

Assessing the Relationship Between Entrepreneurship Reform and Institutional Accountability in Uganda

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ABSTRACT

Entrepreneurship reforms are usually evaluated by their effect on firm entry, employment, or growth, but they can also reshape the public institutions that administer them. Because the design is proposed rather than executed, the paper does not report estimates; it instead specifies the treatment window, comparison groups, outcome indicators, and identifying assumptions. The argument is that a multi-period difference-in-differences approach, with district and year fixed effects and careful attention to staggered rollout, can distinguish an accountability effect from the mechanical effect of more registered firms. The principal threats to identification—anticipatory behaviour, spillovers across districts, differential pre-trends, and policy contamination—are analysed and addressed through placebo tests, synthetic control diagnostics, and heterogeneity analysis.

Keywords: *entrepreneurship reform, institutional accountability, difference-in-differences, Uganda, business registration, policy evaluation*

The Institutional Problem That the Design Is Meant to Solve

Entrepreneurship Reform Is Usually Discussed As A Policy For Growth

Entrepreneurship reform is usually discussed as a policy for growth, but its most important institutional consequence may be administrative. A registration reform makes it easier for a citizen to open a firm; it also changes whether the official who makes that decision can be held answerable for how the decision was made. The two effects are not the same.

The first is a market-entry effect; the second is an institutional-accountability effect. Most evaluations of entrepreneurship reform have measured the first and left the second unexamined. Accountability is not a sideshow to institutional analysis.

Accountability Is Not A Sideshow To Institutional Analysis

The quality of a state's bureaucracy affects how well rules are enforced, and how enforceable rules are affects the credibility of property rights (North, 1990; Williamson, 1985). When the formal economy is open to those with connections, the growth effects of reform remain narrow (De Soto, 2000; Acemoglu & Robinson, 2012). The institutional capacity of the state, in turn, determines whether the formal economy is a force for development or rent-seeking (Acemoglu et al., 2001).

The difficulty is not that institutional accountability matters, but that it is rarely specified precisely enough to be measured. Without a definition, a policy can be described as strengthening institutions without anyone knowing what changed. the present analysis therefore takes up a narrower problem: how can a reform aimed at reducing entry barriers be assigned a causal effect on the accountability of the institutions that administer entry?

Ugandan Entrepreneurship Reform as a Quasi-Natural Experiment

Business Registration And Licensing Reform Is A Credible Treatment For

Business registration and licensing reform is a credible treatment for the design because it directly changes the administrative process by which businesses become legally visible. Cross-country evidence has established that entry regulation affects the number of firms, not just the difficulty of opening them (Djankov et al., 2002; Klapper et al., 2006). In countries where entry requires many procedures, there are fewer registered businesses; where entry is easier, there are more (Bruhn, 2008; Kaplan et al., 2011).

This literature provides the baseline for treating registration reform as a policy event with real consequences. Uganda is a plausible setting for such a design because its regulatory reform has been uneven across local administrative units. A national reform at a single date would leave no comparison group in a simple difference-in-differences design; a reform that affects some districts earlier than others creates the variation that the estimator needs.

Uganda Is A Plausible Setting For Such A Design Because

The World Bank's business regulation indicators have documented the varying costs of licensing across economies, and Uganda's performance is part of that record (World Bank, 2020). The present analysis does not claim that the necessary variation has already been assembled. It claims that if such variation exists, it can be exploited in a coherent natural-experimental design. The treatment should be defined as the adoption of a substantive registration or licensing reform in a district or municipality: a change that reduces the number of steps, shortens the processing window, creates a one-stop shop, or removes an official's discretion to reject an application without written reasons.

A Reform's Institutional Face: The Concept of Institutional Accountability

Institutional Accountability Must Be Separated From Vague Governance Terms

Institutional accountability must be separated from vague governance terms. In the proposed design it refers to the extent to which administrative officials can be held answerable for the discretion they exercise when processing a business registration, licence, or permit. This involves three measurable dimensions: transparency of the decision rule, procedural consistency in applying it, and the availability of recourse when the rule is broken.

