Do Corruption Reforms Spur Entrepreneurship?

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Abstract

Does reducing corruption encourage entrepreneurship? I evaluate how sustained improvements in corruption control affect entrepreneurial activity across countries, using two complementary causal designs: the Callaway and Sant'Anna (2021) difference-in-differences estimator and an entropy-balanced stacked difference-in-differences. I define treatment as a sustained five-year improvement in the ICRG corruption index following Bologna Pavlik et al. (2023), and measure entrepreneurship with new firm registrations from the World Bank and early-stage activity from the Global Entrepreneurship Monitor. I find no lasting effect of corruption reform on entrepreneurship. Formal registrations rise immediately after reform, consistent with a "sand the wheels" effect, but the effect loses significance within three years; on the other hand, overall activity (GEM) remains unchanged during this period. This pattern suggests that reforms shift the form of entrepreneurship (informal firms formalize) rather than its level. The results hold under a stricter treatment threshold, a ten-year horizon, reverse treatment, and an extended sample including post-COVID-19 treated countries. Although the existing literature reports both positive and negative associations, identifying the effect from actual reforms reveals no durable causal relationship.

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1. Introduction

Entrepreneurship is often seen as the link between institutions and economic growth (Urbano et al., 2019). It is the mechanism through which the boundaries of production in a society are expanded. It has come to be seen theoretically as an innate part of the market process, in which individuals mainly innovate (Schumpeter, 1983), find and take advantage of opportunities in markets through alertness (Kirzner, 1973), or deal with uncertainty when venturing into an enterprise (Knight, 2005).[1] However, diverging incentive structures can lead to different outcomes, leading entrepreneurship to take many forms, some of which are more productive than others (Baumol, 1990; Henrekson & Sanandaji, 2011), or having different motivations behind starting a business (Global Entrepreneurship Monitor, 2022).

Corruption has been hypothesized to have two essentially different effects on economic outcomes (Méon & Sekkat, 2005): the "sand the wheels" theory, which affirms that corruption increases the costs of a bad set of rules and bureaucracy, and the "grease the wheels" theory, stating that corruption may aid economic activity by compensating for defective governance, making it easier to operate in the market. If corruption is deeply ingrained in the culture, it may even be perceived as a "fact of life," leading individuals to navigate it as part of "business as usual."

Although the majority of the literature has found support for the "sand the wheels" theory for economic growth (Bologna Pavlik et al., 2023; Campos et al., 2016; d'Agostino et al., 2016; Méon & Sekkat, 2005; Mo, 2001; Podobnik et al., 2008), others have found that in some cases corruption may be less detrimental or even positive for growth (Dreher & Gassebner, 2013; Gründler &

[1] All these theories are non-exclusive of each other, for example, an entrepreneur could find an innovation opportunity, while having to deal with uncertainty regarding the potential profit and loss of their venture.

Potrafke, 2019; Saha & Sen, 2021; Vaal & Ebben, 2011).[2] The latter provide support for a "weak form" of the grease the wheels theory, where corruption can be conducive of economic growth in very specific circumstances. When there are bad enough "business climates," corruption might lead to some improvement in economic conditions, as it facilitates going through the red tape and regulations.

This paper studies the effect of sustained improvements in corruption, and how these affect entrepreneurial activity in the years following the jump with a novel causal inference design. Findings suggest that sustained improvements in corruption levels seem to raise business registrations in the immediate years following treatment, while overall entrepreneurial activity remains unhampered. In the following sections, I will (2) outline the mechanisms through which corruption might affect entrepreneurship, (3) provide a review of the literature on the topic, (4) explain my data and method, (5) provide my results and robustness checks, and (6) discuss them.

2. How does corruption affect entrepreneurship?

Given that entrepreneurship serves as a bridge between institutions and economic outcomes, one would expect the mechanisms outlined earlier to also apply to entrepreneurial activity. Therefore, the relationship between corruption and entrepreneurship could be understood through the lenses of the "sand the wheels" and the "grease the wheels" theories.

As with economic growth, the predominant view of the outlined relationship points towards the "sand the wheels" hypothesis. Corruption increases transaction costs, uncertainty, and discourages investment in new ventures (Anokhin & Schulze, 2009); it could be seen as a "tax"

[2] Berggren et al. (2012) study how growth is affected by institutional quality, with an aggregate political risk index from ICRG – which has a component of corruption. They find higher institutional quality enhances growth, while instability reduces it, but this last effect might be positive in rich countries. This is not necessarily evidence for the "sand the wheels" theory, but they use the same data this paper uses to measure corruption.

(Estrin et al., 2013). [3] Some of the mechanisms through which corruption might hurt entrepreneurship are outlined here. First, (1) corruption increases the costs and the barriers to enter a market. Bribes, unofficial fees, and navigating a corrupt system could impose significant costs on entrepreneurs. These costs decrease the potential profits to be made from business opportunities and reduce the attractiveness of entrepreneurship compared to alternative economic activities. Then, (2) corruption could lead to regulatory uncertainty, in which entrepreneurs cannot trust the consistent applicability of laws, regulations, and even property rights. This mechanism makes expected profits much more uncertain, which in turn makes entrepreneurship a much riskier endeavor.

On the other hand, corruption can facilitate entrepreneurship by providing a way to avoid complex regulatory frameworks. When the costs of regulation become unbearable for many entrepreneurs, resorting to corruption (when possible) could result in a smaller overall cost, and higher expected profits. The channels in which corruption could promote entrepreneurship are: (1) time, as entrepreneurs could get permits, licenses, or approvals in a timelier manner (Chowdhury et al., 2015); (2) reducing the burden of regulations (Liu et al., 2019), by bypassing laws or specific rules; and by (3) enabling entrepreneurs to operate informally, functioning outside the margin of the law, making the first two channels unnecessary. These channels are particularly relevant in contexts where regulatory burdens are excessively high, and institutional quality is relatively weak.

3. Literature Review

A body of research investigates the relationship between corruption and entrepreneurship, framed by the two competing theories outlined before. Nevertheless, the evidence is far from settled, and in

[3] Estrin et al. (2013) mention that corruption could even be a "progressive tax," where businesses with higher growth aspirations and bigger scale attract more attention from corrupt officials, as opposed to small-scale entrepreneurs, which can "fly under the radar."

turn points to the complexity of the relationship, with the effect being heavily context-specific, influenced by the type of entrepreneurship being studied, the level of economic development, regional conditions, and regulatory environments. The literature has used both the Global Entrepreneurship Monitor (GEM) data (Reynolds et al., 2002) and new firm data from the World Bank Entrepreneurship Database (WBED) (World Bank, 2026), linking them to corruption indices, such as the Worldwide Governance Indicators (WGI) Control of Corruption (Kaufmann et al., 2010; World Bank, n.d.) measure or the Transparency International's Corruption Perceptions Index (CPI) (Transparency International, 2025).

At a global level, most empirical research tends to support the "sand the wheels" theory. Anokhin and Schulze (2009) use GEM entrepreneurial activity data combined with the Worldwide Governance Indicators (WGI) corruption measure. Their findings indicate that corruption negatively affects overall entrepreneurial activity and innovation, viewing corruption as an obstacle that discourages investment by increasing uncertainty and transaction costs. Similarly, Dutta and Sobel (2016), employing panel data methods across 104 countries, find corruption negatively influences entrepreneurship as measured by WBED, though the deterrent effect becomes smaller (but remains negative) in countries already suffering from poor business climates. Park and Shin (2022), through both OLS and instrumental variables analyses (using legal origins as instruments), reinforce these findings, demonstrating consistently lower entrepreneurial activity in more corrupt environments (using both GEM and WBED). Aidis et al. (2012) support these conclusions to an extent, but interestingly, they find corruption's negative influence loses statistical significance after incorporating country-specific fixed effects, suggesting the critical importance of country-specific contexts and conditions.

As mentioned before, context matters when evaluating how corruption impacts entrepreneurship. One key contextual factor is what type of entrepreneurship is considered.

Audretsch et al. (2022), using GEM data to distinguish between necessity-driven and opportunity-driven entrepreneurship, find that corruption negatively impacts both categories, but the detrimental effect is notably stronger for opportunity-driven entrepreneurs, who likely have greater options for avoiding corrupt environments. Sánchez-Vidal et al. (2024) extend the discussion to corporate entrepreneurship, observing that corruption negatively impacts entrepreneurial initiatives within established firms, particularly in developed economies, whereas developing economies exhibit a quadratic relationship. Finally, Chowdhury et al. (2015), examining international entrepreneurship (entrepreneurs who mainly transact internationally), find that corruption can indeed help entrepreneurs overpass complex regulatory environments, offering some evidence for the "grease the wheels" theory in specific regulatory contexts, which illustrates corruption's nuanced role: facilitating entrepreneurship when regulatory barriers are excessively high.

The level of economic development further complicates this relationship. Avnimelech et al. (2014), utilizing LinkedIn data across 176 countries, argue corruption has a notably stronger negative effect on entrepreneurship in developed economies, potentially because entrepreneurs in these contexts have more viable alternatives to pursuing corrupt markets. However, Motoki et al. (2025) study entrepreneurial intentions in response to major corruption scandals in both developed (U.S.) and developing (Brazil) contexts, finding corruption scandals negatively impact entrepreneurship significantly in the developing context, while having minimal effects in a developed economy.

Examining regional and institutional contexts provides additional insights. At a subnational level, Bologna and Ross (2015) demonstrate in Brazilian municipalities that corruption generally deters business establishment, but it may actually encourage entrepreneurial entry in municipalities with particularly poor institutional quality. Bastos and Bologna Pavlik (2025) also study Brazilian municipalities, finding that corruption tends to lower female entrepreneurial participation. Liu et al. (2019), studying subnational regions in China, report an inverted U-shaped relationship, where low to moderate corruption levels initially encourage entrepreneurial activity, but high corruption ultimately discourages it. These subnational studies show that institutional quality at a more granular level can shape how corruption reforms impact entrepreneurship, suggesting that national-level data may obscure important variation.

