bribery in general. The economic significance of this impact is also worth noting. These findings are in contradiction to those reported by naive estimation strategies where the sample selection problem is totally ignored.

Although my attempt is the first of its kind and calls for more evidence in the future, I believe it provides a meaningful improvement on the literature concerning firm-level corruption across countries and a state-of-the-art technique for applying Heckman (1979) model under specific data constraints. At the very least, my analysis should draw the attention of empirical researchers to the underlying sample selection problem with these firm-level studies of corruption. It is vital to take into account that empirical analysis in a non-random sample applies to that sub-population only and not the general population. Ignoring such an issue may bias the results and bear deceptive policy implications. My current study aims to make researchers more cautious about this eventuality.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix

See Tables A.1–A.5.

Table A.3
Difference in mean and standardized difference of control variables between two samples.

Variables	Combined N	Difference in mean (Two-sample *t*- Test)	Standardized mean difference (Cohen's *d*)
Country Specific Characteristics			
GDP per Capita (log)	22292	−0.007 (0.382)	−0.013
Government Revenue	22292	−1.549 (0.000)	−0.337
Imports	22292	−2.473 (0.000)	−0.141
Voice and Accountability	22292	−0.091 (0.000)	−0.483
Fuel Exports	22292	9.057 (0.000)	0.355
Population (log)	22292	0.582 (0.000)	0.347
Firm Specific Characteristics			
Ownership Status (dummy)	22261	−0.325 (0.000)	−0.748
Sector (dummy)	21888	0.369 (0.000)	0.788
Size (dummy)	20655	0.488 (0.000)	0.672

Note: The first sample is the firm-level ES data set. The second sample is the individual-level GEM data set. Combined N represents the total number of observations in the first and second samples together. Two-sample t-test performs the test on the equality of means between two unpaired samples. *P*-value of the difference in the mean is reported within parenthesis. The standardized mean difference is defined as the "difference in means or proportions divided by standard error" (Yang and Dalton, 2012, p. 5). Cohen (1988) proposed an effect size index known as Cohen's *d* , where values of 0.2, 0.5, and 0.8 are interpreted as a small, medium, and large effect sizes, respectively (Andrade, 2020). See text for further details.

Table A.4
Impact of fiscal decentralization on corruption: With same number of observations.

Variables	Informal payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−0.729	−1.179	−0.625	−0.736	0.253	−0.547	0.582 †	−1.040	2.681 †	−1.690	0.193
p-value	(0.514)	(0.212)	(0.904)	(0.238)	(0.608)	(0.188)	(0.036)	(0.672)	(0.018)	(0.504)	(0.778)
IMR							−1.754 †	0.553	−5.742 †	1.868	−1.320
p-value							(0.024)	(0.902)	(0.014)	(0.708)	(0.356)
N	10789	10789	10789	10789	10789	10789	10789	10789	10789	10789	10789
Clusters	13	13	13	13	13	13	13	13	13	13	13
Revenue Decentralization	−0.894	−1.558	−0.819	−0.743	−0.135	−0.836 ‡	0.332	−1.267	5.013 †	−1.781	0.047
p-value	(0.248)	(0.218)	(0.834)	(0.124)	(0.844)	(0.086)	(0.342)	(0.636)	(0.014)	(0.366)	(0.972)
IMR							−1.605 ‡	0.568	−9.125 †	1.404	−1.307
p-value							(0.092)	(0.902)	(0.016)	(0.542)	(0.494)
N	10789	10789	10789	10789	10789	10789	10789	10789	10789	10789	10789
Clusters	13	13	13	13	13	13	13	13	13	13	13

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Model with "No ER-No IMR" excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model "With ER" includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model "With IMR" controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with "All" include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. See text for further details.

Table A.5
Impact of fiscal decentralization on corruption: With same number of observations.

Variables	Contract payments										
	No ER-No IMR	With ER					With IMR				
		All	Procedures	Time	Cost	Capital	All	Procedures	Time	Cost	Capital
Tax Decentralization	−2.326	21.753	−2.938	−1.414	−0.760	−2.378	−9.393	−2.582	−11.482	−12.337	−2.503
p-value	(0.496)	(0.264)	(0.532)	(0.548)	(0.864)	(0.506)	(0.294)	(0.748)	(0.404)	(0.340)	(0.622)
IMR							15.850 *	0.505	16.835	18.966	0.396
p-value							(0.004)	(0.986)	(0.354)	(0.494)	(0.978)
N	1806	1806	1806	1806	1806	1806	1806	1806	1806	1806	1806
Clusters	13	13	13	13	13	13	13	13	13	13	13
Revenue Decentralization	−2.494	29.063	−3.256	−2.383	−0.255	−2.493	−11.647 ‡	−1.607	−20.112	−6.892	−3.536
p-value	(0.414)	(0.246)	(0.486)	(0.384)	(0.976)	(0.468)	(0.094)	(0.870)	(0.334)	(0.306)	(0.604)
IMR							17.786 *	−1.547	27.955	7.773	1.941
p-value							(0.004)	(0.954)	(0.338)	(0.506)	(0.860)
N	1806	1806	1806	1806	1806	1806	1806	1806	1806	1806	1806
Clusters	13	13	13	13	13	13	13	13	13	13	13

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Model with "No ER-No IMR" excludes firm entry regulation as a regressor in the structural equation and does not control for sample selection issue. Model "With ER" includes firm entry regulation as a regressor in the structural equation but does not control for sample selection problem. Model "With IMR" controls for sample selection issue via two-sample Heckman selection approach. Here, firm entry regulation is used as the exclusion restriction that identifies the selection equation having nascent entrepreneurship as the dependent variable. IMR is the Inverse Mills Ratio that acts as an additional regressor in the outcome equation after controlling for sample selection bias. P-value is based on wild bootstrap clustered at the country level with 500 repetitions. Columns with "All" include all the three proxies for entry regulation in the model, namely, Procedures, Time, Cost and Capital. Remaining columns include each of these proxies one at a time in the model. See text for further details.

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