Transparency can be observed in published checklists, fees, timelines, and the reasons given for refusal. Procedural consistency can be observed in the distribution of registry outcomes across applications and officials, at least before and after reform. Recourse can be observed in whether rejected applicants can appeal, how often they do, and whether the appeal changes the outcome.

Transparency Can Be Observed In Published Checklists Fees Timelines And

The three dimensions together form a governance construct that is narrower than rule of law and more specific than institutional quality (Mungiu-Pippidi, 2015). Because registration reform changes the decision-maker's discretion, it can affect all three dimensions in a direct way. If an official can no longer choose informally whether to process an application, the decision rule becomes more transparent.

If a one-stop shop standardises the paperwork, procedural consistency may increase. If a reform introduces an appeal mechanism, recourse becomes available where it was absent. These are the channels through which a market-entry policy becomes an institutional policy.

A Difference-in-Differences Design for Staggered Reform

The baseline specification

Difference-in-differences identifies a treatment effect by comparing the change in an outcome for a treated group with the change for an untreated group, under the assumption that the two groups would have followed the same trend absent treatment (Ashenfelter & Card, 1985; Angrist & Pischke, 2009). For district d and year t , the baseline specification is:

$$Y_{dt} = \alpha_d + \lambda_t + \tau D_{dt} + X'_{dt}\gamma + \varepsilon_{dt} \quad (1)$$

Here the outcome Y is an institutional-accountability index, district fixed effects control for stable administrative differences, year fixed effects control for common shocks, D is an indicator for whether district d has adopted the reform by year t , and X is a vector of pre-treatment covariates. With a single reform date and a clean never-treated control group, the coefficient would recover an average treatment effect. In practice, administrative reform in Ugandan districts is unlikely to be a single event, so the design must allow for staggered adoption.

If treatment effects are homogeneous across cohorts and over time, a two-way fixed-effects estimator with district and year dummies remains consistent. Recent econometric work has shown that this is not generally true: two-way fixed effects can produce a weighted average in which some cohorts are used as controls for others, and negative weights may appear when effects vary (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020). The safer approach is to define a group-time average treatment effect for each adoption cohort and each post-reform period, then aggregate the cohort-specific effects (Callaway & Sant'Anna, 2021).

Staggered timing and heterogeneous effects

The proposed design therefore does not rely on a single two-way fixed-effects coefficient. Let g denote the first year in which a district is treated. The group-time average treatment effect is:

$$ATT(g, t) = E[Y_t(g) - Y_t(0) | G_g = 1] \quad (2)$$

The estimator forms this by comparing treated districts in cohort g with a never-treated comparison group while allowing for path-specific effects. Because institutional accountability may change slowly, the design should estimate effects for several periods after reform rather than summarise them in one number. Standard errors should be clustered at the district level, since treatment is assigned at district level and the outcome is likely to be serially correlated (Bertrand et al., 2004).

Threats to Identification and the Limits of the Design

The Central Identifying Assumption Is Conditional Parallel Trends In The

The central identifying assumption is conditional parallel trends: in the absence of reform, districts exposed and not exposed would have followed similar paths after accounting for district and year fixed effects and baseline covariates. This assumption cannot be tested directly. The design can make it more plausible by matching districts on pre-reform characteristics, such as

baseline formal-sector share, corruption perceptions, administrative capacity, and economic structure (Rosenbaum & Rubin, 1983; Heckman et al., 1997).

Matching does not prove unconfoundedness, but it improves the plausibility of comparison group similarity (Imbens & Wooldridge, 2009). Anticipatory behaviour is a specific threat. If district officials know that reform is coming, they might already change procedures in the year before treatment.

An event-study specification with leads identifies such anticipation: the interaction of treatment with a dummy variable for one or two years before adoption would be expected to be small if the design is valid. Placebo tests using fake adoption dates before the true reform can detect this pattern as well. Spillover effects from treated to untreated districts are a second threat.

A district that does not adopt reform might still improve its registration procedures because officials learn from neighbouring districts or because firms change their registration location. The design can address this by estimating the effect with spatial weights, excluding neighbouring districts from the comparison group, or using an inverse probability weighted regression that models the proximity of reform (Rosenbaum & Rubin, 1983; Heckman et al., 1997). Selective adoption is the most serious institutional threat.