Another aspect explored by Estrin et al. (2013) are entrepreneurial growth aspirations, finding that corruption hurts entrepreneurs' growth expectations. While not directly assessing entrepreneurial entry, these findings suggest broader negative impacts on entrepreneurial dynamism and ambitions caused by corrupt practices. Adomako et al. (2021) study realized growth, showing that Ghanaian small and medium-sized enterprises that perceived corruption is associated with increased institutional networking, which in turn makes firm grow more.

Overall, the literature presents a predominantly inverse relationship between levels of corruption and entrepreneurship, but the size and significance of this effect may vary depending on institutional quality, regulatory environments, or development levels. While the "sand the wheels" theory seems to predominate, it is suggested that this negative impact of corruption on entrepreneurship might be mitigated or reversed under specific conditions, such as weak institutionality or diverging business climates. These mixed findings stress the need for a causal approach to understanding this relationship, as most of the studies rely on cross-sectional or panel regressions.

This paper contributes to the existing body of literature by using a causal inference approach, Callaway and Sant'Anna (2021) Difference-in-Differences (CS DiD) and event studies with entropy balancing, to analyze the effect of corruption on entrepreneurship. Rather than relying solely on cross-sectional or correlational analyses, this study aims to establish a more robust understanding of how shifts in corruption levels influence entrepreneurial activity over time. By identifying "treated countries" that have experienced sustained and significant changes in corruption (lowering levels of corruption) and comparing them to "control countries" where corruption has remained relatively stable (or sustained improvements have not happened), this study aims to isolate the direct impact of improvements in corruption on entrepreneurship. This approach allows for a clearer distinction between short-term fluctuations and long-term institutional changes, shedding light on whether reductions in corruption create a more favorable environment for entrepreneurial activity.

By employing this framework, this study provides a fresh perspective on the broader debate regarding corruption and entrepreneurship. Given Bologna Pavlik et al. (2023)'s finding that corruption reform increases economic growth, this study examines one of the possible mechanisms through which this growth comes from. Ultimately, this research aims to expand on correlation-based studies and a more rigorous causal understanding, offering new insights into the complex interplay between corruption and entrepreneurship.

4. Data and Method

Given the staggered adoption of corruption improvements (meaning that sustained reductions in corruption occur at different points in time for different countries) a conventional panel-data methodology, such as a fixed-effects model, would be inadequate. Such traditional approaches assume either simultaneous treatment or static treatment groups, which would incorrectly use previously treated countries as controls for countries treated at later points, potentially causing biased or misleading estimates (Goodman-Bacon, 2021).

To solve this challenge, I employ the Callaway and Sant'Anna (2021) DiD estimator and an entropy-balanced stacked DiD (Cengiz et al., 2019; Hainmueller, 2012), which are specifically designed to handle staggered adoption of treatment.[4] I construct appropriate control groups, and use

[4] Claude Code (Anthropic) was used to assist with writing the Stata and Python code for the empirical analysis, tables, and figures, using Claude Fable 5 (July 2026) and Claude Opus 5 (August

covariates to match controls with treated countries in a way that makes comparison post-treatment identifiable. With these two methods, I am able to estimate an average treatment effect on the treated, as well as dynamic treatment effects (because shocks might be temporary, or take time to have an effect on the outcomes).

For CS DiD, I use the Doubly Robust Inverse Probability Weighted (DRIPW) estimator (Sant'Anna & Zhao, 2020) based on covariates lagged one year before treatment, compute standard errors clustered at the country level, and the control group uses never-treated units. The second method is entropy-balanced stacked DiD, but instead of doing a propensity model, it balances by assigning control weights that make the controls' covariates match those of the treated. For this I also use never-treated countries as controls, and balance, at the year before treatment, on the same set of covariates together with the baseline level of the outcome and its one- and two-year pre-treatment changes, using the full pooled stack, not cohort by cohort. Estimation is done with an event-study regression, entropy-balance assigned weights, two-way fixed effects, and standard errors clustered by country.

For both estimators, I include the following covariates to match on: human capital index, government expenditure (as a % of GDP), exports (as a % of GDP), real GDP per capita, capital stock per capita, and corruption levels; the first five taken from the Penn World Table (Feenstra et al., 2015) and corruption from PRS Group (2026). These covariates are picked based on the existing literature, to ensure that the treated countries are compared to control countries that share similar characteristics. This, in turn, is to improve the pre-treatment fit, as countries with similar characteristics are more likely to have a similar trajectory before treatment happens on the treated. Both estimators rest on the conditional parallel trends assumption, but they use different methods to

2026). I determined the research design and empirical specifications, and reviewed, ran, and verified all code and statistical output.

enforce it. The reason why the two estimators are used at the same time is to give more confidence to the results, showing they are not estimator-specific.

For corruption measurement, this study employs the ICRG corruption index from the PRS Group's International Country Risk Guide (PRS Group, 2026). The index has a scale from 0 to 6, where higher scores mean that countries face lower levels of corruption; it takes into account financial corruption (bribes and side payments) as well as "excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business" (PRS Group, 2022, p. 12). The index is an expert assessment, and ICRG analysts assign it based on pre-set questions, consistently across countries and time.

The definition of treatment, a sustained improvement in corruption, follows Bologna Pavlik et al. (2023). Countries are categorized as treated if they experience a sustained five-year improvement of at least 0.60 points in the ICRG corruption index within the panel window, followed by no reversal exceeding 0.20 points (approximately one third of the increase) in the subsequent five-year period. [5] This criterion ensures that identified reforms reflect genuine institutional changes rather than temporary policy shifts. If a country had more than one reform episode, only the first sustained jump is used as treatment. In Appendix C of the supplementary material I also employ the inverse treatment, to see the effect of sustained corruption deteriorations.

Table 1 lists the countries identified as treated according to these criteria, showing the exact years in which treatment happened; Figure 1 maps treatment status in each panel. These countries represent diverse geographic and institutional contexts, allowing the analysis to explore whether entrepreneurial responses to anti-corruption efforts differ systematically by country context.

[5] The 0.60 rise and 0.20 reversal tolerance follow Bologna Pavlik et al. (2023)'s Appendix B specification. In Appendix A of the supplementary material I use their main specification (a 0.80 rise and a 0.25 reversal tolerance) and find no meaningful difference in the results. A similar design has been used in Bologna Pavlik and Young (2022), but using the control of corruption measure from WGI to study the relationship between corruption and aid.

Table 1. Treated countries and treatment years. Main definition (0.60/0.20), with treatment year in parentheses. † marks countries excluded from the Callaway–Sant'Anna estimation sample and ‡ those excluded from the entropy-balanced one, for lack of covariate or outcome data in the base year. All other countries listed enter both.

WBED (24 treated)	GEM (20 treated)
Albania (2011)	China (2007)
Bangladesh (2011)‡	France (2007)
France (2011)	Germany (2007)
Ghana (2011)	India (2007)
Indonesia (2011)	Indonesia (2007)‡
Japan (2011)	Chile (2008)
Lebanon (2011)†‡	Belgium (2009)
Namibia (2011)	Canada (2009)
Papua New Guinea (2011)†‡	Ireland (2009)
United Arab Emirates (2011)	Korea, Rep. (2009)
Gabon (2012)	Japan (2010)
Kenya (2012)†‡	Lebanon (2010)†‡
Tunisia (2012)	United Arab Emirates (2010)
Uruguay (2012)	Uruguay (2012)
Zimbabwe (2012)	Estonia (2015)
Paraguay (2014)‡	Saudi Arabia (2015)
Estonia (2015)	United Kingdom (2015)
Ireland (2015)	Croatia (2017)
Saudi Arabia (2015)	Morocco (2017)
United Kingdom (2015)	Qatar (2017)
Bulgaria (2016)	
Croatia (2017)	
Morocco (2017)	
Qatar (2017)	

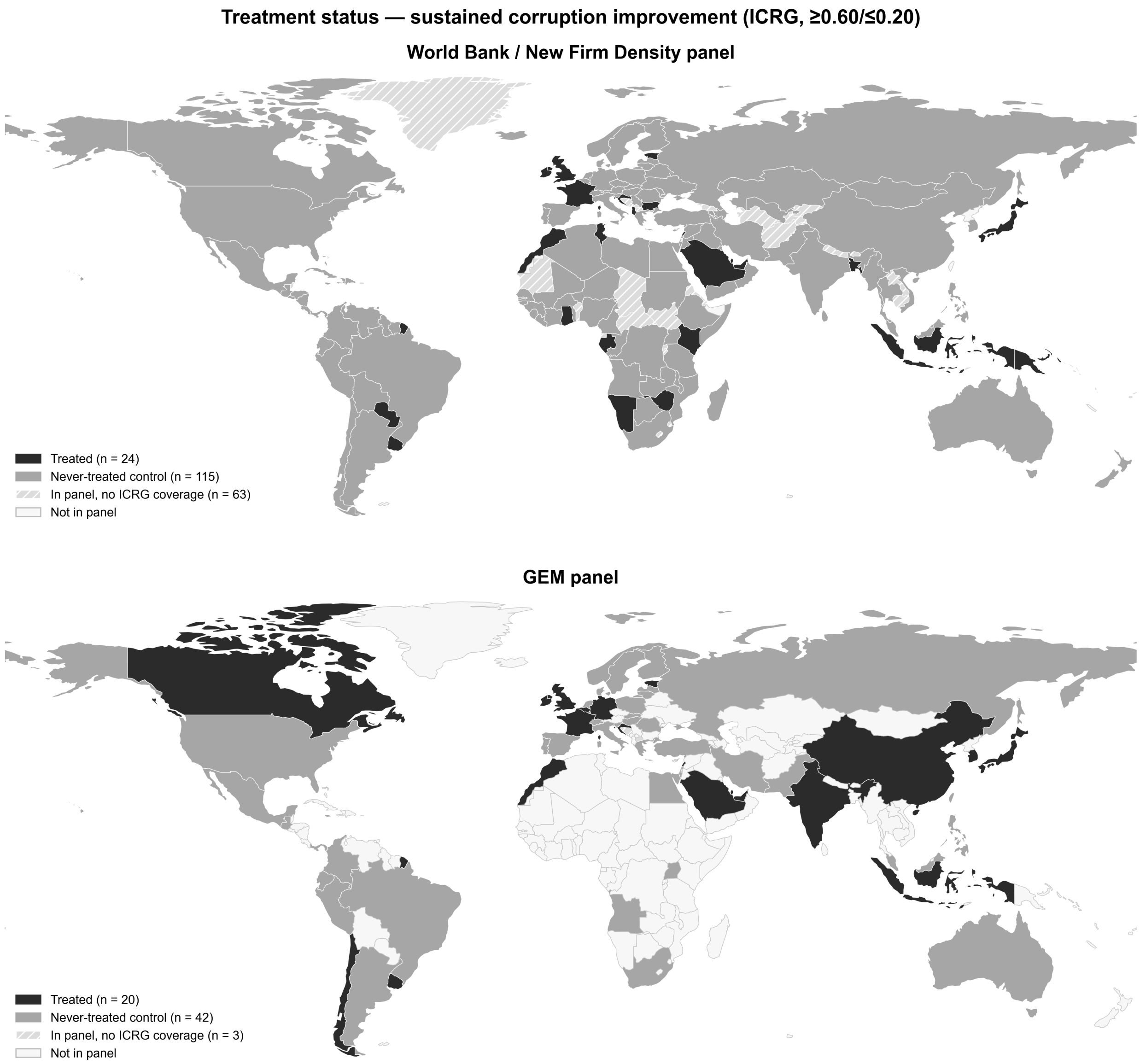

Figure 1. Treatment status under the main definition (0.60/0.20). Top: WBED / new firm density panel (24 treated); bottom: GEM panel (20 treated). Black = treated; mid-grey = never-treated control with ICRG coverage; hatched = in panel but no ICRG; white = not in panel

As for the outcome variables, two major sources are used: the World Bank Entrepreneurship Database, and the Global Entrepreneurship Monitor database. Both are explained in the following subsections.

4.1 World Bank Entrepreneurship Database

The first dataset consists of annual observations from 174 countries, covering the period from 2006 to 2022. The first measure of entrepreneurship is taken from the World Bank's Entrepreneurship Database (Meunier et al., 2026; World Bank, 2026). This is the number of new firms registered per 1,000 working-age people, a standard proxy for new formal entrepreneurship (understood as a firm registered with the national business registry). Studies have used this measure to study entrepreneurship (Dutta & Sobel, 2016; Park & Shin, 2022), as it provides a consistent, objective way of measuring business activity in countries.[6] In this panel, there are 24 treated countries. Table 2 shows the summary statistics for the panel using this measure as outcome.

Table 2. Summary statistics: WBED analysis panel (treated and never-treated). Each variable is summarized over its non-missing range (Period column); the outcome and ICRG corruption run to 2022, while the Penn World Table covariates end in 2019 (since the last treated cohort is in 2017, this period works). Corruption is the ICRG level (0–6, higher = less corrupt).

Variable	Obs	Mean	Std. Dev.	Min	Max	Period
New firm density (per 1,000)	2,566	5.00	21.51	0.01	377.04	2006–2022
ICRG corruption (0–6)	2,348	2.65	1.18	0.00	6.00	2006–2022
Human capital index	1,960	2.57	0.69	1.13	4.35	2006–2019
Government expenditure (% GDP)	2,338	0.18	0.07	0.02	0.59	2006–2019
Exports (% GDP)	2,338	0.30	0.29	0.00	2.65	2006–2019
GDP per capita (USD)	2,338	20,928	22,049	251	166,520	2006–2019
Capital stock per capita	2,324	86,894	95,073	1,107	565,254	2006–2019

4.2 Global Entrepreneurship Monitor

The second dataset consists of annual observations from a reduced pool of 65 countries, covering the period from 2002 to 2023. The main variable of interest in this dataset is Total Early-Stage Entrepreneurial Activity (TEA), which is defined by GEM as "Percentage of 18-64 population who are either a nascent entrepreneur or owner-manager of a new business" (Global Entrepreneurship

[6] Appendix D of the supplementary material extends the WBED panel through 2024 as a robustness check. The reason this extension is put in a robustness check is because all post-treatment periods in the treated countries added with the extension are after COVID-19.

Monitor, n.d.). Other variables analyzed are Necessity-Driven TEA and Opportunity-Driven TEA, to see if different types of entrepreneurship change in response to corruption reform. All these variables are taken from the Adult Population Survey from GEM, which is a questionnaire that collects data on the respondents' activities, attitudes and aspirations. Given that some countries have missing observations for specific years, linear interpolation was applied to complete the panel dataset for empirical analysis. The opportunity and necessity data end in 2018, so they have a reduced time period compared to TEA. In this panel, there are 20 treated countries. Table 3 shows the summary statistics for the second panel, using GEM data as outcomes.

Table 3. Summary statistics: GEM analysis panel (treated and never-treated). Each variable is summarized over its non-missing range (Period column); TEA and ICRG corruption run to 2023, the opportunity/necessity components to 2018, and the Penn World Table covariates to 2019 (since the last treated cohort is in 2017, this period works). Corruption is the ICRG level (0–6).

Variable	Obs	Mean	Std. Dev.	Min	Max	Period
Total early-stage activity (%)	1,137	11.29	7.19	1.48	49.60	2002–2023
Opportunity-driven TEA	846	6.67	4.62	0.09	28.72	2002–2018
Necessity-driven TEA	846	4.30	3.93	0.15	29.57	2002–2018
Opportunity/necessity ratio	846	3.19	3.35	0.03	27.41	2002–2018
ICRG corruption (0–6)	1,355	3.16	1.21	1.00	6.00	2002–2023
Human capital index	1,080	2.90	0.58	1.32	4.35	2002–2019
Government expenditure (% GDP)	1,134	0.18	0.06	0.04	0.32	2002–2019
Exports (% GDP)	1,134	0.36	0.29	0.01	1.80	2002–2019
GDP per capita (USD)	1,134	30,074	23,892	1,299	166,520	2002–2019
Capital stock per capita	1,134	135,704	104,519	1,647	641,968	2002–2019

GEM data has been used in an extensive body of work to study entrepreneurial activity across countries. Bosma (2013) reviews 89 academic publications that have been published in SSCI-listed journals since 2004, emphasizing the impact of the data on scholarly research. In contrast to registration data, TEA also considers entrepreneurial activity in its earliest stages and includes entrepreneurs that could be in the informal sector (operating without registering their firms).

5. Results

5.1 World Bank Entrepreneurship Database

Table 4 presents the estimated Average Treatment Effects on the Treated (ATT) for new business creation, measured as the number of new firms registered per 1,000 people. The overall ATT is positive but statistically insignificant under both estimators: 0.44 (SE 0.33) under CS DiD and 0.60 (SE 0.43) under entropy balancing. These findings suggest that, on average, sustained improvements in corruption are not associated with significant sustained changes in new business formation across all treated countries.

Table 4. Average treatment effects on the treated: overall and by period. Main definition (0.60/0.20). The treatment rule identifies 24 treated countries in the WBED panel and 20 in the GEM panel. Estimation samples are smaller where covariates or outcome history are unavailable—WBED 21 (CS DiD) and 19 (entropy balance), GEM 19 and 18.

	Overall	**Years since treatment**					
Outcome / estimator	**ATT**	*t*=0	*t*=1	*t*=2	*t*=3	*t*=4	*t*=5
New firm density (WBED)							
CS DiD	0.44	0.39***	0.57**	0.70**	0.45	0.65	1.16*
	(0.33)	(0.13)	(0.25)	(0.34)	(0.47)	(0.61)	(0.69)
Entropy balance	0.60 (0.43)	0.30** (0.14)	0.59** (0.29)	0.72 (0.45)	0.56 (0.58)	0.62 (0.63)	0.83 (0.64)
Total early-stage activity (GEM)							
CS DiD	0.29	0.27	0.32	1.36*	1.31	1.00	1.22
	(1.04)	(0.78)	(0.88)	(0.80)	(1.14)	(1.37)	(1.28)
Entropy balance	−0.10 (0.86)	−0.63 (0.51)	−0.27 (0.72)	0.46 (0.97)	0.27 (1.18)	0.48 (1.15)	−0.90 (1.20)
Opportunity-driven TEA (GEM)							
CS DiD	0.04	0.04	0.83	0.70	1.36	1.37	−0.24
	(0.90)	(0.77)	(0.70)	(0.81)	(0.99)	(1.33)	(1.20)
Entropy balance	−0.18 (0.89)	0.19 (0.62)	−0.26 (0.87)	−0.58 (1.05)	0.51 (1.18)	0.00 (1.43)	−0.96 (1.42)
Necessity-driven TEA (GEM)							
CS DiD	−0.74	0.49	−0.25	−0.15	−1.51	−1.42	−0.06
	(1.05)	(0.76)	(1.01)	(1.08)	(1.29)	(1.37)	(1.35)
Entropy balance	−0.12 (0.73)	−0.46 (0.45)	−0.21 (0.99)	0.37 (1.23)	−0.01 (0.80)	−0.21 (0.97)	−0.18 (1.12)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses. CS DiD period effects are the Callaway–Sant'Anna event-study coefficients (errors clustered by country), entropy-balance period effects are the stacked event-study lag coefficients with standard errors clustered by country. Pre-treatment coefficients are reported in the event-study figures.