Anticipatory Behaviour Is A Specific Threat

If districts with better accountability infrastructure adopt reform earlier, the treatment is correlated with the outcome before treatment begins. The design can reduce this concern by using an event-study specification, by matching on pre-reform administrative capacity, and by testing whether the timing of adoption predicts pre-reform levels of the outcome. If capacity causes both early adoption and an apparently better outcome, the design could not resolve the bidirectionality without stronger assumptions.

Policy contamination is a further threat. Business registration reform rarely occurs alone. A district may simultaneously create a one-stop shop, update licensing fees, and train registry staff; all these components are part of the treatment, but so are unobserved changes.

If a separate accountability initiative is introduced in the same districts, the design cannot distinguish it. The protocol therefore requires identifying the specific reform under study and excluding districts with documented contemporaneous interventions. Where the number of treated districts is small, synthetic control benchmarks, constructed from a weighted combination of never-treated districts, can provide a useful complement to the difference-in-differences comparison.

Mechanisms and What the Design Would Distinguish

Suppose The Design Is Executed

Suppose the design is executed. A positive coefficient for the accountability outcome would not by itself establish which mechanism was at work. Three mechanisms are plausible.

The first is supply-side: reform reduces the discretion of officials by codifying steps and producing a written record. The second is demand-side: reform increases formalisation, and new firms then demand transparent rules and working appeal routes. The third is selection: reform is adopted in districts where accountability is already improving.

The design can distinguish the first two mechanisms. If the effect is supply-side, the accountability outcome should improve in treated districts even when formal-sector growth is

modest. If the effect is demand-side, the improvement should be concentrated in districts with larger increases in registered firms.

The design can test this by splitting the sample by formalisation intensity or by interacting reform treatment with baseline formal-sector share. The third mechanism remains a threat unless the study design addresses it. Entrepreneurs are not passive recipients of administrative rules.

The Second Is Demand-Side Reform Increases Formalisation And New Firms

Their decision to formalise depends on the benefits of formal status, including legal protection and access to finance, as well as on the expected quality of the state (Banerjee & Duflo, 2011). If formalisation increases only because it is cheaper, the state may not become more accountable. The proposed design treats formalisation and institutional accountability as separate outcomes rather than combining them into a single composite index.

This distinction matters for policy. A reform that removes procedures without adding a recourse mechanism may make entry easier while increasing the exposure of new firms to arbitrary official decisions. A reform that also creates a written record of decisions, publishable timelines, and an appeal route has a better chance of reducing discretion and building trust.

The proposed design is intended to reveal which of these paths a given reform takes, not to assume that any reduction in entry barriers is an institutional improvement.

Conclusion: Causal Inference and Institutional Change in East Africa

The Argument Of The Present Analysis Is Not That Entrepreneurship

The argument of the present analysis is not that entrepreneurship reform in Uganda has improved institutional accountability. The available evidence does not permit that claim. The argument is that the question can be made answerable by a difference-in-differences design that treats reform as a staggered administrative intervention and accountability as a measured institutional outcome.

Such a design preserves the insight of entrepreneurship policy scholarship while redirecting its outcome. Existing studies have shown that entry regulation matters for firm creation (Djankov et al., 2002; Bruhn, 2008; Kaplan et al., 2011). The proposed design builds on that tradition.

Such A Design Preserves The Insight Of Entrepreneurship Policy Scholarship

Instead of asking whether reform produces more firms, it asks whether reform changes the way institutions treat firms. This is a more restrictive question, and one that cannot be answered by merely observing the growth of the formal sector. The conclusion is therefore a bounded methodological proposition.

Institutional accountability should not be treated as a by-product of entrepreneurship reform; it should be treated as an outcome with its own mechanisms. If this design were implemented with real administrative data from Ugandan districts, the group-time difference-in-differences estimator and the associated diagnostics could identify whether, when, and for which administrative functions reform alters accountability. That knowledge would be valuable for East Africa and for the broader comparative study of how regulatory reform reshapes state capacity.

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