The new-firm panel of Figure 2 displays the dynamic treatment effects for the full sample of treated countries over time. In this case, the pre-treatment coefficients fluctuate around zero and remain statistically insignificant, showing no evidence against the parallel trends assumption. This implies that, before the sustained improvements in corruption, the treated countries did not exhibit systematically different trends in firm creation compared to the control group.

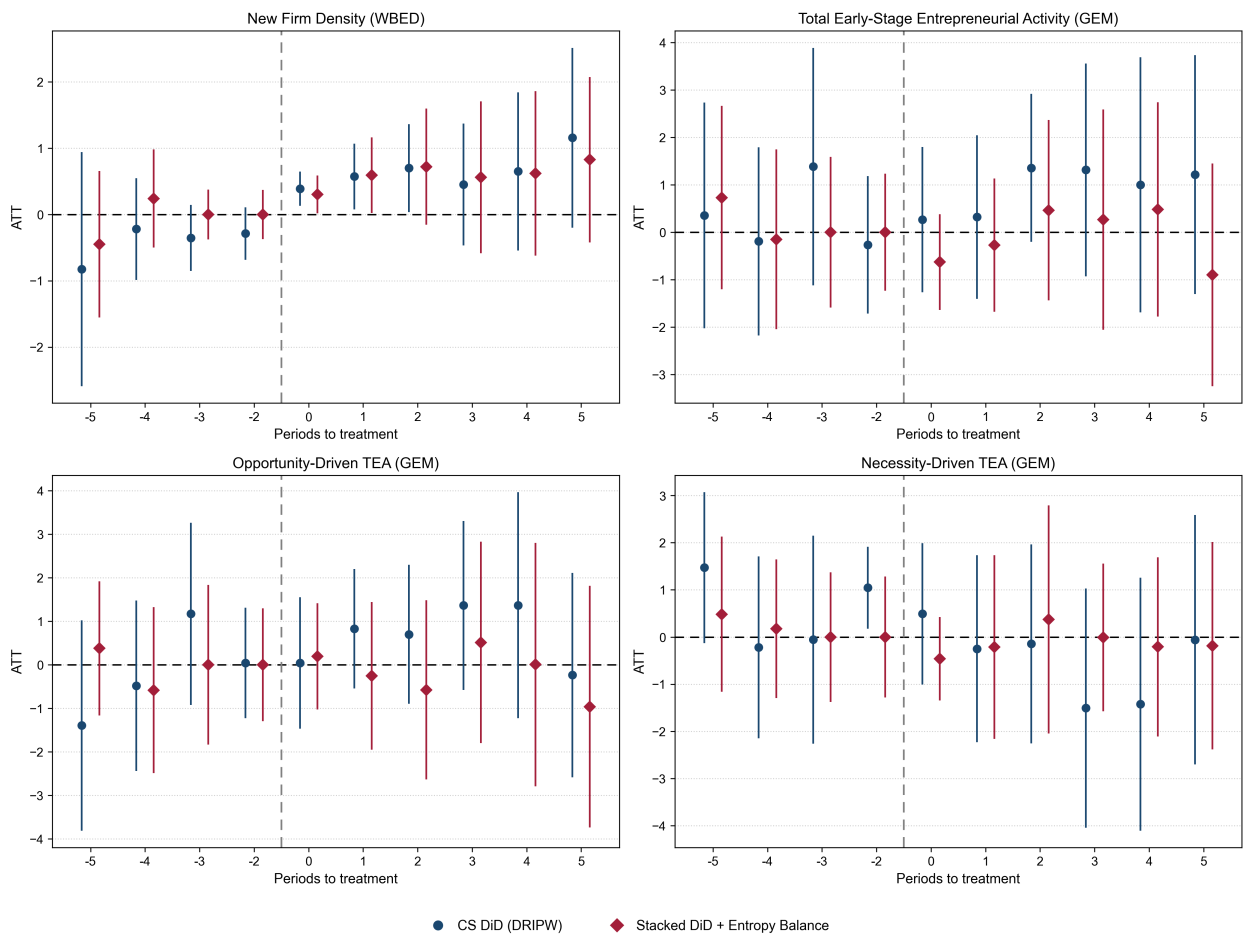

Figure 2. Corruption reform and entrepreneurship: event-study estimates. CS DiD (circles) and entropy-balanced stacked DiD (diamonds); bars are 95% confidence intervals; the omitted period is the year before treatment

Post-treatment, the figure shows an upward trend in the rate of new firm formation. The event study shows varied significance of effects (at the 1% to 5% level) for periods 0 to 2 under CS DiD and periods 0 to 1 under entropy balancing. These results would lend some evidence to the "sand the wheels" theory, where corruption was hurting business creation (making it costlier to start a business, or register it formally) and improving control of corruption makes the creation of new businesses easier in the short run. However, the effect ceases to be significant after the second year (year 5 is significant in CS DiD, but not in entropy balancing), indicating that while at first corruption reform does favor the creation of new businesses, in the medium term and long term there is no significant effect.

One possible explanation is that lowering the cost of doing business following corruption reforms prompts a bigger number of individuals to start a business, because it is now easier to carry out business than before. However, this effect fades away in the long run. The World Bank data captures only formally registered businesses, so another explanation is that businesses that were operating previously in the informal realm now register due to the lower barrier. This creates a temporary effect, where some of the businesses register, but it doesn't last because the backlog (non-registered businesses) clears up after a couple years.

5.2 Global Entrepreneurship Monitor

Table 4 presents the estimated Average Treatment Effects on the Treated (ATT) for Total Early-Stage Entrepreneurial Activity (TEA), Opportunity-Driven TEA, and Necessity-Driven TEA using CS DiD and entropy balancing.

The ATT estimates, aggregated across all treated countries and treatment years, are statistically insignificant for all the studied variables. These results indicate no consistent average effect of sustained improvements in corruption on entrepreneurial activity across all treated countries. A corruption reform would not affect overall entrepreneurship, opportunity-driven entrepreneurship, and necessity-driven entrepreneurship.

Figure 2 shows the event-study estimates, capturing the dynamic evolution of the outcome variables before and after sustained corruption improvements. Pre-treatment coefficients consistently hover around zero and are statistically insignificant (besides one year in the Necessity Entrepreneurship CS DiD estimates). In general, results do not show any significant effects in the post-treatment period. This essentially means that corruption reform does not affect entrepreneurial activity (and its subcomponents) as measured by GEM.

These findings, although not statistically significant, could provide some information as to what is going on with entrepreneurship following a corruption reform. Since GEM also takes into account informal types of entrepreneurship, by contrasting these results with the business register results, an interesting insight can be drawn: Corruption reforms do not directly affect entrepreneurial activity, but there is a transitional effect where more entrepreneurs are registering their firms because of the lower cost to registering. Then, previously informal entrepreneurs formalize, and the rate to which new firms are registered returns to the former levels.

5.3 Robustness and Extra Outcomes

In the appendices, there are many more results estimated to check whether the conclusions reached in the previous analysis hold. First, in Appendix A of the supplementary material, I used the same treatment threshold that Bologna Pavlik et al. (2023) use in their main specification. Table S1 shows the treated countries under this threshold, Table S2 the results, and Figure S1 the event studies. The results are unchanged: there is a small upbeat in firms registered in the first years after reform, but it fades away in the following years, and the entrepreneurial activity outcomes gain significance in only one of the estimators. This indicates that the results do not depend on treatment being "more relaxed" than Bologna Pavlik et al. (2023)'s main specification. Then, I also expand the post-treatment period for estimation in Appendix B of the supplementary material, reaching +9 and +10 for each specification. Table S3 shows the results, and Figure S2 the event studies, indicating there is no significant effect after the original +5 period.

To test the opposite treatment (whether a deterioration in corruption affects entrepreneurial activity), I also estimate results for the flip-side treatment in Appendix C of the supplementary material: a sustained decrease in the corruption index (with the same threshold—0.60/0.20). This sample, compared to the main specification, has fewer treated countries, ranging from 4 to 7 across both panels and estimators, so the estimates should not be interpreted as robust evidence of the effect of corruption deteriorations: the sample is too small to support inference. Results are shown in Table S4 and Figure S3, and are generally insignificant, with some isolated significant years that only show up in one of the two methods utilized.

In Appendix D of the supplementary material, I added an extended panel that would incorporate treated countries whose post-treatment period would be almost exclusively after COVID-19. Results are shown in Table S5 and Figure S4. Again, the results do not change significantly.

Since the GEM series has missing values which were linearly interpolated, in Appendix E of the supplementary material I estimated the effect of corruption reform on all three GEM outcomes without any linear interpolation. Due to missing values, some treated countries are lost, with CS DiD retaining 17 out of 20 countries (since it can handle unbalanced panels), and entropy balancing retaining only 10, because of missing pre-trend values. In these estimations, shown in Table S6 and Figure S5, the result remains on being a null, so interpolation was not "hiding" an effect in this case.

Finally, I estimate the effect of reforms on an extra set of six entrepreneurial activity-related outcomes from GEM in Appendix F of the supplementary material: perceived opportunities, fear of failure, entrepreneurial intentions, growth expectations, established business ownership, and the opportunity/necessity ratio. The results are reported in Table S7 and Figure S6. Across all six the ATT is generally not significant, and the one ATT that does reach significance appears under only one of the two estimators. This is relevant because previous literature had linked levels of corruption to some of these: Estrin et al. (2013) with growth expectations, Motoki et al. (2025) with entrepreneurial intentions, and Audretsch et al. (2022) with the opportunity/necessity ratio. Once these variables are studied under a framework of sustained corruption change, the effect ceases to be significant—except for year five in fear of failure, which is significant and positive in both specifications.

6. Conclusions

This study investigates how sustained improvements in levels of corruption affect entrepreneurial activity across countries, using a causal inference framework applied to the registering of new businesses (World Bank Entrepreneurship Database) and early-stage entrepreneurial activity (Global Entrepreneurship Monitor).

The main findings show that, on average, corruption reforms do not lead to statistically significant changes in business registering, but dynamic estimates reveal a short-term rise in formal firm registrations following anti-corruption reforms. This supports the "sand the wheels" perspective in the short run: when corruption inhibits business, removing it may temporarily increase business entry, at least in the formal sector (or in terms of registration).

On the other hand, GEM data offers a deeper layer of insight. Even though the average treatment effects across all GEM metrics (TEA, opportunity-driven, and necessity-driven entrepreneurship) are statistically insignificant, they reveal a key distinction: while more businesses may be formally registered in the immediate aftermath of reform, overall entrepreneurial activity (including informal entrepreneurship) remains stable. This suggests that entrepreneurs might have been operating informally before the period of corruption reform. It indicates that anti-corruption reforms shift the form of entrepreneurship (formal registration) rather than its overall level.

Thus, this study provides a complex view: corruption reforms do not kill or foster entrepreneurship, but they reshape it. These findings imply that while corruption reforms are unlikely to boost entrepreneurship, they do not suppress it either. The previous literature had found evidence supporting both the sand-the-wheels and grease-the-wheels theories, but this evidence came largely from cross-sections and standard regression estimation. Identifying the effect of sustained corruption reform shows it has no lasting effect on the level of entrepreneurship, suggesting earlier evidence may reflect a correlation and not a causal relationship.

In relation to Bologna Pavlik et al. (2023), the economic growth benefits of anti-corruption reforms likely do not come from increased entry of firms in the long run, but from shifts in the quality or productivity of existing entrepreneurial efforts. As entrepreneurs face less corruption, de facto regulatory costs might decrease, which might be followed by expansion and economies of scale, factors beyond the sheer number of new firms or entrepreneurs.

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Do Corruption Reforms Spur Entrepreneurship?

September 2026

Supplementary Material

This supplement accompanies the article Do Corruption Reforms Spur Entrepreneurship? Tables are numbered S1 onwards and figures S1 onwards.

Appendix A. Stricter Treatment Definition

Table S1. Treated countries under the stricter definition (0.80/0.25). Treatment year in parentheses; 18 treated per panel. Estimation samples are smaller: WBED 15 (CS DiD) and 14 (entropy balance), GEM 15 and 16. Marking as in Table 1: † excluded from the Callaway–Sant'Anna sample, ‡ from the entropy-balanced one.

WBED (18 treated)	GEM (18 treated)
Albania (2011)	China (2007)
Bangladesh (2011)‡	France (2007)
France (2011)	Germany (2007)
Ghana (2011)	India (2007)
Japan (2011)	Indonesia (2007)‡
Lebanon (2011)†‡	Chile (2008)
Namibia (2011)	Belgium (2009)
Papua New Guinea (2011)†‡	Canada (2009)
United Arab Emirates (2011)	Lebanon (2010)†‡
Gabon (2012)	United Arab Emirates (2010)†
Kenya (2012)†‡	Japan (2011)
Uruguay (2012)	Uruguay (2012)
Zimbabwe (2012)	Estonia (2015)
Estonia (2015)	Ireland (2015)
Ireland (2015)	United Kingdom (2015)
United Kingdom (2015)	Qatar (2017)†
Bulgaria (2017)	Croatia (2018)
Qatar (2017)	Saudi Arabia (2018)

Table S2. Average treatment effects: stricter definition (0.80/0.25). Overall and by period; 18 treated per panel, of which WBED 15 (CS DiD) and 14 (entropy balance), and GEM 15 and 16, enter estimation.

	Overall	**Years since treatment**					
Outcome / estimator	**ATT**	***t*=0**	***t*=1**	***t*=2**	***t*=3**	***t*=4**	***t*=5**
New firm density (WBED)							
CS DiD	0.44 (0.42)	0.30* (0.17)	0.44 (0.30)	0.61 (0.48)	0.64 (0.64)	1.05 (0.81)	1.19 (0.82)
Entropy balance	0.74 (0.58)	0.35* (0.18)	0.59 (0.36)	0.77 (0.59)	0.75 (0.76)	0.87 (0.84)	1.12 (0.85)
Total early-stage activity (GEM)							
CS DiD	0.26 (1.35)	0.18 (0.74)	1.10 (0.84)	1.82* (0.95)	2.15* (1.24)	2.62* (1.44)	1.80 (1.49)
Entropy balance	0.83 (0.96)	−0.29 (0.54)	0.28 (0.79)	1.12 (1.03)	1.40 (1.29)	1.77 (1.25)	0.72 (1.44)
Opportunity-driven TEA (GEM)							
CS DiD	0.52 (0.98)	1.01 (0.90)	0.16 (0.88)	0.79 (1.17)	1.57 (1.20)	1.93 (1.71)	1.42 (1.37)
Entropy balance	−0.19 (1.03)	0.45 (0.80)	−0.49 (1.04)	−0.95 (1.23)	0.31 (1.26)	0.07 (1.64)	−0.51 (1.60)
Necessity-driven TEA (GEM)							
CS DiD	−0.76 (1.34)	0.03 (0.75)	0.20 (1.08)	0.20 (1.21)	−1.27 (1.36)	−0.76 (1.67)	−1.12 (1.76)
Entropy balance	0.57 (0.83)	−0.61 (0.47)	0.64 (1.27)	1.53 (1.40)	0.97 (0.71)	0.74 (1.09)	0.15 (1.28)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses. CS DiD period effects are the Callaway–Sant'Anna event-study coefficients (errors clustered by country) and are identical to the plotted figures; entropy-balance period effects are the stacked event-study lag coefficients with standard errors clustered by country. Pre-treatment coefficients are reported in the event-study figures.

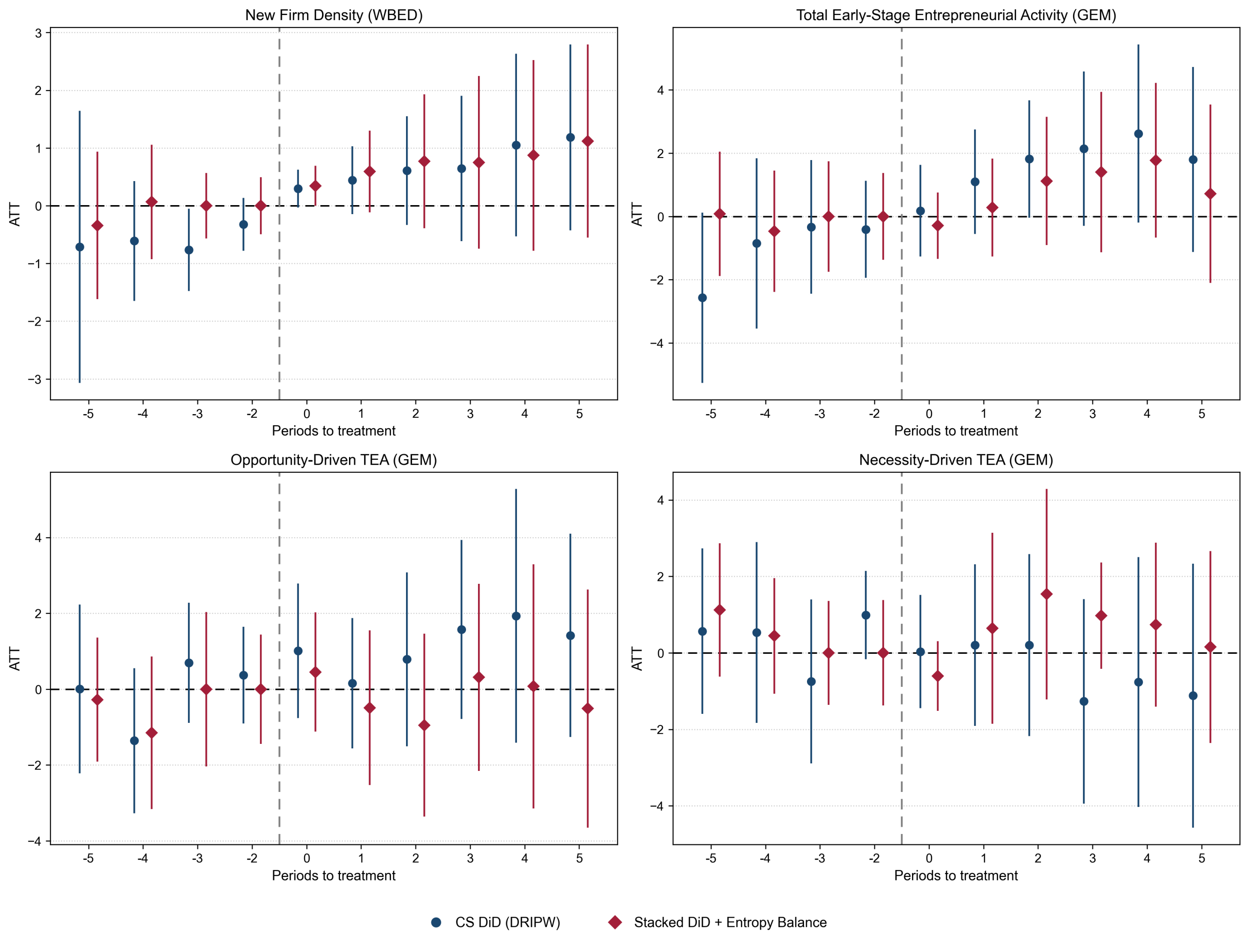

Figure S1. Stricter treatment definition (0.80/0.25). Four main outcomes, both estimators; 18 treated countries per panel, of which 15 (CS DiD) and 14 (entropy balance) enter the WBED estimates and 15 and 16 the GEM estimates. Bars are 95% confidence intervals

Appendix B. Ten-Year Horizon

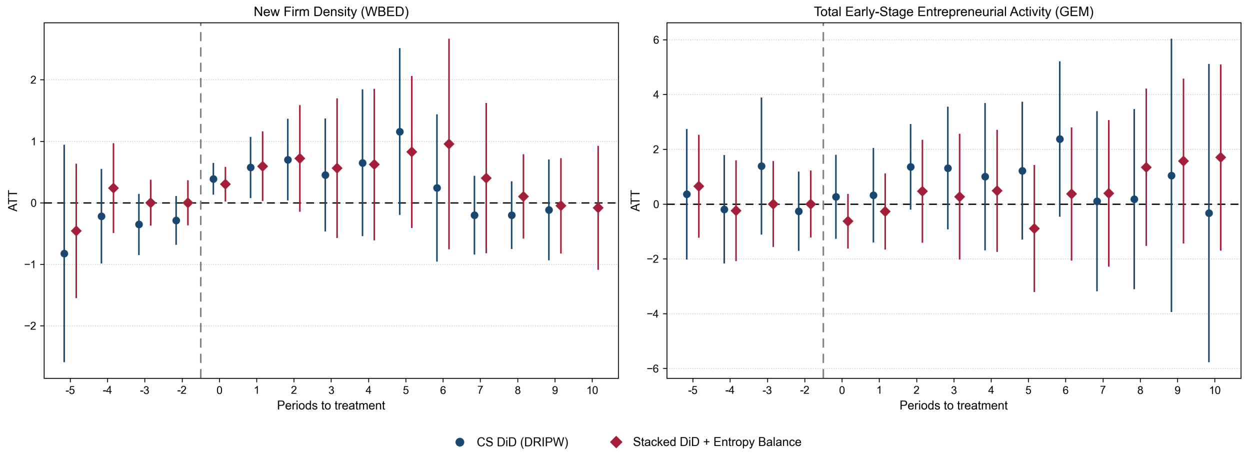

Figure S2. Ten-year horizon: new firm density and TEA. CS DiD is identified through year +9 for WBED and +10 for total early-stage activity and the entropy-balanced design through +10 for both. Of the 24 treated countries in the WBED panel, 21 (CS DiD) and 19 (entropy balance) enter estimation; of the 20 in the GEM panel, 19 and 18. Bars are 95% confidence intervals

Table S3. Average treatment effects: ten-year horizon. Main definition (0.60/0.20); overall and by period. The Callaway–Sant'Anna design identifies new firm density through t=9 and total early-stage activity through t=10; the entropy-balanced design reaches t=10 for both outcomes. Estimation samples match the main specification: WBED 21 (CS DiD) and 19 (entropy balance) of 24 treated, GEM 19 and 18 of 20.

	Overall	**Years since treatment**										
Outcome / estimator	**ATT**	***t*=0**	***t*=1**	***t*=2**	***t*=3**	***t*=4**	***t*=5**	***t*=6**	***t*=7**	***t*=8**	***t*=9**	***t*=10**
New firm density (WBED)												
CS DiD	0.36	0.39***	0.57**	0.70**	0.45	0.65	1.16*	0.24	−0.20	−0.20	−0.12	—
	(0.31)	(0.13)	(0.25)	(0.34)	(0.47)	(0.61)	(0.69)	(0.61)	(0.33)	(0.28)	(0.42)	
Entropy balance	0.45	0.30**	0.59**	0.72	0.56	0.62	0.83	0.96	0.40	0.10	−0.05	−0.08
	(0.43)	(0.14)	(0.29)	(0.44)	(0.58)	(0.63)	(0.63)	(0.87)	(0.62)	(0.35)	(0.39)	(0.51)
Total early-stage activity (GEM)												
CS DiD	0.80	0.27	0.32	1.36*	1.31	1.00	1.22	2.37	0.10	0.18	1.04	−0.33
	(1.03)	(0.78)	(0.88)	(0.80)	(1.14)	(1.37)	(1.28)	(1.45)	(1.68)	(1.68)	(2.54)	(2.78)
Entropy balance	0.43	−0.63	−0.27	0.46	0.27	0.48	−0.90	0.37	0.39	1.34	1.57	1.70
	(0.93)	(0.51)	(0.71)	(0.96)	(1.17)	(1.14)	(1.18)	(1.24)	(1.36)	(1.47)	(1.54)	(1.73)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses; — denotes a period the design does not identify. Period effects are the Callaway–Sant'Anna event-study coefficients over the window [-5,10] and the stacked entropy-balanced event-study lag coefficients (standard errors clustered by country); both are identical to the plotted figures. The overall column reports the Callaway–Sant'Anna post-treatment average; the corresponding entropy-balanced average over lags 0–10 is not reported.

Appendix C. Corruption Deteriorations (Reverse Treatment)

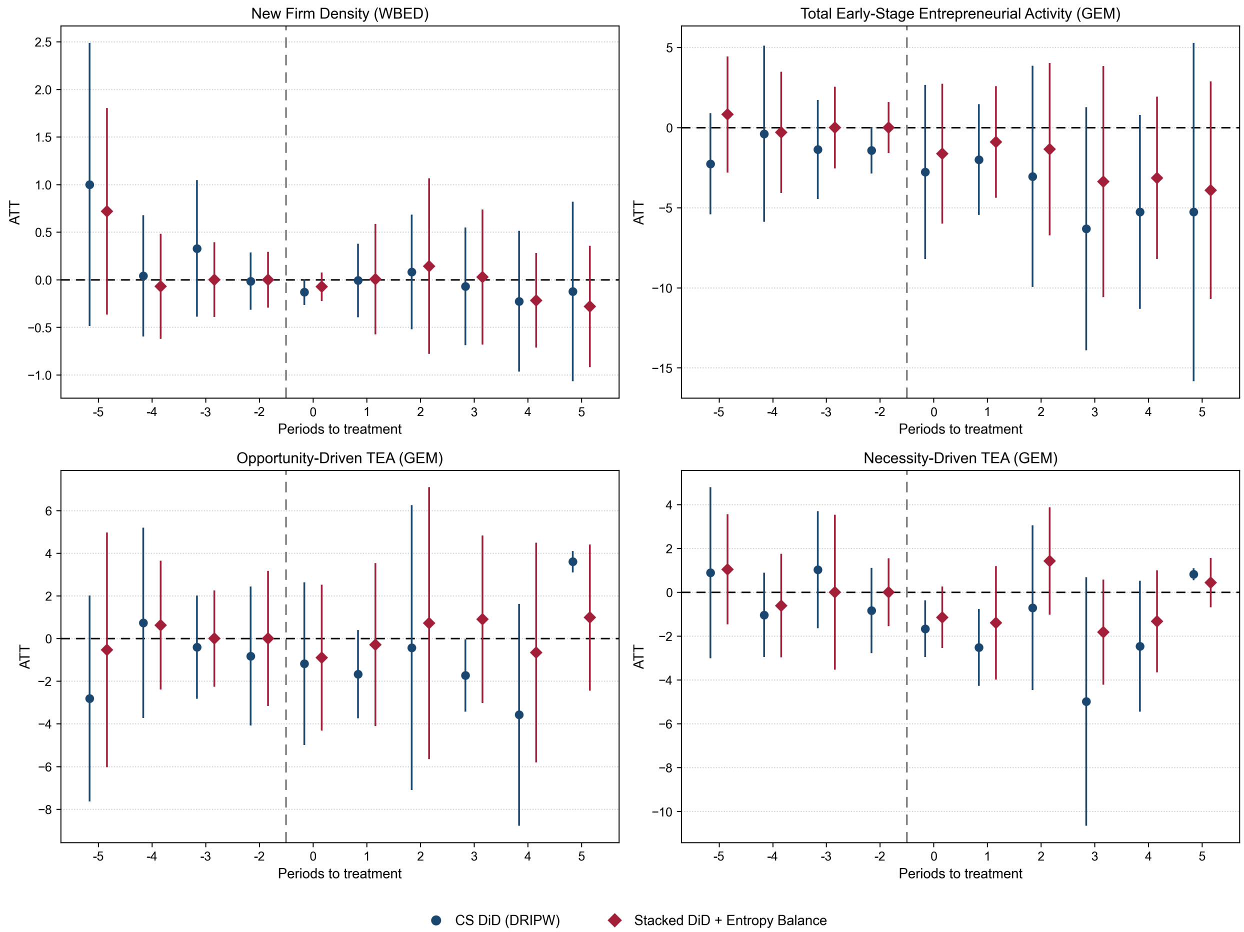

Figure S3. Corruption deteriorations: event-study estimates. Reverse treatment; both estimators. Of the 14 treated countries in the WBED panel, only 7 (CS DiD) and 6 (entropy balance) have the registration data the estimates require; the GEM panels rest on 5 treated countries, of which 4 and 5 enter. Bars are 95% confidence intervals

Table S4. Average treatment effects: corruption deteriorations (reverse treatment). Overall and by period. The treatment rule identifies 14 treated countries in the WBED panel and 5 in the GEM panel, but registration data are missing for most deteriorating countries: only 7 (CS DiD) and 6 (entropy balance) enter the WBED estimates, and 4 and 5 the GEM estimates.

	Overall	**Years since treatment**					
Outcome / estimator	**ATT**	***t*=0**	***t*=1**	***t*=2**	***t*=3**	***t*=4**	***t*=5**
New firm density (WBED)							
CS DiD	−0.02	−0.13*	−0.01	0.08	−0.07	−0.23	−0.12
	(0.20)	(0.07)	(0.20)	(0.31)	(0.32)	(0.38)	(0.48)
Entropy	−0.07	−0.07	0.01	0.14	0.03	−0.22	−0.28
balance	(0.23)	(0.08)	(0.30)	(0.47)	(0.36)	(0.25)	(0.32)
Total early-stage activity (GEM)							
CS DiD	−1.57	−2.77	−2.00	−3.05	−6.32	−5.27*	−5.27
	(4.09)	(2.77)	(1.77)	(3.52)	(3.87)	(3.09)	(5.39)
Entropy	−2.38	−1.63	−0.90	−1.34	−3.38	−3.14	−3.91
balance	(2.53)	(2.22)	(1.78)	(2.74)	(3.68)	(2.59)	(3.46)
Opportunity-driven TEA (GEM)							
CS DiD	0.01	−1.18	−1.67	−0.43	−1.73**	−3.58	3.60***
	(1.43)	(1.94)	(1.06)	(3.41)	(0.87)	(2.65)	(0.25)
Entropy	0.13	−0.89	−0.29	0.72	0.90	−0.66	0.98
balance	(1.86)	(1.74)	(1.95)	(3.25)	(2.00)	(2.63)	(1.75)
Necessity-driven TEA (GEM)							
CS DiD	−1.33	−1.67**	−2.52***	−0.71	−4.98*	−2.46	0.83***
	(1.16)	(0.66)	(0.89)	(1.92)	(2.89)	(1.52)	(0.14)
Entropy	−0.64	−1.15	−1.40	1.43	−1.82	−1.33	0.44
balance	(0.60)	(0.72)	(1.32)	(1.25)	(1.22)	(1.19)	(0.57)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors in parentheses. CS DiD period effects are the Callaway–Sant'Anna event-study coefficients (errors clustered by country) and are identical to the plotted figures; entropy-balance period effects are the stacked event-study lag coefficients with standard errors clustered by country. Pre-treatment coefficients are reported in the event-study figures.

Appendix D. Extended panels

Table S5. Average treatment effects: extended panels (WBED to 2024, GEM to 2025). Overall and by period; 28 treated in the WBED panel and 21 in the GEM panel, with estimation samples of WBED 23 (CS DiD) and 22 (entropy balance), and GEM 19 under both estimators.

Outcome / estimator	Overall	Years since treatment					
	ATT	*t*=0	*t*=1	*t*=2	*t*=3	*t*=4	*t*=5
New firm density (WBED)							
CS DiD	0.47	0.28**	0.42*	0.70**	0.53	0.72	1.35*
	(0.33)	(0.12)	(0.24)	(0.34)	(0.47)	(0.61)	(0.73)
Entropy balance	0.61	0.31**	0.60**	0.67*	0.56	0.69	0.85
	(0.37)	(0.13)	(0.26)	(0.39)	(0.50)	(0.55)	(0.56)
Total early-stage activity (GEM)							
CS DiD	0.50	0.23	0.33	1.16	1.56	0.97	1.75
	(1.03)	(0.70)	(0.83)	(0.99)	(1.16)	(1.45)	(1.42)
Entropy balance	0.39	−0.15	0.46	0.72	0.47	0.68	0.14
	(0.84)	(0.55)	(0.83)	(0.96)	(1.17)	(1.14)	(1.25)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses. CS DiD period effects are the Callaway–Sant'Anna event-study coefficients (errors clustered by country) and are identical to the plotted figures; entropy-balance period effects are the stacked event-study lag coefficients with standard errors clustered by country. Pre-treatment coefficients are reported in the event-study figures.

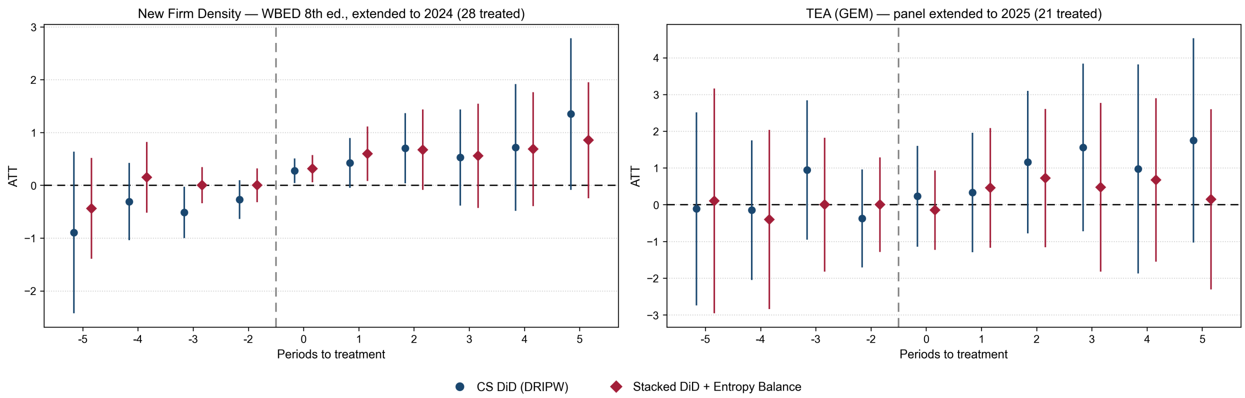

Figure S4. Extended-panel event studies. New firm density (WBED, to 2024; 28 treated, of which 23 CS DiD / 22 entropy balance enter estimation) and
total early-stage activity (GEM, to 2025; 21 treated, 19 entering under both), both estimators; bars are 95% confidence intervals

Appendix E. GEM Outcomes Without Interpolation

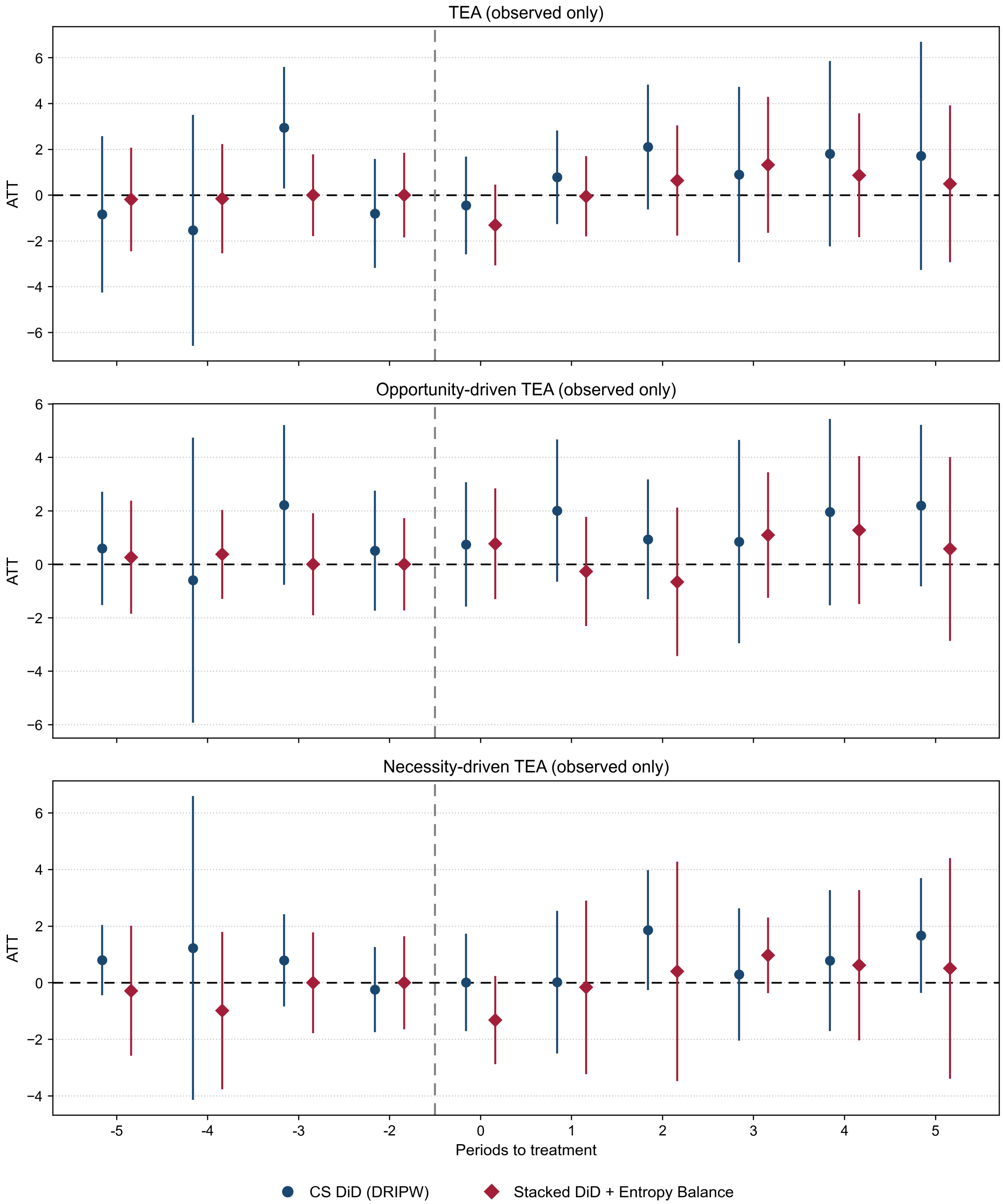

Figure S5. GEM outcomes estimated on observed values only, without interpolation. Both estimators; the omitted period is the year before treatment; bars are 95% confidence intervals. CS DiD retains 17 of the 20 treated countries.

Table S6. Average treatment effects: GEM outcomes without interpolation. Main definition (0.60/0.20); overall and by period. Estimated on observed values only; CS DiD retains 17 of 20 treated countries and the entropy-balanced design 10 of 20.

	Overall	**Years since treatment**					
Outcome / estimator	**ATT**	***t*=0**	***t*=1**	***t*=2**	***t*=3**	***t*=4**	***t*=5**
Total early-stage activity (GEM)							
CS DiD	0.25	−0.45	0.78	2.10	0.90	1.81	1.72
	(1.58)	(1.09)	(1.04)	(1.39)	(1.96)	(2.07)	(2.54)
Entropy balance	0.33	−1.30	−0.05	0.64	1.32	0.87	0.49
	(1.14)	(0.90)	(0.90)	(1.23)	(1.51)	(1.38)	(1.75)
Opportunity-driven TEA (GEM)							
CS DiD	0.79	0.75	2.01	0.94	0.85	1.96	2.20
	(0.94)	(1.19)	(1.36)	(1.14)	(1.94)	(1.78)	(1.54)
Entropy balance	0.47	0.77	−0.27	−0.66	1.10	1.28	0.58
	(0.74)	(1.06)	(1.04)	(1.42)	(1.20)	(1.41)	(1.75)
Necessity-driven TEA (GEM)							
CS DiD	−0.09	0.01	0.02	1.86*	0.29	0.78	1.67
	(1.09)	(0.88)	(1.28)	(1.08)	(1.19)	(1.27)	(1.03)
Entropy balance	0.17	−1.31	−0.16	0.40	0.97	0.62	0.51
	(1.01)	(0.79)	(1.56)	(1.98)	(0.68)	(1.35)	(1.99)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors in parentheses. Outcomes are the raw GEM series (tea, teaopp, teanec) rather than the interpolated ones used in the main text; balance pre-trends and entropy weights are rebuilt on the raw series. All other specification choices are those of Table 4. No overall effect is significant at the 10% level under either estimator.

Appendix F. Additional Entrepreneurial Outcomes (GEM)

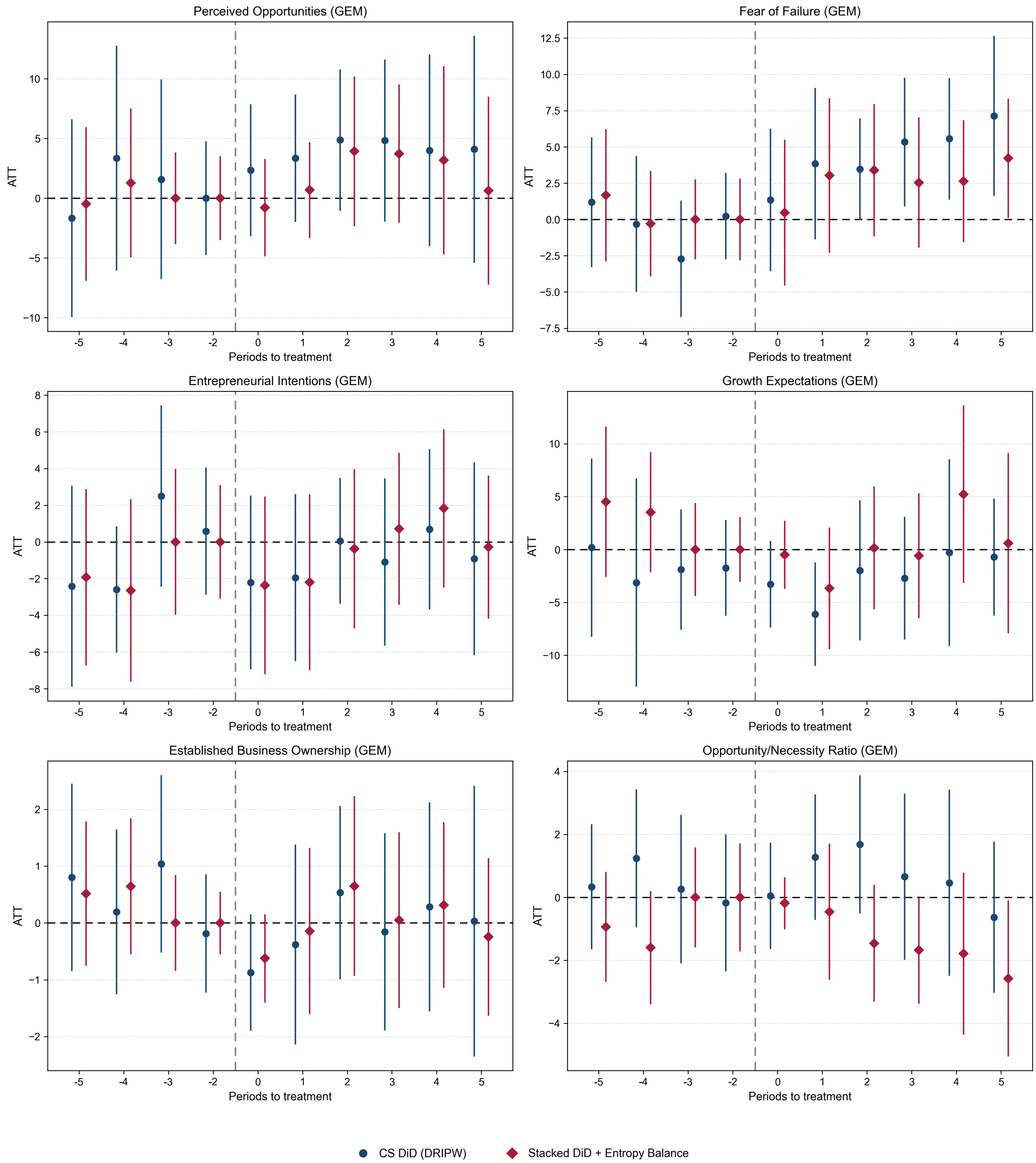

Figure S6. Additional GEM outcomes: event-study estimates. Both estimators, main treatment definition; bars are 95% confidence intervals; series are linearly interpolated. Of the 20 treated countries, 18–19 (CS DiD) and 17–18 (entropy balance) enter each estimate, depending on outcome availability

Table S7. Average treatment effects: additional GEM outcomes. Main definition (0.60/0.20); overall and by period. Series are linearly interpolated. Of the 20 treated countries, 18–19 (CS DiD) and 17–18 (entropy balance) enter each estimate, depending on outcome availability.

	Overall	**Years since treatment**					
Outcome / estimator	**ATT**	t=0	t=1	t=2	t=3	t=4	t=5
Perceived opportunities							
CS DiD	3.74	2.35	3.35	4.87	4.84	4.01	4.10
	(2.51)	(2.82)	(2.73)	(3.02)	(3.47)	(4.09)	(4.85)
Entropy balance	1.90	−0.79	0.69	3.94	3.73	3.18	0.63
	(2.36)	(2.08)	(2.05)	(3.20)	(2.96)	(4.02)	(4.01)
Fear of failure							
CS DiD	4.39**	1.35	3.84	3.46*	5.33**	5.56***	7.14**
	(2.05)	(2.50)	(2.66)	(1.78)	(2.26)	(2.14)	(2.82)
Entropy balance	2.71	0.47	3.03	3.39	2.54	2.64	4.22**
	(1.98)	(2.57)	(2.72)	(2.33)	(2.29)	(2.15)	(2.10)
Entrepreneurial intentions							
CS DiD	−0.98	−2.21	−1.95	0.06	−1.10	0.69	−0.92
	(2.06)	(2.42)	(2.33)	(1.75)	(2.33)	(2.23)	(2.68)
Entropy balance	−0.45	−2.37	−2.20	−0.38	0.71	1.83	−0.28
	(1.81)	(2.47)	(2.45)	(2.22)	(2.11)	(2.20)	(1.99)
Growth expectations							
CS DiD	−2.78	−3.28	−6.12**	−1.97	−2.71	−0.29	−0.72
	(2.92)	(2.08)	(2.50)	(3.38)	(2.96)	(4.50)	(2.82)
Entropy balance	0.20	−0.50	−3.67	0.15	−0.59	5.22	0.59
	(2.54)	(1.64)	(2.94)	(2.96)	(3.02)	(4.28)	(4.34)
Established business ownership							
CS DiD	−0.88	−0.87*	−0.38	0.53	−0.16	0.28	0.03
	(0.86)	(0.52)	(0.90)	(0.78)	(0.88)	(0.94)	(1.21)
Entropy balance	0.00	−0.62	−0.14	0.65	0.05	0.31	−0.24
	(0.60)	(0.40)	(0.75)	(0.81)	(0.79)	(0.74)	(0.71)
Opportunity/necessity ratio							
CS DiD	0.80	0.05	1.27	1.68	0.66	0.46	−0.63
	(0.81)	(0.86)	(1.02)	(1.12)	(1.34)	(1.51)	(1.22)
Entropy balance	−1.36	−0.19	−0.46	−1.46	−1.67*	−1.78	−2.57**
	(0.84)	(0.42)	(1.10)	(0.94)	(0.87)	(1.31)	(1.26)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses. CS DiD period effects are the Callaway–Sant'Anna event-study coefficients (errors clustered by country) and are identical to the plotted figures; entropy-balance period effects are the stacked event-study lag coefficients with standard errors clustered by country. Pre-treatment coefficients are reported in the event-study figures.